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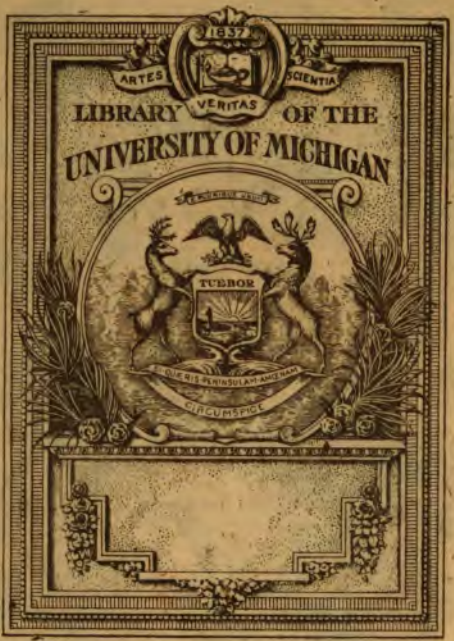
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THE CHEMIST;

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INTRODUCTION

TO THE SIXTH VOLUME.

NUMEROUS as the laborers in the Science of Chemistry have become, little, if anything, of considerable importance has been done during the past year. Indeed, we may safely affirm, that this year, of the whole period during which we have conducted this journal, has been the least productive of valuable additions to our knowledge in any department of Chemistry. This fact, however, is by no means matter of surprise, when we reflect on the circumstance that the rich mine has been zealously and industriously worked, to such an extent, indeed, as to leave much less to be done. But we think that Chemists have taken too much for granted; they have given an almost blind acquiescence to theories, in some instances, little better than dogmas, because, to verify or contradict them, involved much laborious and patient investigation, and because they had received the sanction, or had even emanated from, authority more brilliant than profound—more dazzling than luminous. They have, too, labored in a given direction; they have not troubled themselves much with ascertaining the correctness of that direction. Liebig's theories have been assumed to be correct; most experiments in organic chemistry have been based on that assumption. It has also been taken for granted, that the analyses of organic substances at present published are correct. Has this correctness been sufficiently demonstrated? Of the correctness of the bases on which many of these analyses are founded, we are extremely doubtful; a doubt shared by many esteemed and eminent scientific friends. Here is matter for labor; a direction in which the indus-

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trious investigator may pursue his researches with honor to himself, and benefit to science.

Unfortunately, many who have the genius lack the means of giving it full sway; innumerable ideas— sometimes, doubtless, of inestimable value, lie entombed in the brains of men who cannot elaborate them, from a deficiency of pecuniary means, and are thus lost to the scientific world. This defect must always exist; but its extent may be diminished by the means of investigation being afforded to them by an institution, whose laboratory and funds should be available to all who seek its aid, provided, that the objects for which they seek them be proved, to the satisfaction of competent and unbiassed judges, legitimately entitled to them. Such an Institution do we hope to find in the Royal College of Chemistry recently established; we know not whether this will be one of its contemplated objects, but we hope that it is, or may become so.

It is not our intention in this introduction to pursue the plan adopted in the preceding ones, of giving a kind of synopsis of the contents of the volume. We must, therefore, conclude, by promising a continuance of our best endeavors on behalf of our readers in every respect; and we have every reason to believe that the volume for the year 1846, will be found to excel its predecessors. We now, then, take the sixth anniversary leave of our readers, wishing them all the enjoyments of the approaching festive season, and that the commencement of the ensuing year may be an auspicious one for all of us.

THE CHEMIST.

I. CHEMISTRY.

ON BERTHIER'S PROPOSED EMPLOYMENT OF SULPHUROUS ACID IN CHEMICAL ANALYSIS.*

BY DR. H. BÖTTINGER.

BERTHIER communicated a series of experiments in his treatise "On the Use of Sulphurous Acid and Alkaline Sulphites in Analysis," which seemed to be of great importance to chemical analysis: this eminent analyst thought that sulphurous acid and sulphite of ammonia would constitute an excellent means for the separation of several metals, especially tin and antimony, to separate which quantitatively a more sure and simple way than that hitherto used was much wanted.

Berthier did not quantitatively examine the methods of separation proposed by himself; at least, in his treatise, he mentions no experiments made for that purpose: from the behavior of some oxides with sulphurous acid and alkaline sulphites, he decides on the use of the latter as a means for quantitative analysis, and he merely indicates the way of effecting the separation.

It having appeared, from experiments instituted in the laboratory of Giessen, that several of those methods of separation are liable to difficulties, the removal of which was desirable previous to entering into an accurate and careful examination of Berthier's assertions; I undertook this labor at the request of Dr. Will, and in communicating its results I embrace the opportunity of thanking him for his kind directions and assistance. As will appear, in the sequel, the results which I obtained are not throughout in accordance with Berthier's statements.

BEHAVIOR OF ALUMINA.

I found Berthier's assertions to be perfectly confirmed. Solutions of alumina in mineral acids are not rendered turbid, either by cold or by boiling sulphurous acid. From a solution containing sulphurous acid, when cold, ammonia separates only one portion of any alumina present; the filtered mass, however, on being boiled with hydrochloric acid until the sulphurous acid is removed, yields a large precipitate on being treated by ammonia.

Solutions of alumina are not rendered turbid by sulphite of ammonia containing free sulphurous acid, without the aid of heat; on boiling, however, the alumina is perfectly separated. Sulphite of ammonia, in the absence of any free acid, precipitates solutions of alumina even without heat. Humid hydrate of alumina readily dissolves in sulphurous acid; on applying heat, the alumina is again precipitated.

If the precipitate obtained by sulphurous acid, or by sulphite of ammonia, be allowed to stand with the supernatant liquid in the open air, a portion is redissolved as sulphate of alumina; but, when carefully kept from the air no alumina is redissolved.

We possess analyses of the combinations of alumina with sulphurous acid, made by Gougginsperg,* lately corroborated by Muspratt,† and, moreover, others made by Fourcroy and Vauquelin.‡

The solution of alumina in sulphurous acid, heated to 165°F. (74° C.), throws down a salt presenting the formula SO^3, Al^2O^3, 4HO (Gougginsperg). The precipitates thrown down from salts of alumina by sulphite of ammonia, by boiling for a long time, are, according to Berthier, pure hydrates; however, even by boiling a very long time, I could not obtain them free from sulphurous acid.

BEHAVIOR OF BERYL EARTH.

The beryl earth which was employed in the following experiments was extracted by myself from beryl; it was separated from the

* Liebig's *Annalen der Chemie und Pharmacie*, September, 1844.

* Liebig's *Annalen*, vol. xlv., p. 132.

† *Ibid.*, vol. l., p. 277.

‡ Gmelin's *Handbuch*, 4th ed., vol. ii., p. 298.

alumina by carbonate of ammonia, and examined before the blowpipe with a solution of cobalt. The hydrate is rather readily soluble in sulphurous acid, and by boiling for a long time nothing is thrown down from the solution.

A solution of beryl earth, in sulphurous or hydrochloric acid, being boiled for a long time, was partially precipitated by sulphite of ammonia, presenting a white mass of the appearance of cheese, which adhered to the vessel in the form of a net. Treated before the blowpipe with a solution of cobalt, the precipitate became of a grey color. On being kept in contact with the supernatant liquid for a long time, even in the case of air being entirely excluded, the precipitate was redissolved, but on boiling it was again thrown down.

SEPARATION OF ALUMINA FROM BERYL EARTH.

Berthier proposes two methods of separating these earths. He boils the solution containing them with sulphite of ammonia to throw down the alumina, and from the filtered mass he precipitates the beryl earth by ammonia.

As I stated above, beryl earth, boiled with sulphite of ammonia, is likewise partially precipitated, being, however, after some time redissolved; if, therefore, the precipitate were filtered off immediately and determined as alumina, a portion of the beryl earth would be weighed with it. To prevent this error I allowed the precipitate to remain in contact with the supernatant liquid, in a well-closed vessel, for twenty-four hours, in order to redissolve the beryl earth; with the pure earth this is effected in a few hours. The precipitate was then filtered off, and having been washed, dried, and strongly calcined, for a long time, and weighed as alumina. The sulphurous acid was removed from the filtered mass by boiling with hydrochloic acid, and the beryl earth thrown down by ammonia. The results of two experiments made in this manner, were as follow:—

I.			In 100 parts.
Alumina ..	0·5876	....	81·396
Beryl earth	0·1343	....	18·604
	0·7219		100·000
gave—			
Alumina ..	0·6436	....	89·153
Beryl earth	0·0745	....	10·320
	0·7181		99·473

The calcined alumina precipitate was free from sulphuric acid.

II.			In 100 parts.
Alumina ..	0·3375	....	83·21
Beryl earth	0·0681	....	16·79
	0·4056		100·00
gave—			
Alumina ..	0·4010	....	98·86
Beryl earth	0·0185	....	4·56
	0·4195		103·42

In this experiment the alumina still contained some sulphuric acid, which accounts for the excess resulting. It retains most pertanaciously the last portions of sulphuric acid, and in several experiments, after very strong and long-continued calcination, it was found not to be entirely free from it.

In both these cases the calcined alumina was dissolved in hydrochloric acid, the solution digested with an excess of carbonate of ammonia, and the presence of beryl earth in the filtered mass proved by boiling.

The second method of Berthier, and which H. Rose adopted in his "Analytical Chemistry," consists in the humid hydrate being dissolved in sulphurous acid, the alumina being precipitated from the solution by boiling, and the beryl earth being separated from the filtered mass by ammonia.

In my experiments I entirely followed the method indicated by Rose.* The salts, after being dissolved by hydrochloric acid, were precipitated by ammonia, the precipitated hydrates repeatedly washed with hot water, and while thus suspended in the water, dissolved by directing into it a stream of sulphurous acid gas. To prevent sulphuric acid from being mixed with it, the sulphurous acid gas was washed, and the solutions most carefully kept from the air.

The results obtained by myself are—

I.			In 100 parts.
Alumina ..	0·2883	....	79·335
Beryl earth	0·0751	....	20·665
	0·3634		100·000
gave—			
Alumina ..	0·3150	....	86·68
Beryl earth	0·0510	....	14·03
	0·3660		100·71

II.			In 100 parts.
Alumina ..	0·3359	....	78·08
Beryl earth	0·0943	....	21·92
	0·4302		100 00
gave—			
Alumina ..	0·3947	....	91·74
Beryl earth	0·0405	....	9·41
	0·4352		101·15

* 4th edit., vol. ii., p. 55.

The calcined aluminous precipitate of both experiments was redissolved in hydrochloric acid, and one portion of the solution treated by chloride of barium, when in both cases the fluids were rendered somewhat turbid by sulphate of baryta, which accounts for the excess obtained in both experiments. Another portion of the solution, in hydrochloric acid, was employed as above, to prove the presence of beryl earth, by carbonate of ammonia.

It appears, from these experiments, that the separation of alumina from beryl earth by sulphurous acid, can be only partially effected, though the behavior of the individual earths with this acid seemed to promise a more favorable result. It seems that beryl earth precipitated, together with alumina, by sulphite of ammonia and by boiling, is prevented, by this latter earth, from being redissolved, even after having been digested for a longer time; and that, by heating the sulphurous acid solution, the beryl earth is carried down with the alumina, as is the case with the oxide of chromium, of which we shall presently speak. It is, therefore, better to separate these two earths by carbonate of ammonia, than by sulphurous acid, since in the alumina separated by this latter body, an amount of beryl earth may be detected by carbonate of ammonia.

BEHAVIOR WITH OXIDE OF CHROMIUM.

With reference to this oxide my observations are in accordance with those of Berthier. The hydrated oxide of chromium is soluble in sulphurous acid; on boiling this solution for a long time a greenish precipitate is thrown down, which Berthier regarded as a basic sulphite. The solutions of the salts of oxide of chromium, however, even on being boiled for a long time, are not precipitated by sulphite of ammonia.

The solution of oxide of chromium, on being treated with alum, and boiled with sulphite of ammonia, throws down the oxide of chromium together with the alumina.

Here we see still more clearly than in the case of beryl earth and alumina, that the behavior of a mixture of oxides, with reference to the substance employed for separation, may be different from the behavior of each of them taken individually; one oxide is carried down by the other.

BEHAVIOR OF PEROXIDE OF IRON.

Solutions of peroxide of iron, on being treated cold or boiling with sulphurous acid, or sulphite of ammonia, are not rendered turbid; the liquid becomes of a blood-red color, but on being kept for some time at the ordinary temperature, it is rendered colorless, which is instantly effected by boiling, the peroxide of iron at the same time being transformed into the protoxide.

Having removed the sulphurous acid by boiling, and the liquid having been allowed to stand in contact with air, a yellowish-red ochrey precipitate is deposited, the liquid itself likewise becoming yellow.

Phosphate of iron behaves in the manner indicated by Berthier.

SEPARATION OF ALUMINA FROM PEROXIDE OF IRON.

If the solutions of these two oxides be boiled with a sufficient quantity of sulphite of ammonia until the odor of sulphurous acid disappears, the alumina is completely precipitated, being free from any admixture of peroxide of iron, in conformity with Berthier's statements. Air must be carefully excluded. In separating the solution of the sulphate of the protoxide of iron from the precipitated alumina, we are, however, unable to prevent some particles of the protoxide of iron from falling down, and thus coloring the precipitate, the latter consequently is not obtained perfectly free from iron. For this reason, therefore, it seems that the method of separating the two oxides now in use is preferable.

BEHAVIOR OF THE METALLIC OXIDES OF THE MAGNESIAN SERIES.

The behavior of the salts of the oxides belonging to the magnesian series with sulphite of ammonia is uniform throughout: the results obtained by myself differ, however, from those stated by Berthier. I found that the salts of the said metallic oxides, on being boiled for a long time with sulphite of ammonia, were partially or wholly precipitated. According to the state of concentration of the solutions and the quantity of sulphite of ammonia employed, the precipitates are formed with greater or less facility; thus the solution being highly concentrated, and the proportion of the salt of ammonia very small, it will require long boiling to produce a precipitate; but from a diluted solution containing a great excess of sulphite of ammonia, the precipitate is easily thrown down. These precipitates are throughout of a grainy crystalline structure, varying in composition; double salts are formed, being sometimes combined with ammonia, sometimes more or less basic, and sometimes a mixture of both I am not yet able to account for the circumstances which give rise to the one or the other; I hope, however, that a continued inquiry will lead me to ascertain this point. The matter seems chiefly to rest in the quantity of the sulphite of ammonia employed.

All the salts, the analysis of which is here communicated, were dried over sulphuric acid.

BEHAVIOR OF PROTOXIDE OF IRON.

A solution of sulphate of protoxide of iron, on being boiled for a long time with sulphite of ammonia, throws down a white grainy precipitate, which rapidly becomes brown. On being treated with hydrochloric acid, it disengages sulphurous acid, and with hydrate of potassa, ammonia; it therefore seems to be a sulphite of protoxide of iron and ammonia. This salt being easily decomposed, I did not attempt to investigate its composition.

BEHAVIOR OF PROTOXIDE OF MANGANESE.

The precipitates obtained by boiling solutions of protoxide of manganese with sulphite of ammonia, are throughout of a reddish color, some of them containing ammonia.

100 parts of the precipitates free from ammonia gave on analysis 46·94, 47·52, and 48·52 of protoxide of manganese,* 37·82 sulphurous acid,† and 14·8 water.‡ The protoxide of manganese was determined by precipitating the hydrochloric solution by carbonate of soda, and calcining the precipitate. To determine the sulphurous acid, the salt was fused with nitre and carbonate of potassa (both perfectly free from sulphuric acid); the sulphuric acid thus formed was determined by baryta, and from that the sulphurous acid was calculated. The water was directly determined by heating the salt in a combustion tube, to which a tube containing chloride of calcium was attached.

According to these numbers the salts obtained are mixtures of simple sulphite of protoxide of manganese, with various quantities of protoxide of manganese. 100 parts of the salts containing ammonia gave in two analyses 34·70 and 34·87 protoxide of manganese,§ and 36·35 and 37·00 sulphurous acid.‖ It is difficult to calculate a formula for this composition.

BEHAVIOR OF THE OXIDE OF ZINC.

Calcined zinc dissolves with some difficulty in sulphurous acid; the oxide is again precipitated by boiling. A solution of oxide of sulphate of zinc is precipitated by sulphite of ammonia, only by boiling for a long time; the precipitate is white, grainy, and crystalline, but varies greatly in its composition. In various preparations, I obtained salts containing in 100 parts 59·39, 59·67, 66·81, 68·82, 70·23, oxide of zinc,* and 26·62, 23·23, 14·11 per cent. sulphurous acid.† With this oxide I noticed no double salt of ammonia.

BEHAVIOR WITH OXIDE OF NICKEL.

It resembles that of the protoxide of manganese. The precipitate is of a yellowish-green, and on being dried in the air it soon blackens. All the salts obtained yielded ammonia.

In one instance, the analysis showed in 100 parts 22·84 and 23·09 oxide of nickel,‡ and 36·23 sulphurous acid.§

The formula NiO, NH^3, $2SO^2$, 5HO, requires:—

NiO	469·67	22·78
NH^3	214 47	10·40
$2SO^2$	802·34	38·92
5HO	562·35	27 90
	2048·83	100·00

Another specimen of these salts, on being analysed yielded, in 100 parts, 36·35 oxide of nickel, 1·99 ammonia, and 30·92 sulphurous acid,¶ which, therefore, is a mixture of a double salt of ammonia and a basic salt of oxide of nickel.

BEHAVIOR OF THE PROTOXIDE OF COBALT.

The solution of this body, on being boiled with sulphite of ammonia, behaves in a similar manner to the protoxide of manganese. The fine rose-red precipitate thrown down, on being dried in the air, soon becomes brown. Like the salt of nickel, it always contains ammonia.

If the solution of the protoxide of cobalt be precipitated by ammonia, and the precipitate redissolved by sulphurous acid, the said precipitate is reobtained by boiling.

In both cases, by boiling with a sufficient

* 0·8229 gave 0·4152 Mn^3 O^4; 0·5378 gave 0·2715 Mn^3 O^4; 1·202 gave 0·614 Mn^3 O^4; 0·345 gave 0·180 Mn^3 O^4.

† 0·869 gave 1·183 SO^3, BaO.

‡ 0·5693 gave 0·0845 water.

§ 0·693 gave 0·2585 Mn^3 O^4; 0·603 gave 0·226 Mn^3 O^4.

‖ 0·9036 gave 1·194 SO^3 BaO; 0·8173 gave 1·0993 SO^3 BaO.

* 0·2963 gave 0·176 Zn O; 0·2988 gave 0·1783 Zn O; 0·3056 gave 0·2042 Zn O; 0·2306 gave 0·1587 Zn O; 0·1737 gave 0·122 Zn O.

† 0·3445 gave 0·3335 SO^3, Ba O; 0·2504 gave 0·2115 SO^3, Ba O; 0·2758 gave 0·1415 SO^3, Ba O.

‡ 0·4185 gave 0·0956 Ni O; 0·4775 gave 0·1103 Ni O.

§ 0·6385 gave 0·841 SO^3 Ba O.

¶ 0·1579 gave 0·574 NiO; 0·5182 gave 0·1324 chloride of ammonia and platinum; 0·3759 gave 0·4225 SO^3, BaO.

quantity of sulphite of ammonia for a long time, the whole of the cobalt is separated, hence the filtered mass being rendered alkaline by ammonia, and treated with hydrosulphuret of ammonia, throws down no precipitate.

BEHAVIOR OF THE OXIDE OF URANIUM.

A solution of nitrate of oxide of uranium when cold, is precipitated with a yellow color by sulphite of ammonia; by boiling the precipitate augments. I could not, however, completely succeed in removing uranium from its solution by sulphite of ammonia; even by adding a great excess of this latter body and boiling for a long time, the filtered mass, by the reaction of the ferrocyanide of potassium formed in the operation, threw down a small red-brown precipitate.

The yellow precipitate was lately investigated by Dr. Muspratt;* he found it composed according to the following formula:—

$$Ur^2 O^3, SO^2, 3HO.$$

SEPARATION OF URANIUM FROM MANGANESE, NICKEL, COBALT, ZINC, AND IRON.

According to the foregoing, the separation of these oxides from uranium by sulphite of ammonia, cannot be effected, for two reasons; first, on account of the facility with which the protoxide of iron is oxidised, as I stated when speaking of alumina, owing to which uranium can scarcely be obtained free from iron; and with reference to the other oxides, on account of their property of being themselves partially precipitated by sulphite of ammonia; and, in addition to this, because uranium itself cannot be completely separated from its solutions by the said reagent.

BEHAVIOR WITH OXIDE OF ANTIMONY.

This oxide dissolves slowly and with difficulty, in sulphurous acid; chloride of antimony, when cold, is largely precipitated by sulphite of ammonia; the precipitate is white and flaky, and, on heating the liquid, becomes pulverulent. The liquid still contains much antimony in solution. The precipitate thrown down by sulphite of ammonia is readily soluble in tartaric acid. By adding tartaric acid, therefore, to a solution of oxide of antimony, or by employing a solution of tartar emetic, the sulphite of ammonia, whether cold or boiling, does not precipitate.

BEHAVIOR WITH PROTOXIDE OF TIN.

It has already been recommended by Pelletier, and at a later period again by Girardin,* to take advantage of the behavior of the protochloride of tin with sulphuric acid, for testing commercial hydrochloric acid by sulphurous acid. The liquid is rendered turbid, and deposits a gray precipitate, which gradually, and chiefly on being heated, becomes darker. Girardin and Hering,† on more closely investigating the products obtained, found that the sulphurous acid was reduced, and bichloride of tin and sulphuret of tin (a degree between sulphuret of tin and sulphuret of tin *in minimo*—Girardin, protosulphuret of tin—Hering) are formed. According to Hering, the bichloride of tin remains in solution, whereas Girardin maintains that the modification, which is insoluble in hydrochloric acid, is separated (or, in the absence of any free acid, binoxide of tin).

Protochloride of tin being treated, when cold, with sulphite of ammonia, throws down a white, gelatinous precipitate (a basic sulphite—Berthier), which, on being boiled, in consequence of the loss of the acid, is augmented and becomes darker. The filtered mass, freed from sulphurous acid by boiling with hydrochloric acid, and treated with sulphuretted hydrogen, throws down no precipitate. The precipitate thrown down without heat, is easily dissolved by tartaric acid.

The solution of protochloride of tin in tartaric acid, on being treated with sulphite of ammonia when cold, throws down no precipitate; by boiling the liquid is rendered turbid—a mixture of sulphuret of tin and sulphur, forming a brownish precipitate, is thrown down, whereas the filtered mass contains much tin in solution. By means of sulphuretted hydrogen, a brownish precipitate is thrown down, which becomes yellow by boiling.

BEHAVIOR OF THE BINOXIDE OF TIN.

A solution of the protochloride of tin (obtained by fusing the insoluble binoxide of tin with potassa, and treating the fused mass with hydrochloric acid, or by directing a current of chlorine into a solution of protochloride of tin), without heat, is not rendered turbid by sulphite of ammonia; on boiling, however, the tin is entirely removed, and sulphuretted hydrogen precipitates no sulphuret of tin from the filtered mass.

On adding tartaric acid to the solution of bichloride of tin, sulphite of ammonia throws

* Liebig's *Annalen der Chemie und Pharmacie*, vol. l., p. 299.

* *Journal für Prakt. Chem.*, vol. vi., p. 81.

† Liebig's *Annalen der Chemie und Pharmacie*, vol. xxix., p. 81.

down no precipitate, with or without the aid of heat.

SEPARATION OF TIN FROM ANTIMONY.

It was proposed by Berthier to separate tin from antimony, by adding tartaric acid to the solution of the two metals in hydrochloric acid, and boiling with ammonia, to precipitate the tin; but we do not succeed in that process, on account of the behavior communicated above, for in the presence of tartaric acid the tin is not completely separated, and without this body the antimony is partially precipitated.

BEHAVIOR OF THE OXIDE OF COPPER.

Vogel has communicated a series of experiments* respecting the behavior of the oxide of copper and its salts, with sulphurous acid, from which it appears that, on being treated by sulphurous acid, the whole of the salts of the oxide of copper are reduced to proto-salts. From salts of feeble acids, protoxide of copper is separated, as a red-brown powder; but, in salts of strong acids, this body remains in solution. Berthier states that salts of oxide of copper are not reduced by sulphurous acid which may have originated from an insufficient quantity of sulphurous acid employed by Berthier. A sufficient quantity of sulphurous acid being added to a salt of oxide of copper, and heat applied at the same time, no precipitate is obtained; the liquid becomes green, and then potassa produces a yellowish precipitate. The sulphurous acid, not having been employed in sufficient quantity, potassa throws down a blue precipitate, which, on being heated, becomes brownish-black.

Sulphite of ammonia, containing but a small quantity of free sulphurous acid, being added to a solution of sulphate of oxide of copper, a dirty greenish-yellow precipitate is obtained, which, on the addition of sulphurous acid, is rapidly and easily dissolved, presenting a fine green liquid. If the sulphite of ammonia contain a sufficient quantity of sulphurous acid, no precipitate is thrown down when treated cold. The liquid is likewise green. These solutions being slowly heated to the temperature of about 140° F. a salt is separated in the form of a fine white lamellæ powerfully refracting light; and on the rays of light being directed upon them, colors similar to those of the rainbow, are observed. On applying a stronger heat, this salt is rapidly transformed into a red grainy salt which, under the microscope, presents the form of oblique tetrahedral prisms. This white salt is easily obtained by adding to a solution of sulphate of copper a great excess of sulphite of ammonia, with as much sulphurous acid as throws down no precipitate, applying, at the same time, a gentle heat. On allowing the liquid to stand for some time in close vessels, the above-mentioned combination is separated.

I analysed the salt dried over sulphuric acid, and obtained the following numbers, from which the under-mentioned formulæ were calculated :—

					In 100 parts.
I.	0·1518	gave	0·0147	Cu O	= 50·15 Cu^2 O
II.	0·1826	,,	0·1022	Cu O	= 50·32 Cu^2 O
III.	0·3745	,,	0·4572	SO^3, BaO	= 33·58 SO^2
IV.	0·2346	,,	0·2927	SO^3, BaO	= 34·32 SO^2
V.	0·3749	,,	0·0908	met. platin.	= 4·21 ammonia
VI.	0·4992	,,	0·1375	,,	= 4·78 ,,

These numbers correspond to the formula :—2 Cu ^{2}O, NH^3, $3SO^2$, 3HO, as appears by following table :—

					In 100 parts. Calculated.		Found.
$2Cu^2$ O	1782·86	gave	50·38	..	50·15	and	50·32
NH^3	214·47	,,	6·06	..	4·21	,,	4·78
$3SO^2$	1203·51	,,	34·01	..	34·32	,,	33·58
3HO	337·41	,,	9·53				
	3538·20		99·98				

This combination, as far as I am aware, has not yet been noticed.

A solution of sulphate of copper, being boiled with sulphite of ammonia, containing free sulphurous acid, a fine cochineal-colored salt is precipitated, into which, as has been stated, the white double salt is likewise transformed. This red salt has

* *Journal für Prakt. Chem.*, vols. 29 and 30.

already been analysed by Chevreul, and the following formula adopted—

$$4\ Cu^2\ O,\ 5\ SO^2 + 5\ HO.$$

Muspratt lately analysed a sulphite of protoxide of copper, which he obtained by crystallising a solution of oxide of copper in sulphurous acid. He found that it was composed according to the formula :—

$$Cu^2\ O,\ SO^2,\ HO.$$

I analysed the above-mentioned precipitate, thrown down by sulphite of ammonia, and obtained the following numbers—

			In 100 parts.
I.	0·5495 gave	0·3440 Cu O	= 56·28 Cu^2 O.
II.	0·9012 ,,	0·5620 Cu O	= 56·06 Cu^2 O.
III.	0·5579 ,,	0·6985 SO, BaO	= 34·44 SO^2.

By which the following formula is calculated :—3 Cu^2 O, 4 SO^2, 4 HO.

			In 100 parts. Calculated.	Found.
3 Cu ^{2}O	2674·20 gave	56·55 ..	56·28 and	56·06
4 SO2	1604·68 ,,	33·93 ..	34·44	
4 HO	449·88 ,,	9·51		
	4728·16	99·99		

This salt, being boiled for a long time with the liquid, yields sulphurous acid—a fact already stated by Muspratt. By analysing this precipitate, I found 66·60 per cent. Cu^2 O. This may account for the different results obtained by various chemists who have analysed the red salt. These red salts of protoxide of copper are almost insoluble in water, but soluble in sulphurous acid. Diluted muriatic acid transforms them into difficultly-soluble protochloride of copper ; the concentrated acid dissolves it easily.

On boiling the solution of the oxide of copper, even for a long time, with sulphite of ammonia, though the latter body be present in great excess, the whole of the copper is not precipitated, for by employing sulphuretted hydrogen, a black precipitate is thrown down from the filtered mass.

SEPARATION OF COPPER FROM ZINC, COBALT, NICKEL, MANGANESE, AND IRON.

By the above-stated behavior, it appears that sulphite of ammonia is not capable of producing this effect. The oxide of zinc being difficultly, and only after long boiling, partially precipitated, whereas the greatest portion of the copper is immediately thrown down. This method of separating these two metals might be made use of in experiments, in which only approximative results are desired ; it, however, offers no advantage over any other known process. In accurate analyses it cannot be adopted. To separate copper from iron, maganese, nickel, and cobalt, it cannot be employed for the reasons adduced in the case of oxide of uranium, and because copper cannot be completely precipitated by sulphite of ammonia.

—nt—

ON THE CLAYSTONE OF GEOLOGISTS.*

BY PROFESSOR SCHAFHAULT.

(Syn.—VERHARTERTER THON. THONSTEIN.)

HAUY called it the decomposed compact porphyric feldspath, by which its supposed origin was indicated. As is known, it frequently forms considerable layers in the red clay, in the vicinities of Chemnitz, of Baden in the Schwarzwalde, and of Botzen in South Tyrol. If it contained crystal of feldspath, which generally crumble when exposed to the air, and crystals of quartz, it was called *Thonsteinporphyr*, claystone porphyry being considered as a fused feldspathic mass, which rose from the interior of the earth, lost (later ?) its potassa, and left behind a combination of silica and alumina, being a mixture of bi- and trisilicates.

The circumstance that the principal mass of the feldspath which so considerably predominates having lost its potassa, whereas the smaller interspersed crystals of feldspath remained unaltered, or seemed to have crumbled only to a small extent, and the peculiar structure of the rock, the hardness of which surpasses by far all the other kinds of feldspath which fall to powder, should long ago have excited doubts as to the nature of the rock, if our geologists had not deemed it more convenient to contrive themselves the manner in which nature formed her productions in former ages, instead of (like Newton) studying laboriously, but accurately, the laws in conformity with which nature acts under our own eyes, in forming and producing. They speak, says Liebig, of the origin of rocks and do not know their com-

* Liebig's *Annalen der Chemie und Pharmacie*, 8, 1844.

position, &c. From the rough, lustreless surface of the rock, the geologist has concluded that thonstein is nothing but hardened clay; but Fuchs, as a really well-informed and truly scientific mineralogist, at the first glance guessed at the nature of the rock, and accordingly in his *Naturgeschichte des Mineralreichs*, expressed himself in the following terms (p. 231):—"Thonstein seems for the most part to be an intimate mixture of quartz and feldspath. The thonstein which served for the present investigation is from the rich thonstein layer of the vicinity of Meissen. It is rough to the touch, of a fine grainy earthy fracture and texture, yellowish-white resembling chalk. Its behavior before the blowpipe is remarkable. After the flame has acted for a short time, it exhibits a shining texture, of nearly a crystalline appearance, changing its color into the milk-white; this spot becoming thus clearly distinguished from the neighboring earthy texture of the thonstein which was not within the reach of the flame. In the interior flame it fuses in the edges to a milky-white bead. By this behavior alone it may be concluded, with probability, that a mineral so easily changed by fire did not rise from the depth of the earth in a state of fusion.

But the microscope gives still more convincing proofs that the thonstein was produced in the humid way; for the thonstein, throughout its whole mass, evidently shows remainders of scales of the gaillonella distans, and the grainy structure of the xanthidies.

The above statements are, in an interesting manner, confirmed by chemical analyses. A pure white piece of the mineral having been treated first by carbonate of soda, and afterwards, to separate the silicic acid, by hydrofluoric acid, gave in 100 parts:—

SiO^3	76·45
$Ac^2\,O^3$	14·88
$Fe^2O^3Mn^2O^3$	0·90
KO	6·60
HO	0.93
	99·76

The iron being calculated as protoxide, we obtain:—

72 equivalents
12 ,,
6 ,,
1 ,,

We may, therefore, construct the formula:—

$$6\,Al^2\,O^3\;3\,Sc\,O^3,\;6\,KO\left\{\begin{matrix}FeO\\MnO\end{matrix}\right\}SiO^3.$$
$$SiO^3+6Al^2\;\;O^3(6SiO^3)+$$

If, instead of speculating on the nature of porphyries, the thonstein had been subjected only to a superficial analysis, its considerable amount of potassa could not have remained unnoticed. Chemical analysis, therefore, entirely corroborated the suppositions expressed by Fuchs; instead of belonging to the argil genus it makes part of the feldspath genus, and with reference to its composition, it totally coincides with the weissstein, for according to three analyses made by Klaproth, the average of the composition of weissstein is 77·5 per cent. silica, 12·7 alumina, 5·6 potassa, or, on comparing our Saxon specimen with a sample analysed by Klaproth, the composition of the weissstein of Reichenstein was:—

SiO^3	73·50
$Al^2\,O^3$	15·00
$Ca\,O^1$	1·00
$Fe^2\,O^3$	1·50
KO	6·50
HO	0·75

By forming common feldspath of our thonstein, we obtain

KO	9·10
$Al^2\,O^3$	6·63
SiO^3	23·876
	36·606 per cent.

feldspath; the remaining silica and alumina form the silicate of alumina, consisting of $Al\,Si^6$.

To be able to account for the present form of the thonstein and thonporphyr by volcanic influences, so long as these rocks were considered to be merely hardened clay without potassa, it was necessary to have recourse to the decomposing power of water; which, coming over it at a later period, subtracted the potassa and left the clay behind. The great quantity of potassa, however, contained in these rocks, restores them to their principal individuality. Yet the claystone, in its present form and composition, can have originated only in the humid way (it contains pieces of trees in an erect position), and this the Plutonists partly conceded at the period when they doubted the principal individuality of the thonstein. With a certain consistency, all the various kinds of clay, and the immense depositions of clay, have been considered as the remainder of processes causing matters to fall into powder, and of the destroyed granite formations. The decomposition would then have originated by the subtraction of the potassa induced by the influence of the atmospherilia. Fuchs was the first who proved the individuality of the formations of clay. In his masterly investigations of the hydraulic lime, he discovered its amount of potassa, by which we are war-

ranted in ranking the clay among the younger and older granite formations. But that the argilaceous and granite formations could not have been produced by fire, even the maddest Plutonist will maintain, if he ever noticed, in a potter's manufactory, how fire acts upon clay. It seems, in general, that the fire of the interior of the earth is becoming weaker in proportion as real and vigorous science and impartial observers pay attention to it. In my festival speech on "geology and its relation to the other natural sciences," I directed attention to the undeniable remarks of the English author, Moyle, which, of course, being most unwelcome, were ignored at first.* The spring of the merchant Schergin, at Iakutsk, may serve as a remarkable instance how much we are to rely on the data upon which the belief in the increase of heat towards the centre of the earth is based. Bischof calculates that in those Siberian strata, the thawing point would be found at the depth of 230—256 Prussian feet. And the thawing point not having been found there, Ermann, in his voyage round the world, calculated that it would be found at the depth of 600 feet, and if not there we might add 600 feet again, and as much as we please. At a depth of 54 Saschen, 2 Arschinen = 359·05 Parisian = 382 English feet, the thermometer continues to be —½° R. =31° F.

No real mathematician will seriously be engaged in deriving the law of the increase of heat towards the centre of our earth from the remarks on that subject hitherto known, so long as the immense discrepancy and contradictions of the single observations show, that here mere accident predominates, which has no connexion with the dominion of science.

—nt—

ANALYSIS OF THE MILK OF A BUCK.†

BY J. SCHLOSSBERGER, M.D.

It seems that nature is most disposed to produce anomalies in the sexual parts. Physiological museums contain, besides the most strange hermaphroditical formations of the genital parts proper, almost as numerous and as strange deviations from the normal organisation in the system of the mammæ.

The most remarkable, as they seldom occur, are the cases of male animals furnishing milk. Some instances are recorded of male human beings having yielded milk. The most credible and remarkable case is that noticed by Humboldt,* in America, when the wife was ill, the husband, during five months, gave his breast from two to three times daily to the suckling infant, which received no other nourishment; this milk was dense, and very sweet. The latest case is related by Häser.† With male animals, instances of the secretion of milk are more frequent. We know a similar case of a castrated spe,‡ of a bullock,§ with testes undeveloped, and, finally, of several buck-goats.

Though in these cases,‖ considering the secreting organ, the appearance of the secreted liquid, and chiefly its effect on the suckling (especially in the relation of Humboldt), rendered it probable that it was genuine milk; this secretion, however, in no instance, having been subjected to a microscopico-chemical investigation, doubts remained as to whether the apparently paradoxical statements of the secretion of genuine milk, from male breasts, be correct.

Last summer a buck, furnishing milk, was found in the Neuhof estate, near Giessen, and by the kind invitation of Professor Liebig, the author having had a desirable opportunity afforded to him of making an accurate investigation, he, removing all doubts on that score, was enabled to decide upon the subject in question.

The above-mentioned buck was four years of age, and so late as last autumn several buck-goats having been given birth by his exertions, he gave proofs of his male sex; the testes and penis were examined by Dr.

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* Fox and Forbes, by some experiments, which had indeed little to do with the question, endeavored to render doubtful the results obtained by Moyle, and for this purpose they even changed their manner of calcination; but Moyle met them afterwards with a series of experiments, against which till now no voice was raised.

† Liebig's *Annalen der Chemie und Pharmacie*, Sept., 1844.

* *Reise in der Aequinoctialgegenden*, Stuttg., 1815—19, vol. ii., p. 40.

† Häser's *Archiv*, 1844, p. 272.

‡ Stark's *Archiv für Geburtshülfe*, iv., p. 755.

§ Home's *Lectures on Comparative Anatomy*, London, 1814, vol. iii. p. 326.

‖ Bechstein's *Gemeinnützige Naturgeschichte*, vol. i., p. 420, and Froriep's *Neue Notizen*, 1843, No. 551. According to the statements of an old Swiss, cases of the kind are by no means rare; he even maintained that he had prepared cheese from buck's milk. We shall see that the same must be well qualified for that purpose.

Bardeleben and myself, and found to be of the normal size and development; it was the same with regard to the horns. The two dugs occupied the place in which they usually are met with in bucks, and were of the size of a fist. With some force, which, as it appears to me, occasioned some painful sensation, a liquid resembling milk could be expressed, the quantity of which seemed to be rather inconsiderable; but the animal frequently most eagerly sucking its dugs, it was difficult to obtain considerable quantities of the secreted liquid. I am, however, indebted to the kindness of the proprietor, Mr. Firnhaber, for several ounces of it.

The fresh liquid, obtained by repeated milking, was throughout of the color, consistence, and taste of good milk; and, what is the more remarkable, notwithstanding the vicinity of the fetid organs, no unpleasant odor or taste was noticed. Under the microscope numerous butter globules appeared, the great majority of which were isolated, and singly moved freely one over another; but after this secretion was observed for at least four weeks, rather solid agglomerations of globules were found therein (apparently quite of the same nature as the isolated ones), a species of *corps granuleux* (granular body) discovered by Donné in the *colostrum*; by ether these likewise disappeared. In addition to this, some cells of epithelia were mixed with it (perhaps accidentally). The reaction of the milk was scarcely alkaline; on standing for some time, it deposited a considerable quantity of cream; it showed, however, no great inclination for becoming acid.

6·771 grammes of milk, on being incinerated, left 0·053 grammes of almost pure white ashes; hence 0·782 per cent. In 100 parts of these ashes were 41·6 insoluble in water, and 58·4 salts soluble in water, such as are usually met with in milk. To determine the other constituents of the milk quantitatively, the method recommended by Haidlen was adopted. 17·800 of milk, on being heated with 3·204 gypsum, and dried at 212° F., left 5·858 of solid residue, in which, therefore, 2·654 of *solid parts of milk* corresponded to 14·91 per cent. 5·514 grammes solid residue (containing gypsum), on being extracted by ether, left 5·136; hence 2·12 parts of the solid constituents of milk (the gypsum being subtracted) yielded 0·378 butter. From the residue (5·136) exhausted by ether, alcohol extracted *sugar of milk* and *salts*, and it weighed after this extraction 4·766; hence we calculate 0·370 sugar of milk and salts soluble in alcohol for 2·12 solid constituent parts of milk. That which remained, still combined with the gypsum, consisted of caseïn and salts soluble in alcohol, amounting to 1·376 for 2·12 solid constituent parts of milk.

100 parts of buck's milk, therefore, contain—

85·09 water.

14·91 solid constituent parts (containing 0·782 fixed salts).

In 100 parts of the solid constituent parts are:—

17·83 butter.

17·45 sugar of milk and salts soluble in alcohol.

64·71 caseïn and salts insoluble in alcohol.

In 100 parts of the buck's milk are consequently—

85·09 water.

9·66 caseïn (with salts).

2·60 sugar of milk (with salts).

2·65 butter.

The alcoholic extract of the milk was then investigated solely with reference to its contents in sugar of milk; its presence was clearly proved by sulphate of copper, by potassa, and in various other ways.

On comparing the results obtained here with the analyses of milk known hitherto, it appears that buck's milk is remarkable for its *richness in caseïn*; whereas it is proportionately poorer in butter and sugar of milk than, for instance, cow's milk: it manifestly approaches nearest to the milk of goats, the analyses of which were made some time back, and the methods then adopted were probably rather imperfect.

Goat's milk contains, namely, in 100 parts—*

	According to Boysson.	Luiscius.	John.
Caseïn.....	5·29	9·12	10·54
Butter.....	2·99	4·56	1·17
Sugar of milk..	2·07	4·37	2·34
Water.....	89·28	81·93	84·93

This case of formation of milk in an animal belonging decidedly to the male sex is, perhaps, likewise of some importance for the theory of secretion; it seems to prove the independence of the formation of milk of a peculiar (puerperalis) syncrasy of the blood proper to the pregnant or newly-delivered female animal, confirming the view that the *development of an organ of secretion* is of an unequally higher importance to the production of a specific secretion than a certain syncrasy of the blood. The elements producing milk are present in the blood of both male and female; on the existence of the glands depends the transformation into milk. Or are we to suppose that even in male animals furnishing milk a puerperal syncrasy of the blood takes place, a property of the

* *Vide* Burdach's *Physiology*, vol. iii., p. 148.

blood of which the medical man, rather than the chemist, might form a clear conception?

In some weeks the turn of roes will begin, when it will be interesting to observe whether, at that time, the necessary derivation by the testicles taking place, the milk secretion will experience any change, or cease altogether. —nt—

ANALYSIS OF AN ANIMAL CONCRETION.*

BY F. WÖHLER.

KLAPROTH† analysed an animal concretion, designated by the name of belugenstein, of which Pallas, in his travels, gives the following account:—"In the fisheries of the Caspian sea, the so-called belugenstein is not unfrequently found in the interior of the great sturgeon (accipenser huso) which, undeservedly, however, stands high in the estimation of the Russians as a domestic medicine. According to the statements of the fishermen, this stone is found in one of the cavities near the anus, on each side of the intestines, and through which the fish secrets the feces and emits the eggs. These stones occur likewise in the larger common sturgeon. Their weight amounts to 1, 3, and even 7 oz. Their form is various, sometimes oval, sometimes flat, and somewhat curved. Their color is white, like that of bones, presenting, throughout the fracture, shining, spathiform radii, which run from the surface inwards." In another passage it is said—"It is contained in the red glandular fleshy mass, situated on the posterior portion of the spine, which, in fishes, performs the function of the kidneys, within a peculiar membrane, which occupies the centre of the said glandular organ." From these statements it may be supposed that the stone of these fishes is a concretion produced in the urinary organs. Klaproth found in it

Phosphate of lime.....	71·5
Water and Albumen.....	26·0
Sulphate of lime	0·5
	98·0

Several specimens of this stone are preserved in the Royal Museum of Berlin. Their crystalline and uniform constitution shows that they consist of a determined proportionate combination, to examine which appeared to me interesting, even in a physiological point of view, the phosphate of lime, as it usually occurs in the animal body, being invariably found uncrystallised. J. Müller was kind enough to furnish me with materials which were probably taken from the same specimen that Klaproth investigated.

Fine splinters of this concretion, on being viewed with the microscope, were found to be perfectly transparent, colorless, and of a homogeneous constitution. On being heated they become opaque, white, and lose pure water. On being calcined, they feebly blacken, evolving empyreumatical products. They melt before the blowpipe, in which character this salt of lime essentially differs from the usual bone-earth. This concretion is easily dissolved by hydrochloric acid, without disengaging any gas, leaving behind, however, an organic substance representing the volume and form of the sample employed, yet being very soft and tumid, and after drying amounting to no more than 0·74 per cent. The solution being precipitated by ammonia, phosphate of ammonia remains dissolved, which is another character rendering this substance essentially different from bone-earth. It contains neither magnesia nor any alkali.

On calcination, the weight of this substance was diminished by 27·0 per cent. Reduced to powder, and heated for a long time, until it reached 392° F., it lost 19·93 per cent. water.

To determine the lime quantitatively, the concretion was dissolved in a mixture of alcohol and hydrochloric acid, the lime thrown down by sulphuric acid, and the precipitated sulphate of lime washed with alcohol and calcined. On fusion, with four times its weight of carbonate of soda, it was only incompletely decomposed. The same is the case with bone-earth; it cannot be completely decomposed in that manner.

The phosphoric acid was determined by the loss; 1·882 grammes concretion gave 1·444 grammes sulphate of lime, corresponding to 31·66 per cent. of lime. Hence in 100 parts of the concretion—

		Calculated acccording $CaO\ PO^5+5HO$.
Phosphoric acid..	41·34	41·57
Lime.....	31·66	32·48
Water	26·26	25·95
Organic matter ..	0·74	

This concretion consists, therefore, of the so-called neutral phosphate of lime with 5 atoms of water $=CaO\ PO^5+5HO$ whereas the common bone-earth is $=8\ CaO^3\ PO^5$, or, more accurately $= 2CaO\ PO^5 + 6CaO\ 2PO^5$. As appears from the loss by calcination, and the quantity of the organic matter, only one-fifth of the water

* Liebig's *Annalen der Chemie und Pharmacie*, September, 1844.

† Beiträge, vi., p. 218.

is removed by calcination. The ordinary salt of lime in this stage of saturation is, as is known = $2CaO\ PO^5 + 6HO$, and contains only 21·9 per cent. water. The question now remains, whether the bones of these fishes do contain this salt of lime in place of the common bone-earth.

—nt—

ON EUCHROÏTE.*

BY FR. WÖHLER.

THE analyses of the euchroïte of Libethen made by Mr. Khün, and communicated in Liebig's *Annalen*, vol. li., p. 128 (THE CHEMIST, October, 1844, p. 451), brings to my recollection an analysis of the same mineral which I made some years ago at the request of my colleague, Hausmann, when the results I obtained essentially coincided with those communicated by Turner, in 1825.†

By calcination the mineral lost 18·39 per cent. of water.

Dissolved in nitric acid, the solution treated with an excess of carbonate of soda, the mass obtained evaporated, and calcined gave 0·786 euchroïte, 0·378 or 48·09 per cent. of oxide of copper.

The alkaline solution, on being boiled with hydrosulphate of ammonia, and precipitated by hydrochloric acid, gave pure sulphuret of arsenic, the quantity of which, however, was not determined. The filtered mass, on being treated with ammonia and nitrate of lime, gave 0·005 grammes of phosphate of lime.

The oxide of copper contained 0·004 grammes of oxide of nickel, and 0·001 grammes oxide of iron. It was dissolved in hydrochloric acid, and the solution precipitated by sulphuretted hydrogen.

From the liquid filtered off from the sulphuret of copper, hydrosulphate of ammonia threw down a black precipitate of sulphuret of nickel, mixed with some sulphuret of iron. These results, which are given below, together with Turner's analysis, show the following composition :—

	Turner.	Wöhler.	Calculated according $Cu^4As + 7H$
Arsenic acid	33·02	33·22	34·21
Oxide of copper	47·85	48·09	47·09
Water	18·80	18·39	18·70
Oxide of nickel, Protoxide of iron, Phosphoric acid	in considerably small quantities.		

The mineral must evidently be considered as composed of $(Cu^3As + 6H) + (CuH)$.

This analysis, therefore, corroborates that of Khün.

—nt—

ON A NEW KIND OF SALTS OBTAINED BY THE ACTION OF SULPHURETTED HYDROGEN ON THE ARSENIATES.*

BY MM. J. BOUQUET AND S. CLOEZ.

THE study of the products which are formed when sulphuretted hydrogen is made to act on the soluble arseniates was made by Berzelius, in the memoir which he published, in 1826, on the sulph-arseniates. That illustrious chemist observed that, in this case, sulphuretted hydrogen, by its hydrogen, removes all the oxygen from the arseniate and that the sulphur is substituted for it, so that after the reaction we have a new arseniate in which all the oxygen is replaced by sulphur.

We have observed, in an entirely similar reaction, that the substitution of oxygen by sulphur experiences, in some measure, a period of arrest, and we have obtained a perfectly crystallised salt, corresponding in composition to the arseniates. It differs, however, in this point, that its acid contains both sulphur and oxygen, independently of arsenic, which is its radical.

The following are the circumstances in which this salt is formed :—A rapid current of sulphuretted hydrogen is passed into a cold saturated solution of binarseniate of potassa ; in a few moments, sulphuret of arsenic is precipitated, then white crystals are formed which sink into the bottom of the vessel in which the reaction is operated. When a certain quantity of these crystals is deposited, a little potassa is added, so as to render the liquor alkaline. Sulphuretted hydrogen is continued to be passed in, until the sulphuret of arsenic has acquired a gray tint ; the liquor is then filtered and crystallised *in vacuo*.

The crystals which are formed are always

* Liebig's *Annalen der Chemie und Pharmacie*, August, 1844.

† Poggend. *Annalen*, vol. v., p. 165.

* *Comptes Rendus*, No. 21, Nov. 18, 1844.

soiled by a yellow powder; they are washed with a little distilled water, pressed between folds of blotting paper, and their desiccation is completed *in vacuo*.

The analysis of this salt led us to represent it by the formula $ArO^3 S^2$, KO + 2HO.

This formula is not the only one which may be presented to explain the constitution of this salt; thus, if we triple the foregoing formula, it may be represented in the following manner:—

$$2\,(Ar\,O^5, KO) + Ar\,S^5, KS + 6HO.$$

Under this form, it would be a combination of two equivalents of binarseniate of potassa, and one equivalent of sulpharseniate of potassa. But if such were its composition, it would precipitate sulphuret of arsenic by the action of the acids, which is not the case; in this case, the salt is decomposed and precipitates nothing but sulphur.

According to another view, this salt might be considered as binarseniate of potassa, in which the water of crystallisation is replaced by sulphuretted hydrogen.

Arseniate of potassa = ArO^5, KO + 2HO.

The salt studied .. = ArO^5, KO + 2HS.

But we do not think that this is the constitution of this salt, for if a salt of lead be precipitated by a solution of the salt, a white precipitate is obtained; according to the foregoing hypothesis, it should be black. The precipate turns black, it is true, but it is ordinarily only after two or three hours, and then the precipitate is completely decomposed.

We thought we ought to give the preference to the first formula which we have presented; this formula admits, in this salt, the existence of a new acid, analogous in composition to arsenic acid, but containing, besides its radical, sulphur and oxygen. Without prejudging the name which the further progress of chemistry may assign to this new acid, and desiring only to give it one which may prevent confusion with those already known, we propose that of *sulphoxyarsenic acid*.

SULPHOXYARSENIC ACID

$$= ArO^3 S^2, KO + 2HO.$$

This salt is white, crystallised in small prisms which sometimes attain the length of one or two centimetres;* it is sparingly soluble in water. We were unable to determine this solubility directly, for the salt in solution is promptly decomposed, even *in vacuo*.

However, if the solution be saturated, and if, moreover, it be rather alkaline, a crystallisation *in vacuo* may be effected. The salt is not, indeed, entirely preserved from decomposition, but a portion escapes it, and may be crystallised.

The dry salt is completely unalterable in contact with the air.

But if the temperature be raised, it yields, in decomposing, very complex products.

Towards 380°F., it disengages all its water, and turns yellow, without fusing.

Heated in the flame of a spirit-lamp, it fuses, loses its water, and disengages sulphuret of arsenic, and then arsenic which sublimes in the neck of the retort in briliant crystals.

The red-brown residue which remains at the bottom of the retort, contains sulphate of potassa, a sulpho-salt, containing one of the sulphurets of arsenic, and a trace of arseniate of potassa.

The salt in solution is promptly decomposed; this decomposition, which commences even without heat, is complete at the boiling temperature. Traces of sulphuretted hydrogen are disengaged, and a small quantity of a dirty-yellow powder is deposited, which, when analysed, was found to contain only sulphur.

The liquor contains a new sulpho-salt, and hydrochloric acid precipitates from it sulphuret of arsenic. It appears to contain, likewise, arsenite of potassa, for it immediately precipitates, by sulphuretted hydrogen, when treated by that reagent, after having added to it hydrochloric acid and filtered.

The solution, thus decomposed by ebullition, does not contain sulphate.

If we insist on the decomposition which the sulphoxyarseniate of potassa undergoes in the presence of water. It is we think because that the explanation of this fact will throw some light on the constitution of this salt, as we present it.

Indeed, the ready alteration of sulphoxyarseniate of potassa, and the great facility with which it separates from its two equivalents of sulphur, may cause it to be admitted that at the first moment of the decomposition it separates into sulphur and arsenite of potassa:—

$$ArO^3 S^2, KO = Ar\,O^3\,KO + 2S.$$

Arsenic acid is a feeble acid, and does not completely saturate potassa; so that, admitting that sulphur may act, as if it were nearly free, we have the explanation of these phenomena which are apparently so complicated.

Potassa and sulphur, in presence of water, give a hyposulphite and a polysulphuret: we

* During Professor Mitscherlich's recent stay in Paris, we took occasion to communicate to him the principle results of this work. He was kind enough to undertake the crystallographic examination of our salt.

may remark, that the liquor does not contain sulphate. If, in this reaction, a polysulphuret is formed, it will have sulphurised and have dissolved in proportion, part of the arsenious acid; hence, the formation of a sulpharsenite, and the deposit of sulphur. Finally, as there is little sulphur, part of the arsenious acid escapes sulphurisation.

Pure hydrochloric acid, free from chlorine, immediately decomposes the salt. The sulphur is completely separated from it; for, if the mixture be boiled, the sulphur collects into a globule, whose weight represents, at nearly half per cent., that which more accurate analysis have proved to us to exist in this salt.

The formula of sulphoxyarseniate of potassa shows us that, if all the sulphur of this product be removed, the remaining elements constitute arsenite of potassa. It is, indeed, this product, or rather arsenious acid, which remains in the solution after the decoloration of the salt by hydrochloric acid.

The presence of arsenious acid in this solution was demonstrated by sulphuretted hydrogen, which immediately precipitates it, and also by the characteristic green precipitate which is obtained with sulphate of copper.

When a salt of lead is precipitated by a solution of sulphoxyarseniate of potassa, a white precipitate is obtained, which may be washed with cold water for two or three hours, without its color being altered, but it very soon colors, and, after a day or two, it becomes entirely black.

If, before this alteration, this precipitate in suspension in water, be treated by a few drops of sulphuric acid, and if the liquor be filtered, after a few minutes' contact, we obtain an acid liquor which does not precipitate the salts of baryta, and which soon becomes turbid, and precipitates sulphur.

We evidently had in solution sulphoxyarsenic acid, but its prompt alteration prevented us from crystallising it, and even from studying its properties in the isolated state, for its existence is ephemeral.

If we compare the action produced by sulphuric acid on the salt of lead with that exerted by hydrochloric acid on the salt of potassa, it will be evident that they are of the same order. In both cases, the sulphoxyarsenic acid was isolated; but as soon as separated, it was decomposed into sulphur and arsenious acid. This alteration appears to be its most dinstinctive character.

We have spoken of the great analogy of composition which exists between the arseniate and the sulphoxyarseniate of potassa; the comparison of the respective formula of these two salts completely demonstrates this analogy:—

Binarseniate of potassa = ArO^5, KO + HO.
Sulphoxyarseniate of potassa = ArO^3, S^2 KO + 2HO.

We have also seen that at 338° F., the salt which we have studied completely, lost its water, and was decomposed.

If now we call to mind the very great analogy which exists between arsenic and phosphoric acids, and that which we have endeavored to establish between arsenic and sulphoxyarsenic acids, a theoretical inference which we have deduced from this comparison may not be considered rash.

In our opinion, the 2 equivalents of water of our salt are basic water, and sulphoxyarsenic acid is a tribasic acid, like phosphoric acid.

This property has not been demonstrated, it is true, as regards arsenic acid, but it is very probable, and some experiments which we propose to continue lead us to believe, that arsenic acid may present, in its hydratation, the same phenomena as phosphoric acid.

Another theoretical consideration seems to us to present, perhaps, a higher degree of importance.

We think that sulphoxyarsenic acid, which is intermediate in composition between the acid and sulphuret of arsenic, is not the only compound which connects these two acids with each other.

We believe in the existence of a series similar to that which the excellent work of M. Regnault on the chloruretted ether has made known to us.

The two extreme terms of the series are the acid and sulphuret of arsenic; sulphoxyarsenic acid is an intermedium. The series, to be complete, would require three other terms, and it is probable that researches made with this object may fill up this gap.

This series of compounds may be represented by the following formula:—

ArO^5	Arsenic acid.
ArO^4S, ArO^3S^2, ArO^2S, $ArO\,S^4$	Sulphoxyarsenic acid.
ArO^5	Sulphuret of arsenic.

In terminating this paper, a duty remains for us to fulfil, which is to acknowledge, with thanks, the encouragement and advice afforded us by our masters MM Regnault and Pelouze, and especially MM. Ebelm and Frémy, by which our task has been rendered much more easy.

ON THE PRODUCTION OF BUBBLES IN PLATINUM VESSELS.*

BY PROFESSOR PLEISCHL.

[Abridged from the German.]

This accident has generally been attributed to the presence of arsenic, purposely added, which the late Jeannetti of Paris was indeed in the habit of doing; but though his successors have publicly asserted that this practice is discontinued in their manufactory, the author lost a useful platinum spoon and capsule, by bubbles being formed, with subsequent perforation; yet these vessels were made in the said establishment not more than two years ago.

The author, admitting that arsenic may produce this effect, is of opinion that the purest platinum is nevertheless liable to the same inconvenience; this was suggested to him by the analogy between platinum and iron, with reference to the difficulty of forging them.

In preparing potassium in bottles of forged iron, he noticed the same circumstance — bubbles were formed, and the vessel became perforated, the suspicion of arsenic being added to the iron falling to the ground. He thinks the reason may, perhaps, be found in a want of contact between the metallic particles, and advises that the metals be most carefully hammered in working it out, and hopes thus to prevent this accident.

—nt—

ON LINSEED OIL—ITS PHYSICAL AND CHEMICAL PROPERTIES, AND ITS PRODUCTS OF OXIDATION.*

BY DR. F. SACC.

The linseed oil employed in the following experiments was expressed from seeds of last year's growth, without the application of heat. It was of a bright yellow color, and somewhat thicker than linseed oil commonly is; its taste was rather insipid.

Linseed oil, even when exposed to a very low temperature, does not become solid; it is soluble in alcohol, and still more so in ether, but insoluble in water, and of a less specific gravity than water.

It is easily saponified with alkalies, alkaline earths, and oxide of lead. On being distilled in close vessels, without boiling, it yields a great quantity of white fumes, which, condensed in the neck of the retort, form a clear, colorless oily liquid, the odor of which reminds us of that of fresh bread: but the development of the fumes suddenly ceases, and the oil, being violently tumefied, begins to boil; a skin is formed on its surface, which breaks; from this moment the products passing over become more and more intensely brown, until the oil is converted into a mass of a kind of gelatinous caoutchouc. All these remarkable products will be the subject of another treatise.

Linseed oil, on being treated with nitrous acid, becomes red and tough, but no trace of elaïdin is formed.

On passing through it a current of sulphurous acid, its yellow color becomes lighter. Some light flakes are deposited, which are nothing but gypsum, originating from the lime, which always is suspended in the oil in great quantities.

Notwithstanding all the precautions recommended by Frémy, in his treatise on the products obtained by the action of sulphuric acid on fatty bodies having been observed by mixing linseed oil with concentrated sulphuric acid, I never succeeded in preventing the incipient decomposition. The sulphuric acid first coagulates all the oil with which it comes in contact. The mixture is at first of a purple hue, then turns to violet, and, as soon as the mass entirely blackens, large quantities of sulphurous and formic acids are developed. When the violent action abates, a black tough mass is obtained, which may be drawn out in long filaments, a property which it retains even after boiling with water. It is insoluble in water, but soluble in alcohol, from which, on being evaporated, it separates unaltered. It is easily converted by the alkalies into a fine, gelatinous, light-yellow soap.

On being saponified with soda, linseed oil furnishes a fine yellow soap of a peculiar insipid odor. When this soap is decomposed by hydrochloric acid, a considerable quantity of fatty acids is separated, collecting, on the surface of the liquid, in the form of a yellow oil, which is soon filled with long, shining, interwoven needles, which, as will be more fully shown in the sequel, are nothing but pure margaric acid. The liquid acid, on which the margaric acid needles float, is oleïc acid; this is separated from margaric acid by employing the greatest possible cold. Without this precaution, an extraordinarily great quantity of acid is lost.

On being treated, at a gentle heat, with water and oxide of lead, linseed oil furnishes a fine light-gray unctuous soap, and a great quantity of glycerine. When this soap is

* Poggendorf's *Annalen der Physik und Chemie*, 9, 1844.

* Liebig's *Annalen der Chemie und Pharmacie*, 8, 1844.

shaken with ether, the whole of the oleate of lead is dissolved; and what remains is impure margarate of lead.

The ethereal solution of the oleate of lead being evaporated, the ether rapidly becomes resinous; a white basic salt is precipitated, over which is perceived a gelatinous, transparent, reddish-brown salt, of an acid reaction, the odor of which reminds us of that of the oil. In making this analysis, the spontaneous decomposition of the oleate of lead requires particular precautions. The results obtained by various analyses of this kind never coincide, which apparently originates from the rare homogeneity of the mass, in which salts of various composition and in different manners must be distributed. It is most advisable never to dry at one time more of the dissolved salt of lead than is requisite for a complete analysis; and in that manner numbers are obtained which coincide, and probably approximate to the true ones.

When oleate of lead is dried in thin layers over wood, no varnishing cover is formed like gums; it separates in scales, the principle being wanted to which linseed-oil varnish owes its adhesiveness. This principle is margaric acid, which, by virtue of its fatty constitution, retains the linseed varnishes in their characteristic state of pliability.

The oleate of lead being heated and decomposed by hydrochloric acid, and the resulting acid washed with hot water and treated with ether, which is as rapidly as possible evaporated again, orange-yellow oleïc acid was obtained, which is much less fluid than the common one, and contains one equivalent of oxygen more.

It is, however, better to decompose the salt of lead by sulphuretted hydrogen, to treat the acid with ether, and to evaporate this latter body very rapidly. In this manner a very fluid, light yellow, inodorus oleïc acid was obtained, which possessed all the properties of the common oleïc acid, and which gave, on analysis, the following results:—

I. 0·3044 grammes oleïc acid gave 0·8310 grammes carbonic acid, and 0·2928 grammes water.

II. 0·2676 grammes oleïc acid gave 0·7315 grammes carbonic acid, and 0·2577 grammes water.

	I.	II.	Mean.
Carbon* ..	75·459 ..	75·560 ..	75·509
Hydrogen	10·643 ..	10·650 ..	10·646
Oxygen ..	10·898 ..	13·790 ..	13·844
	100·000	100·000	99·999

Based on the atomic weight of the lead-salt, which is 4425·7, these numbers lead to the formula—

$$C^{46}\ H^{39}\ O^{6},$$

to which the following per centage corresponds:—

Carbon	76·03
Hydrogen	10·74
Oxygen	13·23
	100·00

The formula just given represents hydrated oleïc acid; the composition of anhydrous oleïc acid is—

$$C^{46}\ H^{38}\ O^{5},$$

and differs from the formula derived by Varrentrapp* from the oleïc acid furnished by other fatty bodies—

$$C^{44}\ H^{39}\ O^{4},$$

containing two equiv. carbon, and one equiv. oxygen more, but one equiv. hydrogen less. Varrentrapp, however, having calculated according to the old atomic weight of carbon, the difference is reduced to one equiv. carbon. This difference, and the excess of oxygen with the smaller quantity of the equiv. of hydrogen, are sufficient to characterise the oleïc acid of linseed oil as a new substance, differing from the oleïc acid of other fatty bodies; hence, in this case, analysis has confirmed that which experience long since anticipated.

That portion of the lead-soap which is insoluble, or difficulty soluble, in ether, consists of a semi-solid mixture of a margarate, and of a somewhat basic oleate of lead. This mixture is decomposed by hydrochloric acid, with the acid of heat. On allowing the liquid to cool, an odorous substance, of a shining crystalline fracture, solidifies on its surface, which is margaric acid, contaminated with traces of adhering oleïc acid. It is repeatedly dissolved in alcohol, in which it is the less soluble in proportion as it becomes purer; it is finally obtained entirely colorless; small crystals, which, at first, assemble around a common centre in the form of stars, but afterwards exhibiting masses of a cauliflower form, in which the crystalline structure is no longer recognised. Melted in a water-bath, this acid presents a fine white, brittle mass, shining like mother-of-pearl, the interior consisting of large, fine crystalline lamellæ.

On cooling, the boiling solution, containing an excess of alcohol, the margarate of soda crystallises, forming fine, small, silky needles, arranged in the form of stars. If the quantity of the alcohol employed be not sufficient to dissolve the whole amount of the salt, one portion of it, on cooling, is

* C=75, H=12·5.

* Liebig's *Annalen*, vol. xxxv, p. 196.

thrown down as a homogenous, gelatinous, opalescent, mass, which, on being shaken with a sufficient quantity of alcohol, is converted into a pulpy, crystalline, mass.

Margarate of silver, prepared by precipitation from a solution of margarate of soda in absolute alcohol, by an excess of nitrate of silver, is precipitated in the form of an opalescent, and very voluminous, gelatinous, mass, which being pressed between blotting-paper, and dried in a water-bath, is converted into a white mass, with a shade of rose red. On being heated in a crucible, it melts at first, but is afterwards decomposed; a large quantity of fumes is developed, condensing into a dark brown liquid, whereas metallic silver is left behind. The mean atomic weight of the anhydrous margaric acid was found to be 3354·70.

The analysis of the silver-salt gave the following results:—

0·3978 grammes substance gave 0·7900 grammes carbonic acid, 0·3100 grammes water, and 0·1183 grammes oxide of silver.

	In 100 parts.
Carbon.....	54·14
Hydrogen	8·64
Oxygen	7·84
Oxide of silver..	29·38
	100·00

These numbers correspond to the formula—

$$C^{34}\,H^{33}\,O^{3}+AgO.$$

This is the composition of margarate of silver.

The pure hydrated acid melts exactly at 140° F. The analysis yielded the following results:—0·1652 grammes substance gave 0·4591 grammes carbonic acid, and 0·1838 grammes water.

	In hundredths.
Carbon.....	75·78
Hydrogen.....	12·46
Oxygen.....	11·76
	100·00

To these numbers corresponds the formula—

$$C^{34}\,H^{34}\,O^{4},$$

or rather—

$$C^{34}\,H^{33}\,O^{3}+HO,$$

which is likewise the common one of hydrated margaric acid.

The identity of the margaric acid of linseed oil with that obtained from other fatty substances being already sufficiently proved by analysis, is established beyond doubt by examining the products of oxidation resulting from the action of nitric acid, of which we intend presently to speak.

An easy method of preparing margaric acid from linseed oil consists in saponifying the oil with a solution of caustic potassa; a soft, light-yellow soap is obtained, which, on separation by common salt, yields a solid soda soap, of a feeble but peculiar odor. It is allowed to drain, divided as much as possible while half-dry, and spread in thin layers and put in an airy place of a gentle temperature. The soap, containing an excess of alkali, absorbed oxygen from the air, became yellow and dry. After a lapse of two or three weeks it was thrown into an excess of a boiling solution of caustic potassa diluted with water. It was soon dissolved and the liquid became of a brown color, which was so dark that it appeared to be black. The soap was then separated by sea-salt, the greatest portion of the coloring matters being left behind in the mother liquor. It was sufficient to treat the soap twice or three times in the said manner to obtain it almost white. It was then decomposed by hydrochloric acid; margaric acid was collected on the surface of the cooled liquid, and was purified by repeated crystallisations from alcohol.

An excess of hydrochloric acid being put into the red alkaline mother liquors from which the margarate was separated, a brown unctuous resin was obtained, identical with that formed by the action of nitric acid on oleïc acid: it can only be a product of the oxidation of this acid; for we see that it disappears completely on the formation of this resin, which afterwards passes into the state of suberic acid.

The glycerin which is obtained in large quantities from linseed oil possesses the same properties as that furnished by other fatty bodies.

COMPOSITION OF LINSEED OIL.

My experiments have then proved that margaric and oleïc acids and glycerin are present in linseed oil. With reference to the latter substance, Prof. Liebig established the following formula:—$2C^{3}\,HO^{2}+4HO$, having considered as its radical the combination, $C^{3}\,H^{2}O$.

My experiments lead likewise to that formula as the only true one. In the sequel, therefore, we will always start from this formula.

By analysing the red linseed oil the following results were obtained:—

I. 0·2871 grammes substance gave 0·8218 grammes carbonic acid, and 0·2805 water.

II. 0·2604 grammes substance gave 0·7462 grammes carbonic acid, and 0·2601 grammes water.

	In hundredths. I.	II.	Mean.
Carbon....	78·05	78·18	78·11
Hydrogen..	10·83	11·09	10·96
Oxygen....	11·12	10·73	10·92

From these numbers may be deduced the

formula $C^{1056}\ H^{870}\ O^{108}$, which, in 100 parts, requires the following composition :—

Carbon.....	77·92
Hydrogen.....	10·58
Oxygen.....	11·50
	100·00

By adding 2 equiv. margaric acid, and 20 equiv. oleate of acroleine, considering the latter as composed of $C^3\ H^2\ O$, the following formula is obtained :—

$$2(C^{34}H^{33}O^3,\ C^3H^2O) = C^{74}\ H^{70}\ O^8$$
$$20(C^{46}H^{38}O^5,\ C^3H^2O) = C^{980}H^{800}O^{120}$$
$$C^{1054}H^{870}O^{128}$$

This formula represents 1 atom linseed oil + 20 atoms oxygen. This excess of oxygen can only be attributed to the eagerness with which oleïc acid absorbs this element, which is, indeed, so great, that on being left in an open bottle, after a few hours, the surface is converted into resin.

Considering this circumstance, it seems not improbable that oleïc acid exists in linseed oil, with the following formula :— $C^{46}\ H^{38}\ O^4$, which, in this case, would be the true expression for the anhydrous acid.

Though the developed formula most exactly expressed the composition of the linseed oil employed in my experiments, still it must not be forgotten that old linseed oil, or such as has been expressed in a hot state, may certainly be modified. —nt—

(To be concluded in our next.)

INVESTIGATIONS CONCERNING THE ORGANIC ALKALIS.*

BY CH. GERHARDT.

I HAD the honor, two years ago, to communicate to the Academy the results of some investigations concerning the organic alkalis. The principal object of that work was to fix the composition of quinine, cinchonine, strychnia, codeïn and piperin, whose formula required revision after the correction to which the atomic weight of carbon had been subjected. The most striking result of these researches was the conversion of quinine, cinchonine and strychnia, into a new liquid alkali, *quinoleïn*, whose composition will control that of the foregoing bodies.

Being persuaded that the examination of the products of decomposition is the most sure means of fixing the composition of organic substances, I have extended this kind of investigation to other alkaloïds, and principally to brucia.

* *Comptes Rendus*, No. 21., Nov. 18, 1844.

Chemists are aware of the decoloration which this body, when free or in combination with the acids, undergoes by the action of nitric acid ; this reaction, which is extremely delicate, enables very small quantities of this body to be detected in a liquid, so much so, that it is recommended in medico-legal investigations.

Notwithstanding the importance of this reaction, its chemical nature has remained unknown. I have devoted myself to a series of investigations in this respect, which have given me exceedingly curious results.

When nitric acid is poured on pure brucia, it is colored of a deep red, at the same time developing an odorous and inflammable gas : the mixture becomes greatly heated ; but if this be allowed to be accomplished without the aid of artificial heat, it does not disengage any trace of nitrous vapors or carbonic acid. The product forms a mass on cooling, and then presents an orange tint; water dissolves it, taking a red color ; alcohol dissolves very little when cold, and much more boiling, and abandons it by evaporation under the crystalline form. Ether dissolves scarcely any. It is by means of these solvents that it is obtained perfectly pure. The analysis of this body led to the formula—*

$$C^{21}\ H^{23}\ N^3\ O^8.$$

This body contains, in combination, the elements of nitrous vapor ; indeed, when it is heated, it explodes like gunpowder, and as it contains more nitrogen than brucia, it is evident that there is nitrous vapor.

$NO^2 = X$, replacing H, the red body is, therefore,

$$C^{21}\ (H^{23}X)\ N^3\ O^6,$$

derived from

$$C^{21}\ H^{24}\ N^2\ O^6.$$

But this is not the most curious part of the reaction. If we examine the gas which accompanies the red body, we find in it all the characters of nitrous ether ;† indeed, it is colorless, soluble in water, and very soluble in alcohol ; it possesses the very characteristic odor of rennet apples, and burns with a slight greenish-white flame, developing nitrous vapors ; as it is disengaged, it is not mixed with any trace of carbonic acid or oxide of nitrogen.

The unexpected appearance of nitrous ether led me to suppose, at first, that the formation of this body would be due to the

* C=75 ; H=6·25 ; N=87·5.

† These experiments were made during the heat of summer. It is known that nitrous ether boils at 16° 5 C (61° 3 F.). I likewise made counter-experiments with nitrous ether prepared expressly.

presence, in brucia, of alcohol of crystallisation. But a conclusive experiment proved to me that brucia really contains water of crystallisation, and that nitrous ether is developed just as well by completely dry brucia. Indeed, after having been fused in an oil-bath until all humidity is completely expelled, it disengages this ether in contact with nitric acid, in as large a quantity; it may be said that this mixture is a true source of nitrous ether, for the gaseous disengagement continues until the last portion of brucia has disappeared in the nitric acid.

This reaction is a very pretty experiment for the lecture table; it is as well to employ fused brucia, without powdering it; the attack of nitric acid is then less sudden, and the disengagement of nitrous ether is effected in a regular manner.

1 equivalent of brucia, and 2 equivalents of nitric acid, contain the elements of 1 equivalent of that body, and of 1 equivalent of nitrous ether:—

$$C^{23}\,H^{26}\,N^{2}\,O^{4} + 2NHO^{3} = C^{21}\,H^{23}\,N^{3}\,O^{8} + C^{2}\,H^{5}\,NO^{2}.$$

If the red body be left in nitric acid for a few hours, it is converted into a yellow body, insoluble in water, and which might be mistaken, at first sight, for chrome-yellow. This body also contains the nitrous elements, for it explodes the heat, like the red body.

Morphia is vividly attacked by nitric acid, but does not appear to develope nitrous ether.

NARCOTINE, AND THE PRODUCTS OF ITS OXIDATION.*

Dr. Blyth undertook, with the co-operation of Professor Liebig and Dr. A. W. Hoffmann, to determine the atomic weight of *narcotine*, the analyses of Liebig, Pelletier, and Regnault, and the formulæ deduced from them, differing very considerably. Dr. Blyth began by studying the sources of error attaching to the method of determining the atomic weight of narcotine by means of the double salts of platinum and narcotine. These sources of error are: first, when chloride of platinum and narcotine is washed too long with water, it is decomposed; secondly, bichloride of platinum, at a temperature of 212° F., or even below this, acts as an oxidating agent, and converts the narcotine into several new products. To avoid these errors, pure narcotine was dissolved in dilute hydrochloric acid, and to the cold solution, just so much bichloride of platinum was added as was sufficient to precipitate the double salt.

The organic alkalies, for the most part, form insoluble double salts with bichloride of platinum, which are exactly analogous in constitution to the bichloride of platinum and ammonia—that is, one equivalent of bichloride of platinum is united to one equivalent of the hydrochlorate of the base; so that, on burning the precipitate, the atomic weights of these bases may be readily calculated from the quantity of metallic platinum left. Proceeding in this manner, Dr. Blyth found the atomic weight of narcotine to be 5339·5 (oxygen being 100) and its formula, $C^{46}\,H^{25}\,NO^{14}$.

Cotarnin, one of the crystallisable bases formed by the oxidation of narcotine, on being analysed, was found to have the formula, $C^{25}\,H^{13}\,NO^{6}$; when anhydrous and crystallised, it contains, in addition, 2 aq. To this base the term cotarnin is applied. The crystallised hydrochlorate of cotarnin is represented by the formula $C^{25}\,H^{13}\,NO^{6}\,H\,Cl + 5$ aq.

Opianic acid, another product of the oxidation of narcotine, has the formula $C^{20}\,H^{9}\,O^{10}$, with the atomic weight 2567, and acts as an acid, forming *soluble* salts with metallic oxides.

Hemipinic acid, another product of the decomposition of narcotine, is an acid so called, which forms *insoluble* salts with the oxides of lead, silver, and peroxide of iron. Its formula is, $C^{10}\,H^{4}\,O^{5}$ with the addition of one equivalent of water of crystallisation.

Narcogenin, another base intermediate between narcotine and cotarnin, was found to be sometimes produced by the decomposition of narcotine. It forms, like other bases, double salts with chloride of platinum, but cannot be isolated. It requires further investigation.

On comparing the formula of *narcotine* deduced from the analysis with the products of its decomposition, their relations are seen to be very simple.

Thus, by employing a large excess of bichloride of platinum to effect the decomposition, the products obtained are, *cotarnin*, *opianic acid*, *carbonic acid*, *and water*. With these is usually found *hemipinic* acid, which is derived from the further oxidation of opianic acid.

1 equiv. of narcotine	=	$C^{46}\,H^{25}\,N\,O^{14}$
+ 7 equiv. of oxygen	=	O^{7}
		$C^{46}\,H^{25}\,N\,O^{21}$
Produce:		
1 equiv. of cotarnin	=	$C^{25}\,H^{13}\,N\,O^{6}$
1 equiv. of opianic acid	=	$C^{20}\,H^{9}\,O^{10}$
1 equiv. of carbonic acid	=	$C\,O^{2}$
3 equiv. of water	=	$H^{3}\,O^{3}$
		$C^{46}\,H^{25}\,N\,O^{21}$

C 2

* *Transactions of the Chemical Society.*

The further oxidation of opianic acid gives, from—

1 equi. anhydrous opianic acid	=	$C^{20} H^{8} O^{9}$
1 equi. of oxygen	=	O
2 equ. anhydrous hemipinic acid	=	$C^{20} H^{8} O^{10}$

To prove the disengagement of carbonic acid when narcotine is mixed with bichloride of platinum, the process was conducted in an atmosphere of hydrogen.

If only a very small excess of bichloride of platinum be employed, the decomposition of the narcotine does not proceed so far. The principal product, in such case, is platin chloride of narcogenin, with smaller quantities of opianic and hemipinic acid, and only a trace of platin chloride of cotarnin.

2 equiv. of narcotine	=	$C^{92} H^{60} N^{2} O^{28}$
+ 5 equiv. of oxygen	=	O^{5}
		$C^{92} H^{50} N^{2} O^{33}$
Produce:		
2 equiv. of narcogenin	=	$C^{72} H^{38} N^{2} O^{20}$
1 equiv. of opianic acid	=	$C^{20} H^{9} O^{10}$
3 equiv. of water	=	$H^{3} O^{3}$
		$C^{92} H^{50} N^{2} O^{32}$

Proceeding with the further oxidation of the narcogenin by excess of bichloride of platinum, the following is the result:—

2 equiv. of narcogenin	=	$C^{72} H^{38} N^{2} O^{20}$
+ 9 equiv. of oxygen		O^{9}
		$C^{72} H^{38} N^{2} O^{29}$
Produce:		
2 equiv. of cotarnin	=	$C^{50} H^{26} N^{2} O^{12}$
1 equiv. of opianic acid	=	$C^{20} H^{9} O^{10}$
2 equiv. of carbonic acid	=	$C^{2} O^{4}$
3 equiv. of water	=	$H^{3} O^{3}$
		$C^{72} H^{38} N^{2} O^{29}$

Dr. Blyth suggests that bichloride of platinum has most probably the same oxidising action on many other organic bases, which may account for the differences of their atomic weights when calculated from their platinum double salts. Upon morphia its action is very strongly marked, and Dr. Blyth promises to pursue its investigation.

A READY METHOD OF OBTAINING ASPARAGINE; REMARKS ON ITS FUNCTIONS IN THE ECONOMY VEGETABLES.*

M. PIRIA having had a crystalline substance sent to him from the country, said to have been obtained from a species of vetch (blanched), he was desirous of ascertaining its nature. In order to do this, he sowed about 10 lbs. of vetch seed in a close chamber, the floor of which was covered with vegetable mould; the seeds germinated in the absence of light, and the plants grew to some height. The plants were then gathered, their juice expressed and evaporated. A large amount of albumen first separated. The concentrated juice set aside deposited a mass of brown crystals; further evaporation increased their quantity to nearly eight ounces. Decolored by animal charcoal and repeated crystallisation, nearly five ounces of perfectly white large prismatic crystals were obtained, like those of sugar-candy.

Analysis proved this body to be identical with asparagine. It yielded—

Carbon	31·80
Hydrogen	6·85
Nitrogen	42·54
Oxygen	18·80

This, therefore, furnishes a ready and abundant source whence chemists may obtain that substance.

M. Piria then goes on to observe that several important questions relative to vegetable physiology are connected with this subject:—Whether the absence of light be indispensable to the production of asparagine. 2nd, Does asparagine pre-exist in the seed, or is it produced in the act of germination? 3rd, What part does it play in the economy of the plant? To answer questions, M. Piria treated some seed, and also some plants growing under the influence of light in the manner above detailed. He found no trace of asparagine in the seed, but abundance of it in the plants. Again he tried plants in the state of flowering and during fructification, but he could then obtain no asparagine; the plant contained, instead, a salt of lime, and an acid which he conceives to be new. The development of asparagine during germination, whether the plant be exposed to light, or excluded from its influence, proves that its elements are not derived from the atmosphere, but most probably from some azotized body (caseïn perhaps), existing in the seeds. This inquiry M. Piria has undertaken to pursue.

When asparagine is not perfectly pure, its solution undergoes a kind of fermentation, and in the course of a few days becomes converted into succinate of ammonia. This transformation is affected by the assumption of two atoms of hydrogen by the asparagine, which it derives from the putrefying matter, in a manner analogous to the transformation of blue indigo into white, under the reducing influence of organic matters undergoing putrefaction.

* *Comptes Rendus*, Sept. 16, 1844.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

IMPROVEMENTS IN THE MANUFACTURE OF ALUM.*

BY WILTON GEORGE TURNER.

THIS invention consists in the manufacture of alum from felspar rocks, or any other rocks or mineral substances containing silica and alumina, sufficiently free from other substances to be subjected to the process (which is performed by the aid of potash or of soda: when the former is used it is called potash alum; and when the latter, it is called soda alum). If desirous of making potash alum, the patentee proceeds by fusing felspar, or other rock or mineral substance containing silica and alumina, with such salts of potash as will yield, when fused at a red or white heat, a uniform glassy substance; this glassy substance, if sufficient potash has been used, is readily decomposed by water. The quantity of the salts of potash required will, in general, be about twice the weight of the mineral substance used, although, under certain circumstances, smaller quantities may answer. The aqueous solution contains silicate of potash, and the insoluble residuum is a light porous substance, similar in chemical composition to the mineral commonly called alæolite; this porous substance is decomposed by sulphuric acid, and forms alum by the separation of its silica. If the above process be performed with the salts of soda, instead of potash, the light porous substances obtained will be similar in chemical composition to the mineral known commonly as nepheline; and its decomposition by sulphuric acid will give rise to the formation of a soda alum.

The manner of carrying the invention into effect is as follows:—In order to make a potash alum, the best substance to operate upon is a potash felspar. This felspar is ground in a common edge-stone mill to the consistency of fine sand (a process which is much assisted by heating it to redness, and then plunging it in cold water); it is then mixed with its own weight of sulphate of potash, and placed in the upper part of the inclined bed of a reverberatory furnace, similar in construction to that known in the potteries as a frit-furnace, and which furnace has previously been brought to a full white heat. When, by the action of the heat, a glass has been produced, and is observed to flow down the inclined bed of the furnace, to such glass is to be added gradually, at the lower end of the furnace, as much carbonate of potash as was before used of sulphate of potash; and this process of placing the mixture of felspar and sulphate of potash at the upper part of the bed of the furnace, is to be repeated, adding, at the lower part of the bed, gradually and proportionately, as the glass flows down from the upper part, the carbonate of potash, as before mentioned: this is continued until the sack of the furnace is filled with the glass, which is then fit for the next process.

The preparation of the glass may also be effected in a reverberatory furnace with a flat bed, and the facility of removing it from such a furnace is an advantage. In this case no carbonate must be added to the mixture until the sulphate of potash is observed to be completely decomposed.

On boiling the glass in water, the same quantity of potash as was added to the felspar, and two-thirds of silica, contained in the felspar, are dissolved, while the remaining one-third of the silica. and the alumina, and an equal quantity of potash to that which the felspar originally contained, are left in the form of a light porous substance, similar in chemical composition to the mineral commonly called alæolite. This porous substance is carefully separated from the said solution, and washed with water until freed from the silicate of potash; it is then placed in an open leaden cistern or boiler, and boiled with dilute sulphuric acid of the specific gravity 1·2. This acid will contain about the quantity of water required for the solution and crystallisation of the alum produced by the decomposition of the alæolite: the quantity of the dilute sulphuric acid must be such as will contain about 160lbs. of dry sulphuric acid for every 285lbs. of felspar rock (if that rock be used), and in like proportion to the silica and alumina contained in the substance, if any other substance be used. As it is important that the alum solution, thus obtained, should not contain an excess of acid, the patentee recommends that only four-fifths of the proposed quantity of dilute sulphuric acid should be used in the first operation, which will leave a portion of the alæolite undecomposed; but, by acting upon this undecomposed portion, after the solution has been drawn off, with a full quantity of dilute acid to be used in the next operation, it will be completely

* *London Journal and Repertory of Arts, Sciences, and Manufactures*, for November.

decomposed, and the alum thus formed becomes part of the next batch: in this way a neutral solution of alum is obtained at each process. The boiling solution, after the sediment subsides, is drawn into coolers, such as are commonly used for the crystallisation of alum: about four-fifths of the alum held in solution will form into crystals. The mother liquor is then drawn off, leaving the alum, which is now ready to undergo the usual process of washing and roaching. The mother liquor, from the coolers, is boiled in a convenient boiler to dryness, in order to render the silica it contains insoluble; this residuum is boiled, either in water, or in the mother liquor, from the roaching tubs, so as to dissolve the alum it contains, and the process of crystallisation is repeated.

Had the above process been performed with the salts of soda, instead of potash, a soda alum would have been formed. For this purpose, the soda felspar, or arbite, should be selected. The potash or soda (as the case may be) contained in the liquor drawn, as aforesaid, from the alæolite or nepheline (which is formed when soda is used) may be recovered by either of the following processes:—The strong solutions, which are obtained about the specific gravity 1·2 are placed in any convenient vessel, in which a steam of carbonic acid gas, obtained by any convenient method, may be driven through them; the carbonic acid becomes absorbed, and the solution assumes the form of a gelatinous mass: this mass consists of carbonate of potash or soda, and hydrate of silica. On drying this mass in a furnace, which must never be allowed to rise to a red heat, even in the dark, the silica loses its water, and becomes insoluble; the potash or soda may then be separated from it, in the form of a sesquicarbonate of potash or soda by solution, and evaporation to dryness.

The other process, which, under most circumstances, will be found more economical and convenient, is to allow the boiling solution of silicate of potash or soda to filter through a bed of caustic lime, when it will be found that the lime as combined with the silica, and caustic potash or soda ley obtained:—This process may be conveniently conducted in an apparatus similar to that used by soap-makers for the preparation of their caustic leys. The potash or soda may then be readily obtained, as caustic potash or soda, or as a carbonate, by the ordinary processes. The weak solution of silicate of potash or soda are used to decompose another portion of the glassy substance.

The patentee claims the improvements aforesaid; and the production of substances similar to alæolite and nepheline artificially, by the decomposition, by water, of the glassy substances produced by the fusing of felspar, or other mineral substances containing silica and alumina, with salts of potash and soda; and the use and application of such artificial alæolite and nepheline, in the production and manufacture of alum, as aforesaid; he also claims the process, as above described, for separating the alkalies from silica, by means of caustic lime.

ON GILDING SILK FABRICS BY CHEMICAL MEANS.*

BY DR. BRETTHAUER.

Some years ago the Society for the Encouragement of Arts, at Berlin, offered a reward to any person who might succeed in gilding silken threads by chemical agency, in such a manner that the gilding should be solid (*i. e.* not liable to wear off easily), and the silk be not only not deteriorated, as regards its wear, but in a state fit to be manufactured into woven fabrics, in the same manner as metallic wires.

Although the experiments which I, as well as others, have made for this purpose completely failed, as far as regards the gilding of silk in threads, I have nevertheless succeeded in obtaining, upon silken stuffs, as well as upon other fabrics, a coat of gilding more or less solid, which is more brilliant in proportion to the glossiness and fineness of the fabric before the operation. As I have yet operated only upon woven fabrics, principally satin, I do not pretend to have succeeded in performing the operation above proposed, and the more so as I have hitherto only operated on a small scale, that is to say, on surfaces a yard square; but, thinking a communication of the means I have employed would serve to recal this subject to the minds of those who are interested therein, I have given the details of my first experiments, and the results thereof, which may have the effect of encouraging others to improve upon those means, which I have no doubt will, after many experiments, properly performed, end in complete success.

As regards the choice of the fabric to be gilt, the preference should be given to those of the most uniform texture and glossy appearance, for the reason above-mentioned. My experiments have hitherto been confined to satin, Gros de Naples, the finest linen fabrics, and lastly to paper, upon the gilding of which latter, I shall hereafter add

* *Bulletin du Musée de l'Industrie.*

some observations; but shall, for the present, confine myself to the process applicable to woven fabrics, dividing it into two parts: the first of which will treat of the dyeing of silk or other substances; and the second, of the manner of bringing out the golden or metallic lustre upon the surface of the fabric.

1.—DYEING OF THE SILK.

This operation is performed with an aqueous solution of chlorate of gold, which must not contain the least portion of free acid. A small quantity of free acid would not act directly in a destructive manner upon the silk, as it would upon linen fabrics; but it would be very injurious to the beauty of the gilding, as it seems to cause the gold to assume a purple hue, of which I shall treat presently.

A chlorate of gold, perfectly free from acid, is to be prepared in the following manner:—A quantity of gold (a ducat for example) is to be dissolved in *aqua regia*, which consists of a mixture of two parts of chlorhydric acid, and one part of nitric acid of commerce; the liquor is carefully decanted from the precipitated chlorate of silver, and evaporated to dryness at a gentle heat; the residuum is a chlorate of gold, containing no free acid. It is then to be dissolved again in pure water, and this solution is used for the dyeing process.

During this dissolution in water, especially when dried at too high a temperature, a solid body, consisting of metallic gold, is precipitated therefrom, which must be removed, in order to serve for a fresh operation. This separation of the metallic gold arises from the presence of a chlorate of gold, which, by means of the heat, is formed at the expense of the chloride, which loses a portion of its chlorine, and this chlorine, being again decomposed by water, forms a chloride with a deposit of metal. The chloride of gold, which is the highest degree of chloridation of the gold, is the one most fit to be employed for the operation in question.

The fabric is to be dyed in a hot solution, diluted with the necessary quantity of water. A cold liquor would not sufficiently penetrate the silk, and would adhere in too large a quantity to the surface, which would require to be expressed therefrom, and might, therefore, occasion loss. Besides, the salt of gold combines but very imperfectly with the silk in a cold state, so that the gilding produced therefrom, would rub off very easily; the liquor, therefore, only penetrates the silk uniformly at a boiling temperature.

A peculiar chemical phenomenon is here presented, which may be described in the following manner:—The silk, in fact, in the case, acts in the same manner as porous carbon. The peculiar properties of this body are well known, which are, to absorb not only different gases, and also a great quantity of coloring and other matters contained in liquids, but also to precipitate a sufficient quantity of salts from their aqueous solutions, and take them up; and as the silk acts in precisely the same manner, when it is in sufficient quantity, and has remained a sufficiently long time in contact with the boiling solution of gold, it absorbs all the gold and combines chemically therewith; so that the solution becomes colorless, and contains very slight traces of gold: a phenomenon which requires a more strict examination on the part of chemists.

In all my experiments, I have found a temperature of from 60° to 80° centigrade, to be sufficiently high for the dyeing liquor, only in that case the silk must be immersed a longer time therein. In this operation, it is better to raise the solution of gold to the boiling point, to plunge the whole fabric therein at once, let it boil there during a few seconds, and after taking it therefrom, wring or squeeze the liquor from it gently. The vessels employed must be of porcelain. The squeezing operation must be performed between plates of porcelain or glass, as these substances do not act injuriously upon, or absorb the salt of gold: neither metal, stone, nor wood, therefore, must be used for this purpose.

With regard to the degree of concentration of the solution, which serves as the dyeing liquor, this must be regulated according to the nature of the fabric to be dyed, as a weak solution only is required when a very close fabric is to be operated upon, whereas, if it is light, like satin for instance, it is necessary to use a stronger solution of salt of gold, in order to gild it properly. Before operating on a large scale, it will be necessary to determine, by experiment, the precise quantity of chloride of gold, or rather gold which will be required to gild a certain quantity or a certain surface of fabric.

The silken fabric having been prepared, as above described, by being dyed in the chloride of gold, must be kept sufficiently damp to allow of the operation of bringing out the golden lustre, *i. e.*, reducing the chloride to a metallic state. This operation might be performed, when in a dry state, by various means, such as sulphurous, phosphorous, or gallic acid, a solution of phosphorus, phosporetted hydrogen, &c., and even by the simple action of the solar rays; but in these latter cases the gold would not

possess much metallic lustre, and would have a brownish, bluish, or purplish hue; the silk dyed in chloride of gold assumes this color by simple dessication in the rays of the sun, or the light of day; silk possesses this property in common with other organic substances, such as feathers, &c. This purple coloring has been considered, by most chemists, to be a simple reduction of the chloride of gold; a supposition which is borne out by many facts. Under these circumstances the gold is seen in such minute particles, that it loses its metallic lustre, and assumes a reddish tint.

A great number of bodies, reduced to very thin or minute particles, assume a color very different to that which they present when seen in large quantities. I may mention, for instance, sulphate of copper, oxide of manganese, specular iron, sulphur in certain states, &c. Mercury, upon being reduced to minute particles, loses its metallic lustre, and assumes a greyish tint; many bodies, either in their natural state, or in chemical combination with other bodies, simply by a different molecular arrangement, assume different colors, as phosphorus, sulphate of mercury, &c.; and these various examples, and many others which might be cited, offer the presumption, that gold itself may, under certain circumstances, appear of a purple color. Besides, does not this metal assume this color when melted with porcelain enamel, in which case it is very minutely divided; and also as soon as the superficial and larger portions have been worn off by use?

This purple coloring of the gold upon the silk might be avoided, by plunging the fabric, before the dyeing process, into a solution which would neither affect the reduction of the chloride of gold, nor render the fabric impervious to the solution of gold. I have found that the chloride of gold, upon a ground of resin of varnish, and also upon glass or porcelain, when reduced in a cold state, as hereafter described, does not assume a red color; I have consequently tried a very weak solution of resin in alcohol, or of caoutchouc in an essential oil, but I have not succeeded in obtaining satisfactory results.

2.—REDUCTION OF THE COLORING MATTER OR METALLIC DYE.

There are several methods of reducing the chloride of gold, and giving a metallic lustre to the surface of the silk. I shall only treat of one of these methods, as it is that which operates the most energetically and advantageously, and which, if the nature of the fabric to be gilt is taken into consideration, will probably produce the desired effect. This method consists in employing phosphohydric gas. The fabric, which had been previously dyed with the chloride of gold, is brought, in a damp state, into an atmosphere highly charged with this gas. During the operation the fabric must be kept damp, and the gas allowed to disengage itself freely. Although the fabric must not be dry, it should not be so wet as to allow the water to run therefrom, as the pelico of gold would be carried away by the infiltration of the water, thus causing flaws and defects; the action of the gas must consequently be prolonged, as the operation takes effect first upon the surface of the fabric; and the coating of gold thus formed creates an obstacle to the action of the gas in the interior. The chloride of gold remaining in the fabric becomes ultimately, by the action of the light, of a purple or violet color, and thus injures the gilding. In order to avoid this, it is necessary, besides the phosphoretted hydrogen, which is present in sufficient quantity, to introduce steam into the chamber or box where the fabric is spread out in the manner most favorable to the operation, by which means the requisite degree of dampness is kept up.

Under the chamber is placed a vessel, of suitable size, with a large opening for the escape of the gas contained therein, and to prevent the introduction of any extraneous matters it may contain into the chamber; a plate of metal is placed at a short distance above the opening, and at one side of the chamber the necessary apparatus for injecting steam therein is fixed.

As soon as the disengagement of the gas commences, a slight metallic lustre appears upon the silk, which gradually augments until all the gold is brought out. If this development is moderated, metallic gold only is formed, as all the gas is decomposed, and phosphoric acid is formed, which remains in the fabric, together with free chlorohydric acid. These acids may be afterwards neutralised, by means of damp ammoniacal vapors, although they do not injure the silk in the least degree.

If the action of the gas is more powerful, a brown phosphate of gold is easily formed, which, as long as there remains any chloride of gold undecomposed, acts as an agent for its reduction. But as soon as the chloride of gold has disappeared, there remains some phosphate of gold not reduced, which tarnishes the gilding. Even by employing heat, which must always be done after the operation, these portions remain in a lustreless state, although the phosphate of gold is decomposed at the temperature at which the

operation of bringing out the lustre is effected.

Linen, woollen, and cotton fabrics are operated upon in exactly the same manner as the silken one, only they must be purified from all extraneous matters, such as coloring matters, gums, &c.

Paper and card-board may also be very well and easily gilt. In the first place a deep ground color must be given to them, with a red oxide of iron, and a slight coating of varnish applied, and the surface (well dried) covered with a small layer of chloride of gold in solution. The paper or card-board is afterwards submitted to the action of phosphohydric gas, until the layer is completely reduced; it is afterwards polished with a burnisher of agate or steel.

I have succeeded in obtaining good silvered card-board, by employing a nitrous solution of silver, and operating in precisely the same manner. It is necessary to polish in this case also; and, consequently, this will not do for fabrics, which only take a slight metallic lustre. When the season is favorable I shall try if these card-boards may be substituted for the expensive metallic plates of Daguerre.

I think that the details I have given will suffice to guide those who might be desirous of applying themselves practically to this process. It is unnecessary, I think, to describe the utensils required, as every person will know how to choose those which shall appear to be the most advantageous and convenient; and I shall conclude by some few observations upon the gas serving for the reduction of the solutions.

It is well known that there exist two sorts of phosphoretted hydrogen, the one spontaneously inflammable in the atmosphere, and the other not. These two combinations may nevertheless be considered as modifications of the same gas, as the difference which it was imagined existed in their composition arose only from the impurity of the gases. It is very doubtful whether, for operations upon a large scale, it would be advantageous to employ the gas not spontaneously inflammable, as the preparation of the hypophosphorous or phosphorous acid requisite for the production of this gas is difficult and expensive. I think, therefore, that it is advisable to use the inflammable gas, notwithstanding the loss occasioned by the combustion thereof, as its preparation is much less expensive than the other. Besides, all the gas disengaged does not enter into a state of combustion, but only a small quantity, especially when it is rapidly formed; from which it would appear that the two gases are, perhaps, produced simultaneously. Besides, the inflammable gas burns at the surface of the liquid, where the bubbles burst and form phosphoric acid, which has, consequently, no hurtful effect upon the silk; and even if any portions of this inflammable gas rise as far as the silk, they are decomposed with such rapidity, by the chloride of gold, that no combustion can take place.

The mode of preparing this gas is very simple:—It is only necessary, for this purpose, to have a solution of caustic potash of sufficient strength, which is prepared, as is well known, with the ordinary carbonate of potash and quick-lime, and a small quantity of phosphorus. The phosphorus melts on heating it gently in the caustic lye, decomposes the water, and oxidizes by combining with the oxygen to form hypophosphorus and phosphorus acids, which combine with the potash. Another portion of the phosphorus enters into the combination with the hydrogen of the water, and forms the phosphohydric gas required. A porcelain vessel, with a large mouth, is used for this purpose, and it is above this mouth or opening that a perforated metallic plate is placed, to prevent the effects of its too forcible entry.

The apparatus into which the gas is injected is not hermetically closed; there is, therefore, no fear of explosion.

METHOD OF ASCERTAINING THE QUANTITY OF CRYSTALLISABLE SUGAR CONTAINED IN BEET-ROOT, OR OTHER SACCHARINE SUBSTANCES.*

BY M. BARRESWILL.

This method is based on a peculiar property of sugar, noticed some years ago by M. Frommer, a German chemist. M. Frommer's method consists in adding to the saccharine solution to be tested a few drops of sulphate of copper, and afterwards of potassa, and heating the mixture to nearly a boiling temperature; the uncrystallisable sugar, or molasses, contained in the liquor reduces the salt of copper, and precipitates a red oxide of copper, while the crystallisable sugar contained in the cane does not change the properties of that salt.

M. Barreswill has turned this reaction to advantage, which was well known to chemists, and employed by them as a qualitative test for determining the relative quantities of crystallisable sugar and molasses, when these substances are found either alone,

* *Bulletin de la Société d'Encouragement.*

or in combination with other matters, in a solid body like raw sugar, or in a liquid, such as beet-root juice and cane juice. This process is founded on the following facts:—1st. That crystallisable sugar does not reduce the oxide of copper contained in an alkaline liquid. 2nd. That it will reduce this oxide when treated with sulphuric acid, which, on boiling for a few minutes, converts it entirely into uncrystallisable sugar or molasses. 3rd. That the quantity of deutoxide which is reduced is proportionate to the quantity of sugar employed. We now, in few words, describe M. Barreswill's process:—

If it be required to find the quantity of crystallisable sugar contained in a liquid, exclusive of other organic products, an alkaline solution of oxide of copper is to be prepared, by mixing sulphate of copper, neutral tartrate of potash, and caustic potash. A liquor of a deep blue color is thus obtained, which, on being filtered, remains clear and limpid for a long time. This solution is the *test liquor*, the power of which is to be determined by trying how much liquor, made with a certain weight of pure and dry sugar-candy, and raised to a boiling temperature, will suffice, after the addition of a few drops of sulphuric acid, to decolor perfectly a given quantity of the testing liquor.

The power of the test liquor having been carefully ascertained, a given quantity is to be poured into a capsule of porcelain or glass, and a suitable quantity of highly-concentrated caustic potash is to be added. The only object of this is to augment the density of the liquid, and to facilitate the ultimate precipitation of the oxide of copper. Then, by means of a vessel with divisions marked thereon, the saccharine and acid liquor to be tested, is, after being diluted with a given quantity of water, poured, drop by drop, into the hot solution of oxide of copper. As soon as the two liquors come in contact, a yellow precipitate of hydrate of copper is formed, which becomes red, and falls to the bottom of the vessel, on reaching the temperature of the medium in which it was formed. As the operation proceeds, the color of the liquid becomes less deep, and at the same time the copper is precipitated in the form of protoxide; the operation is complete as soon as the liquid becomes completely colorless. Then, on reckoning upon the graduated vessel the number of divisions which have been employed for arriving at this result, the weight of the sugar contained in the liquor submitted to the experiment may be ascertained.

The essential and delicate part of the operation is to ascertain the exact time when the precipitation of the oxide of copper is complete: this is done by observing the time of decoloration of the liquor, if the saccharine liquor itself be colorless, and also by the cessation of the formation of the opaque yellow precipitate which precedes the deposit of the oxide of copper. This latter will only be observed when the substance to be tested is colored.

An excess of sugar added to the test liquor, after the complete separation of the oxide of copper, gives the well-known brown color, resulting from the reaction of hydrated alkalies upon the uncrystallisable sugar.

If the saccharine liquor to be tested contains crystallisable sugar and molasses, the proportion of this latter substance is ascertained by making a previous experiment with a given portion of the liquid, before submitting it to the action of sulphuric acid; the molasses reduces the copper solution which the crystallisable sugar does not act upon. Another portion of the saccharine liquid is afterwards boiled with the sulphuric acid; so as to convert all the crystallisable sugar into molasses; by means of a second operation, with the liquor thus modified, the total weight of the molasses contained therein is ascertained; and, on deducting therefrom that of the molasses which it contained before (this having been furnished by the first operation) the difference will shew the quantity of crystallisable sugar contained in the mixture of water, ordinary sugar, and molasses.

M. Barreswill's process is distinguished, as will be seen, by great simplicity; it has been submitted by a committee of the chemical arts to strict trial; we have ascertained, that when a liquid contains only crystallisable sugar, its proportion may, in a quarter of an hour, be ascertained to the extent of two or three per cent. It may besides be ascertained, by previous experiment, if the liquor contains the least trace of molasses. When this latter substance is combined with the sugar, as is the case with cane or beet-root juice kept in the air for some time, or in fraudulent mixtures of brown sugar and granulated molasses, the operation is somewhat less correct.

Having shown the advantages of M. Barreswill's method, we must now speak of its disadvantages. The principal one is, that it is only applicable to pure solutions of sugar, or a mixture of sugar with molasses. In fact, if the substance to be analysed contains tartaric acid, dextrine, sugar of milk, &c., these substances act nearly in the same manner as crystallisable sugar, and might, consequently, be confounded with it; on the other hand, there exist, doubtless, some organic matters which have the effect of

reducing the alkaline solution of oxide of copper, in the same manner as molasses: so that this process can only be employed with certainty when it shall have been proved by previous experiment, that no other organic matter exist in combination with the sugar or molasses, or that they have been separated therefrom by suitable means.

Notwithstanding these disadvantages, M. Barreswill's process is capable of rendering great service to science and manufacture; but there is no doubt that it may be greatly improved by chemists who may make use of it. The committee have accordingly awarded to M. Barreswill a silver medal, and a sum of 1,000 francs out of the 3,000 francs. which were offered to the inventor of a method more perfect, and of more general application. It is also decided that the question should be left open for further improvements until the meeting in 1845, and the reward reduced to 2,000 francs.

CHEMICAL INVESTIGATIONS CONCERNING THE MATURATION OF FRUITS.*

BY M. FRÉMY.

When the Academy of Sciences awarded, in 1821, a prize to M. Berard, and an honorable mention to M. Couvershal, for their memoir on the maturation of fruits, while rendering justice to the importance of our labors, it expressed a wish that their experiments might be continued and extended.

I have thought, therefore, that the Academy would receive with indulgence a new work on the maturation of fruits, and that my haste to submit to it my first results would prove to it my desire to follow the direction which it had long since given to chemists.

Before making known the composition which fruits present at the different periods of their growth and of their maturation, I thought it right first to discuss some general questions connected with the phenomena of maturation: it is this first part of my investigations which I have now the honor of presenting.

I wished in the first place to determine the influence which the elements of the air may exert on the development of the fruits, my experiments have principally related to fruits with fleshy pericarps (*pericarpes charnus*). With regard to the respiration of fruits, I have made a great number of experiments, for whose details there is not space here; I will only say that, in operating on fruits detached from the tree, I have always ascertained that fruits placed in atmospheric air rapidly converted the oxygen into carbonic acid. My experiments, therefore, completely confirm this important part of M. Berard's work.

It was known that this skilful chemist, with the view of proving that oxygen is indispensable to maturation, introduced fruits into flasks filled with nitrogen, hydrogen, and carbonic acid. I have thought this experiment difficult to execute on a fruit attached to the tree, frequently had the inconvenience of altering the pericarp, and of complicating the phenomenon by surrounding the fruit with a foreign gas, which might exert a direct action on it.

In order to protect the fruit from the influence of atmospheric air, leaving it in nearly normal conditions, I applied to their surface layers of gum and of resinous varnish.

My experiments were made on pears, peaches, and gooseberries. They resulted in proving that the development of the fruit is always arrested at the moment when it is covered with varnish.

Must it be admitted, with M. Berard, that, in the maturation of fruit, the production of carbonic acid is the essential phenomenon, and that it is the ligneous matter which, in losing carbon, is converted into sugar?

I know not, I own, any fact capable of confirming this theory.

By confining fruit in flasks, or by covering them with varnish, their transpiration is necessarily arrested, which, according to the opinion of the most distinguished botanists, exert a certain influence on the circulation which is carried on in their interior.

The only important consequence to be drawn from the foregoing experiment, is that the respiration and transpiration of fruit are two functions indispensable to their development.

In order to neglect none of the questions connected with the respiration of fruits, it appeared to me useful to determine the nature of the gases which are found in fruit; with this view, I employed a glass phial, to which a disengagement tube was adapted; the phial and the tube, at the time of the experiment, were entirely filled with water saturated with common salt.

I then introduced into the phial fragments of fruit; by boiling the liquor, the gas was immediately disengaged; it was then easy to determine its volume and composition. I submitted to this kind of experiment a very great number of fruit. I will here quote some of these analyses:—

* *Comptes Rendus.*

Air of a ripe apple.		Of a green apple.		Of a green apple.	
I. Carbonic acid	56	Carbonic acid	56	Carbonic acid	56
Nitrogen....	44	Nitrogen....	39	Nitrogen....	59
	100	Oxygen	5	Oxygen	10
			100		100
II. Carbonic acid	56				
Nitrogen....	44				
	100				

Air of green quinces.		Of a ripe pear.		Of an unripe pear.	
Carbonic acid....	70	Carbonic acid....	68	Carbonic acid....	68
Nitrogen.....	26	Nitrogen	30	Nitrogen.....	27
Oxygen.....	4	Oxygen	2	Oxygen	5
	100		100		100

Air of black grapes.		Of black grapes.		Of green grapes.	
Carbonic acid....	93	Carbonic acid....	90	Carbonic acid....	95
Nitrogen.....	7	Nitrogen	10	Nitrogen	5
	100		100		100

These results seem to confirm the observations which have been made concerning the respiration of fruits. It is generally admitted that an unripe fruit disengages oxygen under the solar influence, decomposing the carbonic acid; whilst ripe fruit, as M. Berard has proved, convert, on the contrary, the oxygen of the air into carbonic acid. Now, it results from my analysis that unripe fruits contain more oxygen than ripe fruits.

The quantity of gas disengaged in the foregoing experiments never exceeded half the volume of the fruit. Perceiving that a fruit converts, in a few hours, the oxygen of the air into carbonic acid, it was natural to ascertain whether this transformation was effected under the influence of a kind of ferment pre-existing in the fruit, or whether it depended solely on the organisation of the pericarp.

In order to ascertain this, I introduced into a bell-glass filled with atmospheric air, a pear which, during several days, had formed carbonic acid at the expense of the oxygen of the air; I then bruised the pericarp in such a manner as to rupture all the celules, and I observed, from this time, that the production of carbonic acid was completely suspended. The sugar of the fruit afterwards entered into fermentation.

It will be remembered that a similar experiment was made on leaves by Theodore de Saussure, and that that celebrated chemist ascertained that they decomposed carbonic acid only when their organisation is not destroyed.

The phenomenon of the respiration of fruits is not, therefore, so simple as might be supposed; it must necessarily be admitted that besides the chemical reactions, which are produced during maturation, there exist others which evidently depend on the organisation of vegetables.

It is this important point which I wish to establish in an accurate manner. I will not here dwell on the experiments which I have made with the view of discovering in fruits a principle of the nature of ferments which might, by acting on the sugar, pectine, or acids of fruits, convert the oxygen into carbonic acid. My endeavors have, hitherto, been ineffectual.

Some chemists have stated that the acids contained in fruits were modified by the act of vegetation, and that malic acid might be turned to citric or tartaric.

This opinion, which is far from being admitted, is, I think, founded on inaccurate experiments, in which it was thought that malic, or tartaric acid, was artificially reproduced.

In order to treat this question I followed the development of a fruit, whose acid could be easily characterised. My experiments were made on the grape.

In examining grape-seeds at different periods of their maturation, I ascertained that tartaric acid, which exists in such large proportion in the ripe fruit, in the state of bitartrate of potassa, is also found in an appreciable quantity in a seed which only weighs 5 millogrammes. I think, therefore, that I may state, that tartaric acid is not a product of the modification of another organic acid, and that it exists in the smallest grape-seeds. M. Pelouze arrived at the same results.

It is known, that at a certain period the acid and the astringent taste of fruits is replaced by a sweet and agreeable taste.

The production of sugar in fruits has been variously explained by chemists.

According to M. Couvershel, the sugar of fruits would be formed by the action of organic acids on the gum, dextrine, or starch, found in fruits, or their peduncles.

A serious objection may be made to this theory, for M. Bineau has demonstrated that the sugar which is produced in the reaction of acids on starch, does not exert on polarised light the same action as that which is found in the grape. According to this celebrated natural philosopher, these two sugars would not be identical.

Other chemists have admitted that it is the ligneous matter of fruit which is converted into sugar; I should say that I have found it impossible to obtain sugar by boiling, for a long time, with concentrated acids the ligneous matter contained in the pericarp of fruit.

It is evident, therefore, that this question is not yet solved, and that it requires new experiments.

In order to appreciate the part which the acids perform in maturation, and influence which they exert on the production of sugar, I endeavored to saturate, by an alkaline solution during vegetation, the acid which fruit contains, and afterwards to analyse it at the period of maturation.

I ascertained that certain fruits, during the first period of their growth, contain only insignificant quantities of sugar, and that, at a certain period, which is indicated by physical changes with which every one is acquainted, sugar is developed in abundance; this is the case with prunes and apricots, I therefore thought that by introducing into such fruits alkaline solutions at the period at which they ripen, I might, perhaps, appreciate the influence of acids on their maturation.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

ON THE ROOT OF COHATE, A NEW DIURETIC MEDICINE.*

BY M. ARNOZAN.

THE rhizome of cohate, furnished by a vegetable of the family of the graminaceæ, presents, like the sucking of monocotyledanous plants, approximated and converging nodiform divisions: it sometimes presents remains of radical leaves, thick and fleshy fibres; its external color, of a reddish brown, seems to denote that it must grow in a ferruginous soil. The plant itself reaches nearly to the height of a man, and is sometimes in advance of the bark of thick woods.

Masticated and held for some time in the mouth, this root, at first insipid, manifests an aromatic taste, which arises from its bark. M. Arnozan had an opportunity of examining old and recent roots, and both qualities presented the same characters.

In examining this sample of cohate in a qualitative point of view only, this pharmacien found in it gum or mucous matter, starch, an insipid waxy substance, soluble only in alcohol, and a resinoïd matter, possessing the same color as the root, and an evidently aromatic taste; it is the latter substance which he considers as the native principle of the root.

In comparatively examining the results of different treatments by water, with and without heat, by alcohol, and by ether, the decoction alone seemed to combine all the qualities indispensable to an efficacious medicine, as regards both the possibility of being administered easily, and the advantage of being charged with all the active principles of the root. It is, moreover, the popular mode followed for the employment of this remedy in Havannah, where the decoction is taken as a draught.

According to the statement of several persons who have witnessed the use of this root in the island of Cuba, cohate enjoys diuretic properties, which seem to be more prominent in it than they ordinarily are in the roots of other vegetables of the same family; it is also highly extolled as a remedial agent in œdematous affection; it is even regarded as the only remedy which can be efficaciously employed for the negroes coming from the coast of Africa, who are, for the most part, dropsical at the time of their arrival; and its employment is so general, especially in the habitations, that the colonies are constantly provided with it, and cultivate it on purpose.

M. Arnozan mentions that, some years ago, a woman who was engaged, near Bordeaux, in the cultivation and collection of medicinal plants for pharmaceutical purposes, received a certain quantity of this recent root

* *Journal de Médicine de Bordeaux.*

in order to try to cultivate it. The attempt was not successful; but the woman, who knew the therapeutical properties of cobate, administered a draught of it to a dropsical person, who experienced great benefit from it. This patient continued to use it for three years, and always with evident advantage.

Since the communication made by M. Arnozan to the *Société de Médicine de Bordeaux* on the properties of cohate, Dr. Pereyra has given a draught prepared with 2 grammes of this plant to 2 quarts of water reduced to half, to a man attacked by disease of the heart, and presenting a very intense leucophymasia, and the diuretic effect of the new medicine was powerful.

ON A NEW REMEDY FOR PSORIASIS.*

BY JOSHUA WADDINGTON, ESQ.

WITH the view of rendering as public as possible the easy means I have for some time past adopted of speedily subduing a very common, and, if treated improperly, a very dangerous disease, I avail myself of the extensive circulation of the *Lancet* to state, that in the treatment of psoriasis diffusa, or palmaria, the best application is the ol. palm. cocc. (cocoa-nut oil), diluted with equal portions of ung. cectacei. The affected parts should be washed every night and morning with tepid *rain* water, then made quite dry, and the ointment applied lightly (with a camel's-hair brush); over this, oiled-silk should constantly be worn.

I need scarcely remark, that pilul. hydr. sub. comp. (Plummer's pill), gr. x., each night at bed-time, with liq. potassæ, m. xx., three times a day, are among the best *internal* means of treating psoriasis diffusa, joined with great care as regards regimen: no salted meats, no shell-fish, no fermented liquors, no acids; the less fruit and vegetables the better.

After twenty-eight years' extensive practice at the sea-side, I venture to add, that warm or cold sea-bathing is not generally advisable in cases of psoriasis diffusa, and have often been attended with dangerous results.

At some future period, when professional avocations are fewer and less pressing, I intend to submit to the public a more particular account, both of my theory and practice, as regards psoriasis and other eruptive complaints, accompanied with a schedule of the cases I have treated.

Marine-terrace, Margate,
November 22, 1844.

* *Lancet*, Dec. 7.

ON THE EMPLOYMENT OF FERRUGINOUS SALTS, AND ON THE CARBONATE OF PROTOXIDE OF IRON IN PARTICULAR.*

BY ALPHONSE MEILLET.

M. MEILLET first discusses the relative value, in a therapeutical point of view, of soluble and insoluble preparations of iron, and reproaches the latter with numerous inconveniences.

With regard to the soluble preparations of the protoxide, such as the lactate and sulphate, he finds their atramentary task so insupportable, that they must be administered only in pills. The lactate is, in his opinion, the most convenient; however, he prefers to it the white carbonate of protoxide of iron, which alone appears to him to unite all the conditions of success.

The following are the reasons on which he founds his preference:—

In the first place, says he, as in administering this salt only very small doses are employed (1 to 2 decigrammes), there is no fear of fatiguing the stomach of the patient; again, instead of adding acid to the stomach, it neutralises them as easily and as completely as carbonate of magnesia; so that for bad digestion it is an absorbent and a tonic, two properties which are not united in any other medicament. The free acids which are found in the stomach promptly dissolve it, and form the salts which are best adapted for immediate absorption, for who can know better than nature itself what suits it best? This is only a medicine which is presented to the stomach that it may itself prepare and elaborate it; it is not forced to take, as with the soluble preparations, the salts which do not agree with it: with the white carbonate, it forms them itself, and if there be a slight excess, it is rejected without fatigue or pain, owing to the very minute quantity employed. These considerations have determined us to seek some easy means of manufacturing this compound, which is so difficult to prepare, owing to its great tendency to lose its carbonic acid in the air, and to be converted into hydrated peroxide—an alteration easily recognised by the rusty color of the precipitate. I have discovered a prompt method, but which requires a certain skill and apparatus *ad hoc*.

Thus into a solution of very pure sulphate of iron I pour a corresponding atomic quantity of carbonate of soda in solution; a gelatinous mixture of proto-carbonic acid and green hydrated protoxide of iron, with water,

* *Journal de Pharmacie et de Chimie*, *Dec.*, 1844.

is precipitated. I wash it several times out of contact with the air, which I accomplish by driving carbonic acid into the apparatus replacing the water as it goes out. I afterwards subject it to a pressure of several atmospheres, in the midst of this gas, and the precipitate, being saturated with carbonic acid, whitens, becomes heavy and dense, and falls to the bottom of the liquid completely carbonated. I decant it, and then mix it with a suitable excipient for making pills or lozenges, which have the great advantage of being completely tasteless.

CARBONATE OF AMMONIA IN THE PASTE USED FOR MAKING BISCUITS.*

M. Wuaflart, pharmacien at Paris, made known the following fact. Having been under the necessity of adding calomel to the paste intended to be converted into biscuits, the fashion in Rheims, he recognised that these biscuits had acquired a blackish tint, very disagreeable to the eye. In seeking for the cause of this coloration, he ascertained that it was due to carbonate of ammonia having been added to the paste, in order to make it rise.

The observation made by M. Wuaflart, it appears to me, may be turned to account in some cases. A. Chevallier.

EXTRACT FROM A NOTE ON LACTUCARIUM.†

BY M. AUBERGIER.

M. Aubergier having ascertained that alcohol at 21° easily separates the active principles of lactucarium from the wax, resin, and other inert matters which accompany it, proposes to prepare an extract and syrup of lactucarium in the following manner:—

Lactucarium is exhausted by being twice heated with alcohol at 21°; the liquors are distilled, and the evaporation is carried on in a sand-bath, with continued stirring. The extract thus obtained is brown, bitter, and deliquescent. M. Aubergier recommends its use for the preparation of pills and syrup of lactucarium.

℞. Alcoholic extract of lactucarium, 1 part
Simple syrup ..... 50 ,,

Dissolve the alcoholic extract in a sufficient quantity of boiling syrup. Strain the solution while warm, and immediately add to the boiling syrup. The syrup is rendered turbid by cooling, but it does not deposit.

* *Journal de Chimie Medicale*, December, 1844.

† *Journal de Chimie Medicale.*

FORMULA FOR BOCHET'S IODURETTED SYRUP.*

The formula of this syrup, which is very much in use at Lyon, has been communicated to us by M. Bertrand, pharmacien of that city. It is the following:—

℞. Sarsaparilla.. } *aa* 1 kil.
Sassafras....
Guaiacum ..
Squills ....
Senna .....

Two decoctions are made in a sufficient quantity of water; they are mixed and reduced to 8 kilogrs.,; 5 kil. of sugar and 5 kil. of honey are added; it is clarified with white of egg, and a syrup is made at 28°; it is heated after cooling, and 125 grammes of the tincture of iodine of the codex is added.

This syrup contains per 100 grammes, 1 gramme of the tincture. It is advantageously employed in swellings of the glands, especially of the thyroid gland, dachitis, scrofulous affections, degenerated venereal diseases and gout. A great number of cures have proved its efficacy.

ON THE MIXTURE OF SULPHATE OF QUININE WITH SALICINE, AND ON A PROMPT MEANS OF DETECTING THE FRAUD.†

BY M. PELTIER.

Salicine is admirably adapted for the adulteration of sulphate of quinine, notwithstanding its crystallisation is not the same. When sulphate of quinine is mixed with half, or only one-fourth of salicine, the reaction of concentrated sulphuric acid is sufficiently marked for the fraud to be distinguished. If there be only $\frac{1}{10}$ of the former in the mixture, sulphuric acid does not assume the red color, which so well characterises pure salicine; in this case, the liquid resembles sulphuric acid soiled by some portions of vegetable matter, which it has carbonised. To be able to speak decisively, it is necessary to isolate the salicine, and to obtain, in all its purity, the reaction indicated.

The following is the process which I followed for isolating, in a few minutes, several centigrammes of salicine mixed with sulphate of quinine, in the proportion of $\frac{1}{10}$:—

I poured on 2 decigrammes of adulterated

* *Journal de Pharmacie*, December, 1844.

† *Journal de Chimie Médicale*, December, 1844.

quinine, 12 decigrammes of concentrated sulphuric acid; the salt was dissolved, and colored brown. I added 25 grammes of distilled water; the brown color disappeared, and the salicine remained white and suspended in the liquid. The alkaloïd of saule was not dissolved by this acid solution of sulphate of quinine. I filtered and collected on a watch glass a white and very bitter powder, which gave, with cold concentrated sulphuric acid, the vivid red reaction.

If from 50 to 60 grammes of water be added, the liquid remains heavy, without any appearance of precipitate; but, after some time, a precipitate is formed which has the gelatinous appearance of a precipitate of hydrate of alumina. It is much more difficult to collect; it is, therefore, important to add the water by small quantities, and to cease from adding it when a precipitate is obtained which separates with facility.

DETECTION OF OPIUM IN THE STOMACH SEVERAL DAYS AFTER DEATH.*

BY H. LETHEBY, M.B., LECTURER ON CHEMISTRY AT THE LONDON HOSPITAL.

SAMUEL C——, aged fifty-six, of previously good health, died somewhat suddenly on the 17th of last September. There were some suspicions of his having been poisoned, and I received a notice from the coroner to be present at the disinterment of his body. He had been buried twelve days. The corpse was considerably decomposed, of a dark, livid, green color, and the abdomen distended with gas almost to bursting. The chest and its contents were perfectly normal and free from disease, but on opening the abdomen it was observed, that the whole course of the alimentary canal was highly vascular and congested, putting on a deep purple hue. The stomach contained about one ounce and a half of a thick, dirty, yellow fluid, but the intestines were quite empty, and, except at one small spot, free from ulceration.

A part of the contents of the stomach was boiled with alcohol, and then filtered; it gave a deep red solution, which possessed the property of staining paper of a bright yellow color, and this was not heightened by an alkali, while nitric acid changed it to a purplish green. Acetate of lead was added to the rest of the solution, and a copious precipitate obtained. The filtered liquid, when freed from lead by sulphuretted hydrogen, and evaporated nearly to dryness, gave with chloride of iron a greenish tint, and with nitric acid a bright red—two results indicative of morphia.

The precipitate was then diffused through water, and deprived of its lead in a similar manner, and evaporated until it was nearly dry, this gave with chloride of iron a blood-red color, which was destroyed by nitric acid, potash, and protochloride of tin, but not by bichloride of mercury. It gave no color with terchloride of gold, but white precipitates with lead and baryta. From these I inferred the presence of meconic acid, and my former tests indicated saffron; other reagents proved the presence of carbonate of lime, but I could detect no mineral poison in the stomach. The subsequent examination of the liver, however, gave indications of arsenic.

It came out, in the course of evidence, that the deceased had died with symptoms of dysentery, and that aromatic confection, with opium and chalk, had been administered.

The chief feature of the case is, that the constituents of opium may be detected some time after death. It is also a point of consideration that the inflammatory condition of the small intestines, together with the vomiting and purging, and the absence of any considerable ulceration, and the sudden coming on of the attack, are indicative of something more than dysentery. There can be no doubt of his having died from extensive inflammation of the intestines, and in all probability the cause of this inflammation would have escaped notice but for the examination of the tissues.

Tredegar-square, Nov., 1844.

* *Lancet*, Dec. 7, 1844.

THE LATE CASES OF POISONING BY PRUSSIC ACID.*

BY H. LETHEBY, M.B., PROFESSOR OF CHEMISTRY, LONDON HOSPITAL.

ON Monday, Nov. 25, I was requested by the coroner to make a *post mortem* examination of the bodies of C. W. Duckett and Elizabeth Williams, both of whom had died suddenly during the night, having previously been in good health; the former was aged 21, the latter 27.

I found them lying upon the bed, with their clothes on, and in such a natural position that at first sight any one would have supposed that they were merely sleeping. He lay upon his back, with his right arm under and around the waist of Miss Williams, the fingers being a little flexed, as if they had

* *Lancet*.

suffered from a very slight spasm at the time of death; the other hand lay open and composed upon the left thigh. His features, too, wore the calm, smiling expression of a person who had died without the least agony or convulsion.

Miss Williams exhibited the same placidity of countenance; she lay on her left side, with her head resting upon one hand, while the other was thrown over the body of her lover, as if in an affectionate embrace; her legs were extended, and a little crossed, in the common position of one asleep, the dress covering them to the feet; nor were her clothes anywhere disarranged or tossed, although this must have happened if she had struggled in the act of dying. On raising her head, I observed that some colorless fluid, probably saliva, had trickled from her mouth, and this, as well as her lips, smelt strongly of prussic acid. The lungs of both were found to be quite free from disease, but so completely gorged with blood, especially upon the most dependent parts, that they looked nearly black, and felt heavy and solid.

The right side of the heart was filled with the same dark blood, while the left was nearly empty; indeed, everywhere the blood had an unusually dark color, was uncoagulable, and had a strong odor of the poison.

The abdomen presented nothing abnormal. The stomach of each contained about a pint of thick fluid, like soup, having the smell of hydrocyanic acid, and when examined internally, they were found to be a little redder than usual, except at one or two places, where they had lost color, and assumed a yellowish white tint, through which patches of red points appeared, as if blood had been effused in minute dots.

On distilling the contents of the stomach with a little dilute sulphuric acid, and then precipitating with nitrate of silver, I obtained 47 grains of cyanide of silver, known to be such by its perfect solubility in boiling nitric acid; this indicates about one ounce of Pharmacopœial prussic acid.

In the evidence it appeared that the deceased gentleman had purchased two ounces of prussic acid, and no doubt they drank it simultaneously while they sat upon the bed, for two tea-cups were found close by them.

Remarks.—It appears to me that there are several points of most especial interest connected with these two cases. Thus—the parties were both young, in good health, and they must have taken each about one ounce of the poison, and have died immediately, without the least struggle or convulsion. Now, out of the great number of experiments which I have made upon animals, I do not remember ever to have seen death produced, even by the most potent dose of prussic acid, without some convulsion, and generally there is a very great deal; I must confess, therefore, that if there had been anything about this, or any other such case, which could by a possibility have attached suspicion to a third party, I should most assuredly, though, perhaps, most unjustly, have given my evidence against that party; for the conclusions drawn from my experiments would have pronounced it perfectly impossible for death to have resulted from prussic acid without its leaving some indications of struggling; and I should, no doubt, have believed, from the calm and apparently studied positions of these bodies, that all such indications had been designedly removed. I do say, then, that cases of this kind should teach us caution in giving evidence, however well our opinions may be grounded, or from whatever source they may be drawn. Another interesting point, with reference to the action of the poison, is, that the blood assumed a dark color, was uncoagulable, and had the odor of the acid. The lungs, too, were excessively congested, as was also the right side of the heart; all of which indicates that the immediate cause of death is, the stoppage to respiration and the accumulation of blood in the lungs. This cannot be dependent on a deficiency of the heart's action, for every one who has been accustomed to these inquiries knows that the heart continues to beat for some time after all the evidences of death have set in; and this would be suggested, too, by the empty condition of its left side. I have no doubt that the immediate action of hydrocyanic acid is upon the nervous centres, and through them the true and semi-voluntary muscles become fixed or tetanic, and respiration is in this way prevented. We hear the expiratory shriek; see the spasm of the limbs, and the evacuation of the stomach, rectum, and bladder.

In all three of the cases of poisoning by prussic acid which have recently come under my notice, I have observed the peculiar condition of the stomach, which I have attempted to describe—namely, the partially congested state of its general internal surface, with the occasional white patches and red dots. The last fact to which I would refer is, that the smell of the poison was very evident about the mouth, although both these bodies must have been dead at least twelve hours when I examined them.

Tredegar-square.

NOTE FROM MR. G. DAVIES.

Sir—In the last number of *The Lancet*, Dr. Letheby, in his contribution relative to the late cases of poisoning, directs attention

to the fact, "that the smell of the poison was very evident about the mouth, although both these bodies must have been dead at least *twelve* hours when he examined them;" which from the following circumstances must have been seventeen or eighteen hours, as I believe he saw them about one o'clock on Monday afternoon. I saw them between one and two on Monday morning; that of the young man was in the posture described by Dr. Letheby; his countenance perfectly natural, having known him several years; the lady was lying on her side, with her head upon her lover's breast, her ringlets flowing over his chest and shoulders, her arm round his waist; upon taking hold of her wrist, I found it quite cold, and the arm so rigid that upon raising it her head slipped into the situation found by Dr. Letheby. It was then that a quantity of tenacious fluid flowed from her mouth, strongly impregnated with the odor of hydrocyanic acid, and I observed to the policeman who entered at the same time that I did, that they had taken prussic acid, and had been dead several hours. On the Sunday evening, the parents of the young gentleman returned from church between eight and nine o'clock; there can be no doubt that the fatal act was committed during their absence, as had it occurred after that time the characteristic shriek must have been heard, the deceased persons being in the back parlor.

Allow me to observe, in conclusion, that the very false statement respecting me in an anonymous letter that appeared in a recent number of *The Lancet*, and also one in the *Morning Advertiser*, was the fabrication of a neighboring practitioner, whose "treasury" protects him from the law, and whose position in society renders further notice unnecessary.

Sir, your obedient servant,
GEORGE DAVIES.
Mile End Road, December 17, 1844.

ON THE USE OF CONIUM, AND THE MEANS OF ASCERTAINING THE VALUE OF THE EXTRACT.

IN the *Dublin Journal*, Dr. Neligan communicates his experience of this agent in the treatment of subacute and chronic rheumatism, of neuralgia, and of senile gangrene. Having extensively employed this remedy in hospital and private practice, he met with but few instances in which it failed to afford relief. The extract, as ordered by the Pharmacopœas, is in almost all instances inert; heat of even a moderate degree, when continued for any time, being sufficient to cause a decomposition of its active principle. He recommends the application of the "potash test" before commencing the use of any of the preparations of hemlock.

"It consists merely in triturating in a mortar the preparation we wish to test with a small quantity of strong caustic potash, when the peculiar odor of the active principle, *conia*, is in a few moments emitted; care, however, must be taken not to confound this odor with that of the plant itself, from which it differs most remarkably, the latter bearing a singular resemblance to the smell of mice, while that of *conia* is a peculiar, penetrating, very disagreeable, somewhat alkaline odor, an acquaintance with which may be easily acquired by applying the test to the fresh green leaves, or to the recently-gathered ripe fruit."

Dr. Neligan places much more reliance on what he calls the *succus conii*, than on the extract. It is thus prepared:—

"Take of fresh hemlock leaves any quantity, express the juice in a tincture-press, set it aside for forty-eight hours, pour off the clear, supernatant liquor from the fecula and chlorophylle which it has deposited, and lastly, add to it a fifth part, by measure, of rectified spirit."

The dose seems to have been in the cases recorded from 20 to 40 minims three times a day, or oftener.

THE DIAGNOSIS OF ŒDEMA GLOTTIDIS AND ACUTE LARYNGITIS.*

IN œdema glottidis there are very violent paroxysms of dyspnœa; in acute laryngitis the paroxysms are less marked and violent; in the former disease inspiration is singularly sonorous, sibilant, and difficult, expiration being comparatively easy and exempt from sound, whilst in the latter, respiration is not marked by any peculiar noises, nor is there that discrepancy between its two parts which is manifest in the former. In œdema of the glottis, the quantity of air entering the lungs being very small, the respiratory murmur is very feeble indeed, almost abolished throughout the thorax, whereas in simple acute laryngitis, this murmur is rather loud. Finally, in œdema we can, by carrying the finger to the upper part of the larynx, detect there a soft ring of cushion, caused by the infiltration of serous or sero-purulent fluid into the submucous cellular tissue, which is of course wanting in laryngitis.

The affection of the breathing is exclusive of the swelling round the larynx, one

* *British and Foreign Review.*

of the most distinctive symptoms of the disease. According to M. Valleix inspiration is extremely difficult, requiring the greatest efforts on the part of the patient, and amounting frequently to true orthopnea. On the contrary, expiration takes place with so much ease, that M. Blache regards it as a pathognomic sign. During inspiration there is often a wheezing, a guttural snoring, often very loud.

LIQUOR AMMONII CAUSTICI IN ECCLAMPSY.*

Mad. Berg, aged 37, primipara, was under a fit of ecclampsy; notwithstanding venesections, leeches, cold fomentations on the head, enemas, calomel, the evil was violently augmenting; opisthotonus succeeded, with incontinentiæ urinæ and fecium, the pulse was scarcely perceptible. Mr. Feltes prescribed liquoris ammonii caustici guttas tres, to be taken with some water every five minutes. After the fourth dose, the convulsions ceased altogether. The dose was then augmented to five drops, and continued until the patient had partaken of 105 drops. Next day the forceps was applied successfully, the child was dead for some time previous. During the remainder of the confinement no accidents. —nt—

THE FOOT-ILL (KLAUERSAÜCHE) OF THE SHEEP CURED BY AN ELECTO-CHEMICAL PROCESS.†

BY DR. J. H. GEROLD.

Perfectly new, simple, and cheap in its execution, is the following method of curing an epidemic disease so fatal to best bred flocks of sheep:—A measure of water is boiled, allowed to cool, and thrown into a moderately large earthenware plate, in which previously were placed from six to eight pieces of zinc plate, each of the size of a penny piece, and four to six pennies; to which is added a quarter of an ounce of sulphuric acid of Vordhausen. The diseased paws are first cleaned by river water contained in a cask, and afterwards held in the said plate for three or five minutes. This operation is repeated the next day, and the following one, if the cure is not effected after the first time. One plate containing the above quantity of sulphuric acid, may serve for 200 or 300 sheep, without being renewed, for as long as vesicles are rising, it is an active remedy. —nt—

* *Journ. Méd. de la Neerlande*, April, 1844; Froriep, v. xxxi., p. 144.

† *Casper's Wochenschrift*, No. 5, 1844.

TARTARUS EMETICS IN SYPHILIS.*

Dr. de Willenbrand administered with best effect the tartar emetic in urethritis syphilitica, and likewise in a case of relapse of syphilis with syphilitical ulcers in the throat, and the veins of the mouth complicated with jaundice. The ulcers were cured within twelve days. In other cases of syphilitical ozaena and primary ulcers of the penis, as well as of syphilitical exanthemata and of a paraphimosis, the emetic effected a speedy recovery. Out of thirty patients one only suffered a relapse.

—nt—

OBSERVATIONS ON THE PURIFICATION OF GUM RESINS.†

BY M. GOBLEY.

M. Henri, in his *Pharmacopœia Raisonnée*, recommends the purification of the gum resins in the following manner:—Take 1 part of gum resin and 4 parts of alcohol, at 56 centesimal degrees; dissolve in a sand bath in a closed vessel; the liquor is strained through a cloth and pressed; the residue is treated by a new quantity of alcohol; it is pressed, the two liquors are united, and the greater part of the alcohol is removed by distillation; the rest is evaporated in a sand bath, with continual stirring.

M. Lamothe has lately proposed to substitute this process by the following:—1 part of gum resin (gum ammoniacum, for example), and 4 parts of distilled water are taken; macerate for 12 hours in a tinned copper basin, agitating from time to time with a wooden spatula; at the end of the time, the mixture is exposed, for some time, to a heat of 160 to 170 F.; it is strained through a cloth with great pressure; the residue is treated with a fresh quantity of distilled water, and pressed; the ammoniacum is subjected to a second and even a third digestion, each time with two parts of distilled water. A glutinous *poissen* mogma remains, which is exposed to a gentle heat in half a part of volatile oil of turpentine and as much distilled water; the latter, for dissolving the gummy portion, the former for dissolving the resin; it is strained with pressure. If the resin is not entirely exhausted, it is replaced on the fire with a fresh quantity of volatile oil and water; all the liquids are united, and evaporated in a sand-bath until

* *Oppenheim Zeitschrift*, 1844; Froriep, v. xxxi, p. 144.

† *Journal de Pharmacie*, Dec., 1844.

all the moisture is consumed, and kept constantly stirred.

M. Lamothe's process answers well; it is evidently more economical than that of M. Henri; but it is not more expeditious, although about 6 parts of liquid are employed, instead of 8, to 1 part of gum resin.

It may be objected to both these processes that, they entail a great loss of time and fuel, and moreover, the waste of a certain quantity of the volatile oil of the gum resins.

Weak alcohol is the best solvent of the gum resins, and, in my opinion, the only one which should be employed for purifying them; only, in order to diminish the expense attendant on employing this agent, and to obviate the inconveniences which I have just noticed, I advise operating in the following manner:—2,000 grammes of gum ammoniacum are taken, powdered and put into a tinned copper basin, with 750 grammes of alcohol, at 56 centessimal degrees it is heated on a very gentle fire, continually stirring it with a wooden spatula; when the mass is well divided, it is strained with strong pressure through a cloth. By this means all the gum resin is extracted, for there remains in the cloth only the impurities which it contained.

The product of the operation is a soft homogeneous matter, which it is sufficient to heat for a few moments in a sand bath to obtain a proper degree of consistence. It requires about half an hour to perform the whole operation.

I have ascertained that it is advantageous, when the gum resin is very impure, to fraction the alcohol, making the first operation with 600 grammes, and the second with the remaining 150.

This process, applied to galbanum and sagopenum, succeeds very well. It is not the same with bdellium, which alcohol attacks with difficulty on account of the great quantity of gummy matter which it contains; it is also preferable to employ this gum resin under the form of powder.*

The process which I propose for the purification of the gum resin is much less expensive than that of Henri, since it admits of employing for 2,000 grammes of gum resin, only 750 grammes of weak alcohol; and much more expeditious, since the evaporation requires only a few minutes. It is also cheaper than that of M. Lamothe, for if the quantity of alcohol employed be more expensive than distilled water, the evaporation lasts a much shorter time.

It presents, besides, the great advantage of retaining in the gum resins the greater part of the volatile oil which they contain.

In conclusion, weak alcohol is the best agent to be employed for the purification of the gum resins, and at the same time the most economical, when the process is modified in the manner I have just pointed out.

* Bdellium may be purified by means of pure alcohol, when it is mixed with gum ammoniacum, galbanum, and sagapenum.

CASES OF POISONING WITH CANTHARIDES.*

The practical question as to the best mode of treatment in this accident is well illustrated by comparing the following two cases with each other:—

Case 1.—A lunatic, 45 years of age, swallowed about half an ounce of a paste which contained nearly two drachms of powdered cantharides: this was about half-past seven in the morning. In the course of a quarter of an hour afterwards attempts were made to provoke vomiting by administering tartar-emetic, and ipecacuana in hot water. Copious vomiting was speedily induced, and the patient rejected so much of the ingested mass, along with much of the mucus of the stomach, that it was believed that very little of the poisonous matter was left behind in the stomach. Towards the after part of the day, the patient's body was found to be very cold, and his pulse at the same time was contracted and rather indistinct. A slight re-action ensued, and this state lasted for about twelve hours, when symptoms of adynamic depression returned, and next day the patient died about eight o'clock. The only treatment that had been adopted, was the free use of mucilaginous and other emollient drinks. This case is related in the *Giornale delle Scienze Mediche di Torino*, and is thus commented upon by the French reviewer in the *l'Experience*:—

"It is highly probable that the fatal termination in this instance might have been prevented, if, in place of trusting to the use of such inefficient means as soothing diluents, the medical men had pursued what is commonly called the *medicine of symptoms*—the only rational method, it may be observed, to combat the constitutional phenomena of poisoning. Two circumstances in the preceding case rendered it favorable for successful treatment:—1. The one was the copious rejection of the contents of the stomach by vomiting; and 2, the little soluble form in which the poison existed. The paste had not remained sufficiently long for it to become dissolved in the juices of the stomach and absorbed into the system. The

* *Annali Univ. di Medicina.*

fatal result may therefore be attributed in part to the adoption of an erroneous treatment. The physician in the next case understood his duty better, and the consequence was that he saved his patient."

Case 2.—A healthy and middle-aged man swallowed a scruple of powdered cantharides, which had been mixed with his food by some friends in sport. In the evening he felt a sense of burning and constriction in his mouth and throat, also nausea and frequent efforts to vomit, shiverings and tremor in his limbs, &c. The general weakness and depression became greater and greater; the twitches, pains and formication in the limbs and back were more troublesome; the urine was passed with great difficulty and scalding; there was a distressing tenesmus; the thirst was intolerable, and the extremities were continually icy cold. During this (the second) day nothing but acidulated and demulcent drinks were administered; but, as the symptoms became gradually more alarming—for paralysis of the lower extremities and bloody micturition had now come on—Dr. Podrecca was called in. At this time the patient's countenance was paleand livid; his eyes were hollow and dark, and his pulse was exceedingly low: he complained of intense anxiety at the præcordia, occasional priapism, severe pains in the abdomen, &c. Wine, spirit of canella, and laudanum were immediately ordered to be given in frequently repeated doses; so that, in the course of twelve hours or so, the patient took five *litres* of the first, four ounces of the second, and four scruples of the last. The unfavorable symptoms gradually decreased, and next day the patient voided his urine pretty freely—which he had not been able to do for thirty-six hours. The weakness and cramps in the lower limbs continued for some time afterwards; but these also subsided in the course of a few days.

THE HYDRATED PEROXIDE OF IRON AN ANTIDOTE FOR ARSENIC.*

BY THOMAS UNDERHILL, ESQ., SURGEON, GREAT BRIDGE.

In the last two numbers of the *Lancet* I have read reports of cases of poisoning, by arsenic, successfully treated with the hydrated peroxide of iron. This remedy has, I believe, been employed with great success for the last ten years, and yet there are many who now doubt its powers as an antidote. In order to convince those who are still sceptical, we should endeavor to bring forward all the evidence in our power; and, with that view, I send for publication the results of eleven cases which have occurred, under my own immediate observation, during the last three years; for in this, as in all other disputed points connected with medicine, experience is the grand basis upon which to found our arguments.

The first case presents some rather remarkable peculiarities, the symptoms differing in many respects from those generally observed in cases of arsenical poisoning. They bear some analogy to that class described by Christison as his second variety, which, as that author observes, are generally produced either by taking a large quantity of the poison, or by taking it in a state of solution.

The subject of this case had taken, as he admitted, nearly two table-spoonfuls (two-pennyworth) of arsenic, mixed in about a pint of water. When called to him, I found him lying on the floor, in a state of syncope; pulse imperceptible; skin covered with a cold perspiration, with occasional convulsive tremors of the limbs, but no sickness. In a short time slight reaction came on, when, the stomach-pump not being at hand, I commenced giving the hydrated peroxide of iron, which I had carried with me, in large and frequently-repeated doses. On the arrival of Mr. Davis (a surgeon living in the neighborhood) with the stomach-pump, we immediately proceeded to use it, and well washed out the stomach by the injection of successive quantities of warm milk and water. In a few hours, symptoms of inflammation of the stomach began to present themselves, but these soon yielded to the ordinary antiphlogistic treatment.

The principal peculiarities observed in this case were the extreme faintness, and the absence of sickness, not indicating great irritation of any part of the alimentary canal, but more evidently pointing out some disorder of the nervous system. This man suffered for some time from gastric derangement, but ultimately his recovery was complete.

Case 2.—A child, about three years of age, took by mistake a considerable quantity of arsenic, which had been mixed with oatmeal, for the purpose of destroying rats. The ordinary symptoms of poisoning by this drug soon developed themselves, and were very acute, but gave way, in a short time, on the administration of the antidote.

Case 3.—Rachel S——, aged thirty-one, went to Wolverhampton, a distance of six miles, on the morning of the 7th of last

* *Lancet*, November 30, 1844.

month, on a visit to her sister. She had been in a very low, desponding state of mind for some time, and had more than once threatened self-destruction. On the afternoon of the same day she bought from a druggist's shop two pennyworth of arsenic, and one of essence of bergamot; she took them to her sister's, and, having mixed them together with a little water in a cup, drunk off the mixture, and threw the cup away. In about half an hour the usual symptoms began to appear, and continued, the vomiting and diarrhœa excessive till about 11 o'clock in the morning, when she told her friends what she had done. My father was immediately called in, but she was then in a dying state; we administered the antidote, and used other means which we thought advisable, but it was too late; she died about three hours after we first saw her. On opening the stomach after death, we found one solid lump of arsenic which weighed 15 grains, besides several smaller ones. This is rather remarkable, considering that she had vomited almost incessantly for so many hours. That the hydrated peroxide of iron did not produce the desired effect in this case, is no argument against its powers as an antidote, for the state of the patient, when we first saw her, was hopeless.

The next eight cases occurred on the evening of the 21st of last month. A neighbor of the parties who took the poison, being sadly troubled by rats, had obtained a quantity of arsenic for the purpose of destroying them, and had made up about half an ounce of it into three cakes, with two biscuits and a little flour, and put these cakes in the cellar. One of the children going into the cellar, saw them, and not knowing their contents, eat part of one, and then distributed them among four of her playmates and three women, the mothers of the children. The women described the taste as being very much like gingerbread. They were affected in a short time with the usual symptoms of poisoning with arsenic, the severity of the symptoms being in proportion to the respective quantity which each had taken. Two of the children were exceedingly ill, one, a boy three years of age, was seized ten minutes after taking the poison, and the symptoms were so acute for some time as to lead us to expect a fatal result. In each of these cases the hydrated peroxide of iron was plentifully administered, and with the happiest effect. It seemed to act as a charm, affording almost immediate relief. They all perfectly recovered.

I have given these cases as briefly as possible, not wishing to take up much space. The efficacy of this antidote has been proved, not only by experience in the human subject, but by experiments on animals. Pareira says, "1st. That it renders the poison insoluble. 2ndly. That experiments on animals have shown its efficacy; and 3rdly. That in poisoning in the human subject it has proved successful." These arguments, well substantiated as they are, should seem quite convincing to any unprejudiced individual, it therefore becomes a duty incumbent on every medical practitioner, when called to cases of poisoning by arsenic, to give this remedy a fair trial, for if not a certain antidote, it is, at all events, the best we possess.

HÆMORRHAGE AFTER LITHOTOMY CHECKED BY CREOSOTE.*

During the performance of this operation on a boy 14 years of age, it was remarked that, when the skin and perineal muscles were divided, there was a more profuse discharge of blood from the wound than usual. After the calculus had been extracted, the hæmorrhage nearly ceased for a couple of hours; but then it returned as profusely as ever. From its character, M. Daser suspected that the blood proceeded rather from a multitude of minute vessels than from any single one of considerable size. Several small arteries had been tied during the operation; and cold applications and absolute repose were now enjoined. These failing, the wound was plugged up with a piece of sponge; but, after a few hours interval, the blood having accumulated within the bladder, violent expulsive efforts came on and forced the sponge and a large quantity of coagula out. A canula was then introduced by the wound and retained in the bladder, and the sponge was replaced; still the bleeding continued. As the patient had now become very pale and faint, M. Daser had recourse to the local use of creosote. He dipped a sponge in the pure oil, and inserted it deep into the wound, having previously secured a piece of tape to it, so that it could be easily withdrawn at any time. For a few minutes the pain induced by the application was very sharp, but then it subsided altogether. The edges of the wound assumed a blackish-grey color, and the hæmorrhage ceased. At the end of ten minutes, expulsive efforts ensued, and the sponge, along with a quantity of coagula, was expelled. A fresh sponge was introduced; and no further discharge of blood took place. M. Daser was for some time afraid lest the application of the creosote

* *Graefe and Walthers' Journal.*

might induce a gangrenous inflammation of the deep-seated parts; but fortunately no such accident followed. Next day the sponge was again expelled, but without any renewal of the bleeding. Everything went on well, and the wound was entirely healed by the end of the third week.

The styptic action of the creosote was well marked in the present case. Although highly irritating and almost caustic, the secondary inflammation that followed was very trifling.

M. Daser suggests its application to atonic, varicose, and other unhealthy ulcers.

PASSAGE OF METALLIC MERCURY INTO THE BLOOD AND VARIOUS ORGANS OF THE BODY.*

M. Oesterlen has performed a number of experiments on animals, with a view of determining this point. The results of these are as follow:—

1. It is indubitable that mercury may pass, in the metallic state, through the parietes of the blood-vessels, since minute globules of it have been found in the subcutaneous cellular tissue and in the veins permeating it. The globules have never been discovered in the epidermic layers, but only in the deep-seated layers of the dermis, near the blind extremities of the hair-follicles; also in these follicles and in the sudoriferous canals. 2. The metallic mercury, rubbed on the skin or introduced into the intestinal canal, may give rise to injurious effects, by passing into the current of the circulation. It is not easy to determine in what manner the metallic mercury, when once introduced into the circulation, becomes changed and modified, or how it then acts. At the side of the shining globules M. Oesterlen always found a number of dull and dark-colored corpuscles, which resembled a good deal the granules of a mercurial oxyde; these were found to be not acted upon by alkalis, but to be dissolved slowly in strong nitric acid, after being ground down into a fine powder. In the urine and in the bile, the mercurial globules did not exhibit any appearance of decided change. 3. Minute globules of this metal, in the state of fine division, may traverse the capillaries without producing any inflammatory *stasis*: their presence in the vessels does not seem to influence the formation of the blood or the development of the sanguinous corpuscles. 4. Small quantities of mercury taken inwardly or applied on the skin, appear to pass chiefly into the parenchymatous substance of the spleen, liver, and kidneys, and to be discharged by the last two emunctories.

M. Oesterlen has never been able to detect the presence of any globules in the *cells* of the liver, or in the corpuscles of the spleen, or in the minute tubercles of the kidney: they were always on the outer surface of these organs. He conjectures that they may have escaped from the anatomical preparation after it had been put up. He has, however, detected them in the saliva and also in the urine.

* Roser's *Archives*.

HONEY & SUGAR ADULTERATED WITH STARCH SUGAR.*

It ought to be generally known that a very large proportion of brown or moist sugar sold in this country is adulterated by being mixed with what is called *British sugar*—a sugar made by boiling potato-starch for a long time in water, slightly acidulated with sulphuric acid. This is a gross fraud, because, although harmless, starch-sugar is not sweet. To detect it, all that is necessary is to agitate some suspected sugar in cold water, the genuine sugar dissolves immediately, the starch-sugar remains undissolved, and its lack of sweetness becomes readily detectable.

M. Lassaigne, in the *Journal de Chemie Médicale*, says that honey is adulterated with starch-sugar, especially that usually called *common* honey. This may be detected in the same way as in the adulterated sugar.

ON THE PRESENCE OF LIME IN VERATRIA.†

BY H. VERSMANN.

The *veratria* of commerce often contains a considerable portion of lime, which is employed in its manufacture; and as it improves the appearance of the alkaloïd, and prevents it from becoming resinous in drying, is purposely left in admixture. Veratria ought therefore to be tested by heating a portion in a platinum spoon. If it be pure, it will all sublime; lime will, of course, be left behind if any is mixed with the veratria. To purify veratria from lime, dissolve in alcohol, adding a small quantity of sulphuric acid, filter, precipitate with ammonia, and distil over the alcohol.

* *Lancet*. † *Ibid*.

SPIDER'S WEB.*

This substance was made use of long ago as a popular domestic medicine in intermittent fevers; but according to recent experiments of Italian physicians it may be administered with the greatest advantage in all periodical complaints, or such as molest only by fits, especially convulsions and nervous headaches. It is to be given in pills, from ten to twelve in twenty-four hours.

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A BLISTER INSTANTANEOUSLY PRODUCED.†

From eight to ten drops of liquor ammonii are poured on a small watch-glass, which is to be covered with a piece of linen, the diameter of which is smaller than that of the watch-glass; this apparatus is then to be put on the skin, denuded of hairs, and, sometimes within thirty seconds, a red halo and a blister is formed.

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TINCTURA IODI, A STYPTICAL REMEDY.‡

A man, 80 years of age, who, after a long-continued retentio urinæ, was molested by a violent hæmorrhage of the bladder, by which his life was endangered; several remedies having been employed without success, M. de Wageningen finally injected into the bladder a solution of tincturæ iodi drach. un. aquæ unc. tres, whereat the hæmorrhage was instantly stopped, and the patient soon entirely recovered.

—nt—

THE LEAVES OF BELLADONNA IN HÆMOPTYSIS.§

Dr. Schwöder has administered in several cases of hæmorrhage of the lungs the leaves of belladonna, which being cut into small pieces were thrown on burning charcoal, and the vapor thus produced inhaled. After the lapse of some minutes the sanguine flux was stopped. The patients were not molested by the fumes, and but rarely obliged to cough.

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* *Hufeland's Journal*, vol. i., 1844.

† *Gazetta Med. di Milano*, Feb., 1844.

‡ *Jour. Med. de la Neerlande.* March, 1844.

§ *Oesterr. Med. Wochenschrift*, 1844. Froriep, vol. xxxi., p. 224.

ON THE FORMATION OF THE AMMONIACA-MAGNESIAN PHOSPHATE IN PUTRESCENT MATTERS.*

Numerous experiments have proved that, whenever any animal matter begins to putrefy, there are formed in it crystals of the ammoniaco-magnetism phosphate, which are discoverable by the use of the microscope. By this simple method they may be readily seen in putrescent urine, for example; but, not so, if this fluid be deprived of its fibrine by the addition of corrosive sublimate; then no trace of the crystals is to be found, even after four weeks of putrefaction.

If heat be employed to get rid of the coagulable contents, the crystals are formed only very slowly. They are abundant in the products of the nephritis, and catarrh of the bladder. Dr. Zimmermann has discovered them also in the debris of carious teeth, in the exudation of pleuro-pneumonia, in the nasal mucus, in ordinary purulent matter, in the fæcal discharges—more especially of those animals which have a long intestinal canal, and which consequently retain for a length of time the food on which they live.

As often, therefore, as crystals of the ammoniaco-magnesian phosphate are found in any of the secretions of a patient, we may fairly presume that there exists a putrid decomposition of animal matter. It is only in this manner that we account for the presence of this salt in the alvine discharges in typhus fever, and in the urine of persons affected with paralysis of the bladder.

BERZELIUS ON THE URINE.†

The distinguished Swede, after giving a most elaborate account of the composition of this animal fluid, proceeds to point out how much its composition is affected by a variety of substances when taken into the stomach. The following examples will abundantly prove the truth of this remark.

After the free use of mercurial ointment, the urine is found to contain salts of this metal in minute quantities. To detect their presence, we have only to dry the sediment that is precipitated, and then to calcine it: globules of mercury may thus be obtained.

Nitre, the yellow prussiate of potash, and many metallic salts, especially those of iron, may readily be discovered in the urine not long after they have been swallowed.

* *Wochenscrift fur die gesammte Heilkunde.*

† *Journal de Pharmacie.*

After the employment of large quantities of any ferruginous preparation, the urine sometimes acquires a feeble blueish or greenish hue—owing, says Berzelius, to the union of the iron with the ferrocyanic acid, which may be generated by the decomposition of different animal matters within the body itself.

Soon after tartaric or oxalic acid has been taken into the stomach, the urine often deposits, as it cools, oxalate or tartrate of lime—a deposit that is increased by the addition of the chloruret of lime to the fluid. The malic, citric, and tartaric acids render the urine more or less decidedly acid. Succinic acid also reappears in the urine. Not so, however, with the benzoïc acid; for this seems to become transformed within the organism into hippuric acid—according to the observations of Wöhler, Boyé, and Leaming.

The infusion of nut-galls is known to pass into the urine; for a black precipitate is found to be formed on the addition of a ferruginous solution. After the administration of iodine, the iodurets of potash and ammonia are discoverable. The alkaline carbonates also, the borates, the silicates, and the chlorates may be detected by the addition of their respective reagents. The same holds true of the yellow prussiate of potash; the red prussiate is converted into the yellow. The sulphuret of potassium is absorbed in part only, without alteration; part becoming oxydised during the circulation, and converted into a sulphate.

The vegetable salts, having potassa and soda for their bases, are transformed into carbonates; for the urine is then found to be alkaline, and to effervesce on the addition of an acid. The same phenomenon is often observed when a person has been eating very freely of certain fruits, as apples, cherries, strawberries, and raspberries, which contain the malate or the citrate of potassa. This fact explains the utility of these fruits occasionally as a remedy in uric acid gravel.

MINERAL SALTS, TAKEN BY THE MOUTH, DISCOVERABLE IN THE URINE.*

Dr. Kramer, professor of chemistry in the university of Milan, read, in the course of last year, before the Imperial Institute of Lombardy, an elaborate account of his researches for the purpose of discovering in the blood, the urine, and other products of animal secretions, various mineral salts that had been taken into the stomach.

It would be tedious to report the details of his experiments; all that we propose to do at present is to present the most important conclusions which he has deduced from his extensive and very carefully-conducted investigations.

1. That the salts with alkaline bases, which we are in the habit of administering to man and the lower animals, pass with facility into the blood, urine, and even—this holds true more especially with the ioduret of potassium—into the sweat and saliva.

2. The blood and urine, when once charged with alkaline salts, free themselves of these foreign matters with very rapid progression.

3. The salts of barytes (at least the chloruret) pass in small quantities into the blood and urine: it is, however, difficult to detect their presence by the ordinary chemical reagents.

4. The vapor of certain substances, when inhaled into the throat, are absorbed and pass into the circulating fluids with a truly remarkable rapidity, as is proved by experiments with the vapor of iodine, traces of which are discoverable in the blood in half an hour after the commencement of the inhalation.

5. Very many of the compounds of the metals, properly so called, with other substances, pass into the blood and urine; the presence of the metal may generally be discovered in both fluids. The metallic salts, which have been chiefly subjected to experiment, are the sulphate of mercury, the sulphuret of antimony, the tartrate of antimony, the muriate of silver, the carbonate and sulphate of iron, and, lastly, the various combinations of copper.

6. Some of the metallic salts and compounds—for example, those of copper—may even be detected in the blood and urine eight or ten days after the internal use of the preparation has been quite suspended.

7. Iron, administered by the mouth, is rapidly absorbed and passes into the blood and urine; it should be borne in mind, however, that the urinary secretion in health contains a minute quantity of this metal.

8. Copper also is found, in a still more minute quantity in healthy urine; but it seems probable that the existence of this metal in animal fluids is derived from the copper vessels employed in domestic economy, and also from not a few articles that are used for food. If copper is present in the urine, we may fairly suppose that it may also exist in the blood.

9. Normal blood always contains a certain quantity of manganese; the urine also does not appear to be quite exempt from it.

* *Giornale dell' Instituto Lombardo.*

ARSENIC IN THE BONES.

OBSERVING that a medical man of the name of Brett, of Liverpool, at a coroner's inquest lately held in that neighborhood, stated that Orfila had never recanted his former statement that arsenic exists naturally in the human bones, although we thought that sufficient had been said on this subject in this and other scientific periodicals to have prevented ignorance on a point of such importance, we quote the following extract from M. Orfila's *Traité de Toxicologie*, vol. i, p. 437.*

"*There exists, according to M. Couerbe, arsenic in the body of man: now, who would dare to affirm that the minute portion of this metal, which has been obtained in certain medico-legal investigations, arose from poisoning, rather than from that which is contained in the bones and flesh?*"

M. Couerbe was the first who said that the body of man, in a state of putrefaction, contained arsenic. *I proved in* 1839 *that there was none* in the viscera; but I announced having extracted it from the bones. Some time after, M. Couerbe went further, for he *affirmed*, without proving it, that this metal is found *in the bones*, in the state of *arseniate of lime*, and that it is *produced* in proportion as the *flesh perishes*. M. Devergie, after having confirmed, by experiments, the statement that the bones furnish *a considerable proportion of arsenic*, capable of being isolated by chemical means, added that the muscles *gave* only an extremely small *proportion*—so small that its existence could not be demonstrated by proofs free from objection. (*Medicine Légale*, 2nd edition, vol. iii., p. 449.) On the third of November, 1844, I deposited a sealed packet at the Royal Academy of Medicine, in which I stated that I *could no longer extract arsenic from the bones*. This packet was opened at the sitting of the 13th of June, 1841. It was only at the *end of December*, 1840, that MM. Flandin and Danger announced the same fact. There is, therefore, no occasion to hold this objection any longer, which is of great importance to the applications of the medico-legal system which I have proposed; indeed, in every judicial matter, the defence did not fail to attribute to this arsenic, called *normal*, a great part; it is this, it was said, which you have extracted from the subject on which you have operated, and not arsenic swallowed as a poison. It was well replied that the viscera *do not contain an atom of it*, and that the arsenic extracted by myself and other medical witnesses *was extracted from these viscera;* it was, nevertheless, obstinately maintained that it was so. Be it as it may, it will always be difficult, not to say impossible, to explain how, in 1839, *well-characterised arsenic* was obtained, by calcining bones to a whitish-gray, and treating them by concentrated sulphuric acid, whilst now not an atom is extracted by following the same process, even when the experiments are multiplied beyond measure, and when they are made in the open air or in closed vessels, so as not to lose any product. It may be asked whether the reagents which I then employed contained arsenic? Certainly not: for, by acting with *equal proportions of the same reagent, and bones calcined to whiteness*, or with carbonate of lime, no stain was obtained. (See my memoir on "Normal Arsenic," in vol. viii. of the *Memoires de l'Academie de Médicine.*) Who will believe, as MM. Flandin and Danger pretend, that I mistook for arsenical stains, the pseudo-stains which *I had observed before them*, and on which they have laid so much stress? This will not bear the slightest examination, for I again affirm that the stains obtained by me *presented all the chemical characters of arsenic;* besides, why, if it were thus, cannot these pseudo-stains be obtained now as well as then, by treating by sulphuric acid in the same manner, bones calcined to a whitish-gray? The following is what passed at the sitting of the Institute on the 19th of July, 1841. MM. Flandin and Danger requested by letter that the Commission would express itself on this point. "*The pseudo-stains which we have obtained have been taken for the arsenic, which MM. Couerbe and Orfila called normal arsenic.*" The reply of M. Dumas was as categorical as possible. It is as follows:—

"The Commission has not been able to *compare* the stains obtained by MM. Flandin and Danger with those of normal arsenic, because none of the members of the Commission have seen stains of normal arsenic.

"The Commission could not, therefore, express itself concerning the *difference* or identity of the stains obtained by MM. Flandin and Danger and those which other chemists have designated as stains of normal arsenic."

It is evident, therefore, that the Institute did justice to the singular pretension of MM. Flandin and Danger. These gentlemen had evidently been unable to obtain pseudo-stains by treating bones calcined to gray by sulphuric acid, before the Commission of the Institute, otherwise they could have been compared. This Commission, evi-

[* The words printed in italics are so in the French.—EDS.]

dently, had not collected them in the experiments which it had tried for its report; for myself, in operating before this Commission, in my laboratory, I was not able to show a single one; it is, indeed, because *pseudo-stains are not formed in treating bones by sulphuric acid,* as I did in 1839, *when I extracted from these bones arsenical stains.* Let MM. Flandin and Danger cover one or more plates with pseudo-stains obtained from bones calcined to gray with sulphuric acid; this will be the only means of proving that they are right, and that these stains are those of which they have spoken. They have not done so, they will not do so, and nobody has been able to do so, because the assertion enunciated is completely false.

It must be owned that there is a most impenetrable mystery concerning the history of the arsenic called normal. Why was it easily obtained in 1839, and why cannot a single atom be now extracted by acting in the same manner, and by a very simple process?

ON THE USE OF THE TOBACCO ENEMA IN HERNIA.*

BY W. CLULEY, ESQ., SURGEON, ASHTON-UNDER-LYNE.

I WILL make one or two remarks on the use of opium and tobacco, in cases of hernia; the former remedy having failed, in such opportunities as I have had, to produce the effects which might be expected from the accounts of late writers, and the objection against tobacco, from its dangerous character being obviated by the use of means, which, although only tried in one case, yet in that with such marked effect as warrants the expectation that a similar result will generally follow. When opium fails, which I believe it frequently will, the relief it affords from present suffering is no small objection to its use, inasmuch as the false idea of security which such relief is calculated to inspire may lead to the dangerous delay of more efficient measures. The plan, then, that I refer to, as obviating the dangerous effects of tobacco is, simply, the removal of the infusion from the bowels after it has produced its full effect.

October 27, 1844, half-past 10, a.m. Was called to Mrs. B., aged 48, and mother of sixteen children. Found her suffering from femoral hernia, which had been in a state of strangulation all the morning. The hernia had existed four months, and had never once been returned. I tried the taxis and cold applications ineffectually, then administered a tobacco enema of the usual strength, and left her for about an hour. I returned, accompanied by Mr. Booth, of this town, and found her in a most alarming state of prostration. The tumor was less tense, and, after repeated manipulations, at last returned under the hands of Mr. Booth, but the occurrence of death seemed not the less likely to happen.

The plan I have alluded to was now put into operation. Water was injected as long as it would pass, and allowed to return by the tube, for the bowels seemed to have lost all expulsive power. This was repeated three or four times before signs of reaction took place, which was, however, soon satisfactorily established; and at a visit some two hours afterwards, the safety and gratitude of the patient were equally apparent.

* *Lancet.*

THE ADMINISTRATION OF MEDICINES IN THE FLUID STATE.*

"IT has been found that fifteen grains of sulphate of quinine exhibited in infusion of senna, are more efficacious, as a tonic, notwithstanding the aperient quality of the vehicle, than twenty-four grains of quinine taken in pills. M. Panezza accounts for this difference by supposing that the senna by augmenting the perisaltic action of the alimentary tube, and increasing the secretions of the bowels, excites the production of a fluid well adapted for perfectly dissolving the quinine, and in that state it is applied to a much greater surface of absorption than if it passed along the canal in the form of pills."

INOCULATION WITH TARTAR EMETIC.†

To avoid the symptoms which frequently trouble patients on the exterior application of tartar emetic in the usual way, Dr. Degourge proposes the following method:—The tartar emetic, reduced to powder, is levigated on a glass plate with water or oil; from this mixture a portion is taken by a lancet or an inoculating needle; and on the skin, in the place desired, a sufficient number of punctures are made. These spots are readily inflamed, the inflammation being rather slight, and soon disappears, if the pustules thus produced be not covered by the liquid prepared in the above manner, for which latter purpose Dr. Degourge employs

* *Medico-Chir. Review.*

† *Journal de Méd. de Bruxelles,* vol. i., 1844.

a spatula, or a fine camel's-hair pencil. This treatment to be continued from two to five days. —nt—

CASE OF SECONDARY SYPHILIS TREATED BY A WATER-QUACK AS RHEUMATISM.

BY JOHN HEY ROBERTSON, ESQ., M.D., GLASGOW.

We consider the following case of water-quackery so deserving of exposure, as being a fair sample of the practice of the cold water-*cure* doctors, that we extract it entire from our talented contemporary, the *Lancet*.

"I was requested to see a young person, said to have been for a long time laboring under rheumatism.

"On calling at about two, p.m., I found a very young-looking man, just risen from bed, and his limbs and joints swathed in many folds of wadding and flannel. Right wrist inflamed and much swollen; parts all around thickened and painful, even to gentle fingering; knees and ankles also much affected; the latter very bad indeed, swollen and shapeless. He tried to walk, to show me what he could do. It consisted of a most sorry attempt, leaning his whole weight on a relative, and slowly trying to lift a foot from the ground, without bending the ankle, this accompanied with considerable pain. Over the left ankle, and towards the front of the joint, there are two swollen bursæ, as large as good sized walnuts. On both limbs, from his ankle to the knee, and higher on the back part of the thigh, even as far as the hips, there is a profusion of copper-colored blotches; here and there a scaly patch, and on the dorsal and sacral regions, a great number, but in different stages. On cross-questioning him, it was admitted that the scales fell off, and were *reproduced*, &c.; throat unaffected.

"The account given by himself and his friends was, that having had small-pox some six months ago, *the pocks had never fairly healed up!*—that he must have returned too soon to his occupation, for he shortly after got this 'rheumatism,' for which he had been under a certain publicly-professed *water-doctor*, for some months, but returned home worse than ever.

"He underwent, for the cure of this supposed rheumatism, the usual local and general water-treatment, including the repeated use of the wet sheet, and the other peculiarities of the wise man of Gräfenberg.

"Imagine the astonishment and indignation of all concerned, on my intimating to them that it was a case of secondary syphilis, of exceeding easy detection, but complicated by the grossest mismanagement!

"On my putting the question to the young man, he admitted having had primary disease nine or ten months ago, and with some difficulty I got him to confess that '*a friend* (non-medical) had healed it!'

"The hydropathists pretend to cure lues by cold water, and call mercury and all other medicines *poisons*. On inducing profuse perspiration, they say, pointing to the fetor that is present in all or in most instances of profuse perspiration, 'there is the mercury coming out of the system!'

"In this case they had no mercury to deal with, but a simple untreated case of syphilis, and instead of the syphilis being driven out of his system, it was permitted to become worse, if they did not actually make it worse, superadding to evils great enough already, other evils, which seriously complicated the case, and entirely for months disabled the patient, and which were, undoubtedly, to be laid to the account of the water cure.

The person who keeps this cold-water shop, moreover, professes to be a member of a Metropolitan College of Surgeons, and yet seems not to have known the nature of this very evident and very gross case, when he saw it. At any rate, it will be admitted, either that he knew the disease, or that he did not. If the latter, how is he to escape the charge of ignorance? If the former, how is he to escape the greater charge of dishonesty? For, will it be said, that he knew these symptoms to be lues, and yet persisted in measures he could not help knowing were unlikely to cure the patient, and which he could not help seeing were adding to his evils? He is welcome to the honor of either dilemma.

"The disease was, of cour e, called and believed to be 'rheumatism,' else the patient would neither have been sent to the cold-water establishment, nor allowed to remain there, had the discovery been made sooner. He had severe pains during the night, depriving him of sleep, and was, when I saw him, merely getting himself well rubbed, and the severely afflicted parts kept warm, in despair of any medical treatment doing him any good.

"I put him at once under the action of some of those substances which are familiar to the profession, and in judicious hands so invaluable, but which the water people, with characteristic recklessness, call 'poisons,' forgetting that their arguments, if arguments they can be called, are reasons against, not the use, but the abuse, of remedies. As well might they bid us decline the help of a

candle or gas, because light will blind us—or to warm ourselves, because the fire can burn us. In short, all that it resolves itself into is comprised in two words—IMPUDENCE and ABSURDITY.

"In rather more than a week I saw the patient again. His system was slightly under the influence of the medicine ordered, and every pressing symptom relieved. He no longer complained of pain, and had not an atom of anodyne medicine. One of his ankles had nearly resumed its old size, and he came out of bed to show me how well he could walk! He still leaned upon a relative, but the difference of *weight* was very great, and he bent his ankles in walking. The blotches were of course, as yet, only a little affected. I found him, and those about him, full of gratitude, and, from despair, filled at once with encouragement and hope."

TREATMENT OF THE ITCH IN BELGIUM.*

THE following circular has been addressed to military surgeons by the Inspector General of the Belgian Army:—

"Each patient is to be supplied with an ounce or an ounce and a half of liquid sulphuret of lime in a small pot: this quantity he is to rub carefully and slowly with his hands on every part that is covered with papulæ. If there be any papulæ on the back, another patient is to rub the liquid upon that part. The operation is to be repeated three times in the twenty-four hours, so that each patient consumes three or four ounces of the sulphuret daily. A bath is to be taken every alternate day; the frictions are to be suspended on that day. Fifteen frictions (or ten days' use) are usually sufficient for the cure of the disease, if the medical officer in charge sees that the remedy is properly used."

The sulphuret is prepared thus: take of sublimed sulphur 16 pounds, and quick lime 32 pounds: boil in 80lbs. of water for three-quarters of an hour. Let the mixture rest for some time until it settles, and then let the clear fluid be decanted off. Boil the residue afresh in about the same quantity of water, treat it in a similar manner, and add this decoction to the first. Usually 140 pounds of the sulphuret, at 12° by the aræometer, are thus obtained. If the liquid be more dense, it should be lowered to this standard by the addition of rain water.

* *Ann. de la Soc. de Med. d'Anvers.*

EFFECTS OF PRUSSIC ACID.*

BY GEO. L. BLYTH.

Dr. LETHEBY's report of the late cases of poisoning by prussic acid, brings to my recollection a circumstance which, although relating to an animal, bears a very remarkable analogy to one of the points connected with the unfortunate young persons who came under his examination.

A young cat having lost its mistress (deceased), became very uneasy, crying about the house and garden in a most piteous manner, refusing its ordinary food, declining to sleep in its accumstomed place, and at last becoming so excited that it was an act of mercy, as well as of prudence, to put it out of its misery.

Having with difficulty enticed it with some milk, I poured about a drachm of Scheele's acid on its nose and mouth; I watched the results with some interest, attention having been lately so strongly excited on the subject. The poor thing at first appeared stupified; in about ten or fifteen seconds it staggered, calmly stretched itself at full length, and expired without a struggle, or noise. What I the more particularly wished to observe was the "expiratory shriek," and I was the more astonished, as I had never before seen death take place in animals from the effects of prussic acid, without violent struggling and one or more screams: I think we may infer that no screams were made by the unfortunate couple in the case above alluded to as two violent shrieks, either together, or immediately succeeding each other, would have attracted the attention of the inmates of the house.

It is very ticklish ground to draw any analogy between the mental emotions of a cat and the mental emotions of two human beings; yet, may not the state of mental excitement have tended to produce the results noted? In the case of the cat, it was not mere instinctive regard for kindness, but a positive attachment to its mistress, for it refused the same favors from other hands. Disappointed in its attachment, there resulted the mental condition above described. Disappointment in *their* attachment no doubt produced a state of mental excitement which ended so unfortunately to the young couple. A specious hypothesis might easily be drawn up to account for the result.

I would only offer these ideas as a suggestion. But the collection of facts would tend materially to clear up the present mist that hangs over the action of this fearful poison.

* *Lancet*, Dec. 28th, 1844.

HYPERTROPHY OF THE CARTILAGES OF THE TRACHEA AND BRONCHI.*

Professor Gintrac, of Bordeaux, relates the following case in a recent number of the *Montpelier Journal of Medicine*.

A boy, eight years old, and of a delicate ailing constitution, had been from his infancy affected with a greater or less degree of dyspnœa, which was occasionally accompanied with sharp pains in the chest, and other symptoms of thoracic distress. He had been repeatedly admitted into the hospital; and had generally received relief, at least for the time, from bleeding and the application of blisters between the shoulders. When he entered the hospital on the 24th of May last, his breathing was exceedingly oppressed, so as sometimes—more especially in the evening—to threaten suffocation; there was, however, no cough present. Auscultation discovered nothing, except a strong sibilant noise in the bronchi; and every part of the chest was perfectly resonant on percussion. There was nothing abnormal in the pulsations and sounds of the heart, save that the former were somewhat more obscure than usual: the pulse too at the wrist was regular. The abdomen was rather full, but the evacuations were healthy.

This state, of which the most prominent symptom was a constant dyspnœa—every now and then liable to great aggravation—had been but relieved by the remedies that had been employed, such as enemata of assafœtida, fumigations of stramonium, the use of calomel, sulphuret of soda, &c. The child left the hospital on the 8th of July, but was brought back seventeen days afterwards; the symptoms then being unusually intense. The suffocative dyspnœa continued up till the period of death, which occurred on the 8th of August.

Dissection.—The body was much emaciated, the sternum was very prominent, the thorax was fuller on the left than on the right side, and the face and extremities were œdematous. The lungs were found to adhere to the thoracic parietes, except at the upper part on the left side, where there was present a small quantity of limpid serosity; in every other part the pleura, whose tissue was much thickened throughout, was more easily detached from the ribs and the intercostal spaces than from the surface of the lungs. The larynx was healthy, and so seemed the trachea likewise externally; but, on slitting it open, its parietes were discovered to be much thicker and harder than usual. This alteration was most conspicuous at the lower part, close to the point of its bifurcation; it extended also to the bronchi, for a considerable extent. The result was that the calibre of the air-passages was very sensibly smaller, and more contracted than in a state of health. The lungs were œdematous; for, on dividing them, a quantity of yellowish serosity oozed from the divided surfaces: the lower lobe on the right side was hepatised.

* *Medico-Chirur. Review.*

REMARKS ON THE MOLLUSCUM CONTAGIOSUM.*

The *Molluscum Contagiosum* of Bateman is a disease of a very rare occurrence in France. Biett has never seen an example of it; but Professor Andrieu has recently published two cases of it.

A girl, eight years of age, had a number of *verrucæ* on the chest, neck, and lower half of the face: she had, it was supposed, contracted them at a boarding-school, where she was much in the company of another girl who was similarly affected. This dermatosis exhibited the following characters when first examined by M. Andrieu:—"In what might be called the first stage, each tubercle was of a conical shape, shining on the surface, and of a paler color than the surrounding integuments. The apex of the tubercle was truncated, and in its centre there was a gray or blackish point, giving it an umbilicated appearance: the basis was large and sessile.

"In the second stage or period, the tubercle had increased in volume; its basis was now pediculated; its free part was round and distended; and the umbilicus or central spot was more prominent. By degrees, the little tumor assumed a red hue, and became apparently the seat of a peculiar kind of inflammatory process. Upon being compressed, there escaped from the central opening a small quantity of cheesy-looking matter, very similar to what is squeezed out of the cutaneous follicles in *acne*.

"In the third stage, these warty-looking tubercles resemble, in a very striking manner, gooseberry grains of a medium size. Reddish as first, subsequently of a dull red hue, and at length of a blackish color during a process of destructive maturation, they end by becoming completely desiccated, and then fall off, leaving behind them a discoloration of the skin, somewhat analogous to that which is left from the vaccine inoculation."

* *Gaz. Méd. de Montpelier.*

Two months after his first visit, M. Andrieu again saw his patient, and he then found that her mother had become affected with the disease. Tubercles, similar to those which we have now described, had made their appearance on her face and neck and afterwards on her breast and fore-arms.

Frictions with a pommade—composed of four parts of arsenious acid and thirty-four parts of lard—caused the complete dispersion of the tubercles in both patients: the time required for the cure being about three months in the case of the child, and four in that of her mother. No constitutional remedies were administered in either instance.

PERIODIC SWELLING OF THE KNEE CURED BY OPIUM.*

A MIDDLE-AGED woman, of intemperate habits, sprained her left foot in February, 1843. Six weeks after the accident, and when it seemed that she had quite recovered from its effects, the ankle-joint and lower part of the leg became suddenly very much swollen, hot and painful; this state continued for a few hours, and then gradually subsided. The swelling, &c. on the next and following days recurred about the same time of the day —two o'clock; and, after lasting about three or four hours, disappeared. The general health remained perfectly good during all this time,

The periodicity of this affection, and the absence of all inflammatory symptoms, induced me, says the reporter, to order quinine, which was administered to the amount of 15 grains daily; the joint was at the same time ordered to be rubbed with a camphorated opiate liniment. These means were steadily continued for six days; but no benefit was produced. The starched bandage was then tried, with the view of preventing the recurrent swelling; but the use of it caused so much pain, that it was at once discontinued. Trial was next made of the affusion of cold water upon the joint, followed by brisk friction; the internal use of sudorifics and colchicum was conjoined with this local treatment; but everything seemed to be utterly impotent. On finding this, I began the use of the acetate of morphia. Its effects on the complaint were very speedily manifest; each day the pain and swelling became less considerable, so that in the course of a week, the patient appeared to be perfectly well.

On the day, however, following that when she had discontinued the use of the morphia, the swelling and other symptoms again made their appearance at about ten o'clock in the forenoon, and continued for nearly the usual time. I wished now to give another trial to the quinine; it was therefore administered in large doses, but again it failed. The morphia was then taken, and with equally decided effects as on the former occasion. Its use was continued for a fortnight; after which it was suspended, without any return of the symptoms.

* *La Belgique Médicale.*

ON THE USE OF PLATINA IN VENEREAL DISEASES.*

A SERIES of experiments were instituted in this respect by the late Mr. Fricke, and the following results were obtained:—1. That the chloride of sodium and platinum in small doses (⅛ to ¼ of a grain) produced neither sensibly objective nor subjective phenomena, except some slight symptoms of indigestion, constipation, and sense of oppression in the stomach, which soon disappeared; it, however, is beyond doubt that large doses may produce more striking inconveniences. 2. That the same substance, in the said small doses, has no distinguishable specific action upon any certain organ. 3. That it may perhaps provoke a change in the general functions of vegetative life, but such an influence, in the cases before us, was not recognisable. 4. That a specific action of that body upon any individual system, for instance the dermatic, could not be demonstrated. 5. That no effect was noticed on after-productions and degenerations existing in a dyscrasic ground. 6. That its efficacy, therefore, in inveterate blenorrhœa, fluor albus, condylomata, syphilitical disorganisations, &c., is at least very problematical, and that hence its employment in the said cases must not be preferred to any rational one, by which the individuality of the patient may be kept in view. —nt—

TREATMENT OF BED-SORES.†

A WRITER, in a recent number of Walther and Ammon's *Journal*, recommends the application of a lotion composed of equal parts of spirit of camphor and the vegeto-mineral water of Goulard. The parts, that have become red by the pressure, should be repeatedly moistened with this lotion; it requires to be briskly shaken before it is applied.

* Froriep's *Notizen*, 1844.

† *Medico-Chirurgical Review*, Oct. 1844.

If, in spite of this treatment, the skin should break, the zinc or lead ointment, to which some camphor has been added, is a good application. In still more obstinate cases, an ointment, consisting of four parts of fresh-prepared tannate of lead, and thirty of lard, has been sometimes found to answer extremely well. On the whole, however, nothing succeeds so uniformly, alike as a prophylactic remedy against the abrasion of the skin and a healing one to that which has become broken, as a solution of creosote —prepared after the method of Reichenbach —in the proportion of one part of the oil to 80 parts of water.

When the affected part becomes gangrenous, fomentations with a decoction of yellow bark, to which some tincture of myrrh has been added, may be useful. Some patients have found benefit from the sprinkling of the ulcerated surface with a powder composed of bark, camphor, and myrrh; others, from the use of the camphorated styrax ointment. The tinct. benzoïni compos., or Friar's balsam, is often an excellent application to bed-sores. Whatever be the nature of the application employed, the most important remedy of all is the removal of pressure from the affected parts, by means of air or water cushions. The comfort derived from the use of these is most pleasing.

BOOK RECEIVED.

THE LITERARY and SCIENTIFIC REGISTER and ALMANACK, for 1845. By J. W. G. Gutch, M.R.C.S.L. This is a very useful and interesting little book, and should be in the hands of every one.

TO CORRESPONDENTS.

"W. H." (Bristol) has our thanks. Our pages are always open to original communications.

"S. L." (Manchester).—We will have the diagrams engraved at our own expense, if the article is approved of. We must, however, have the *entire* MS. before we can decide.

"J. T." (Maidstone).—"Agricultural Chemistry" will, as usual, form an important feature of *The Chemist*.

"R. W." (Farnham).—The analysis of the guano shall be forwarded in a few days.

"A. R." (Liverpool) —Yes; a reduction will be made if the advertisement is inserted more than once.

"O."—The 5th volume is ready, and will be published with this number.

SUMMARY OF METEOROLOGICAL OBSERVATIONS MADE AT OTLEY, YORKSHIRE, IN NOVEMBER, 1844.

BY E. THOMPSON.

BAROMETER.

Monthly mean	29·66
Mean at 9 a.m.	29·66
,, at 3 p.m.	29·66
,, at 9 p.m.	29·66
Maximum on 21st	30·21
Minimum on 9th	28·81
Range	1·40

THERMOMETER.

Monthly mean	42°
Mean of maxima	48
,, minima	37
Maximum on 4th	56
Minimum on 26th	31
Range	25

RAIN.—2·46 inches.

WIND.

	Days		Days.
N.	0	S.	0
N.E.	4	S.W.	5
E.	4	W.	8
S.E.	2	N.W.	1

WEATHER.

	Days.		Days.
Clear	7	Rain	6
Cloudy	4	Hail	0
Overcast	8	Misty	4
Showers	6	Frost	3

GENERAL REMARKS.

The temperature during the month has been remarkably uniform, there having been but three frosty nights throughout the period. Most of the rain recorded fell about the 6th of the month. The unusually mild weather has not been favorable to health. Typhus, scarlatina, and inflammations have been common.

NOTA BENE.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of the Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th; Advertisements on or before the 27th.*

THE CHEMIST.

I. CHEMISTRY.

ON BENZOÏN.*

BY M. E. KOPP.

M. KOPP, with the view of investigating the relations which exist between the resins of benzoïn and the benzoïc acid which accompanies them, submitted the former to a series of reactions, such as dry distillation and treatment by nitric acid, and studied the products. This examination readily showed the existence of two different types in these resins—the type of benzoyl and the type of phenol. The derivatives of these two series are met with, indeed, in the different reactions, and their nature depends on that of the agents employed. Thus, for example, dry distillation furnishes: 1st, benzoïc acid; 2nd, phenol. The action of nitric acid produces, for the first series, hydroguret of benzoyl, benzoïc acid, and a body isomeric with benzoïc acid; for the second series, nitropicric acid. The action of chromic acid gives rise, on one hand, to hydroguret of benzoyl and benzoïc acid, and, on the other hand, to carbonic acid and formic acid arising from the complete destruction of the phenol series.

The analysis of two different samples of benzoïn, made according to Unverdorben's method, gives the following results:—

	I.	II.
Benzoïc acid.....	14·0	14·5
Resin *a* soluble in ether ..	52·0	48·0
Resin *b* soluble only in alcohol	25·0	28·0
Resin *c* soluble in a solution of carbonate of soda	3·0	3·5
Brown resin deposited by ether.....	0·8	0·5
Impurities	5·2	5·5
	100·0	100·0

The composition of benzoïn must evidently be variable, since the white tears are formed only of the resin *a*, and contain from 8 to 12 per cent. of benzoïc acid, whilst the brown parts contain the two resins *a* and *b*, and as much as 15 per cent. of acid.

The dry distillation of a mixture of resins freed from their acid furnishes, by conducting the operation with skill:—

1. An unctuous fatty matter, which appears to be the odorous matter of benzoïn:

2. A crystalline matter, partly dissolved in an oily liquid. This liquid is at first colorless, or of a light rose color; but, towards the end of the operation, the temperature being progressively raised, it becomes more and more thick and of a deeper color. The separation of the crystals and the oil is operated by means of a weak alkaline solution. The oil is separated, and we have a saline solution from which the powerful acids again precipitate the crystalline matter; the latter, purified and analysed, possesses the characters and composition of benzoïc acid $C^{14} H^{12} O^4$. The oil, purified by rectification and dishydrated, possesses all the properties of phenol, as well as its composition. Indeed, it boils at about 292° F., has an odor similar to that of creosote, coagulates albumen, and colors blue the wood of fir, when it is afterwards moistened with hydrochloric acid, &c.

The action of nitric acid on the resins is extremely energetic, especially at the commencement. The matter swells up, turns yellow, copiously disengaging nitrous vapors, and a very porous, brittle, orange-yellow mass, of an extremely bitter taste, is obtained. This mass is a mixture of several bodies greatly resembling each other, and which it is very difficult to separate. The reaction having become slower, this yellow mass may be introduced into a retort, and treated by fresh portions of nitric acid; it is cohobated three or four times, and, finally distilled to dryness.

In the receiver is then found a liquid acid, containing crystals of benzoïc acid, hydroguret of benzoyl, hydrocyanic acid, and nitric acid. By pouring on the residue of the retort three or four times its bulk of boiling water, the unattacked resin is separated,

* *Comptes Rendus*, No. 24, December 9, 1844.

and, after having removed it, we have a yellow solution which, by cooling, deposits a beautiful, amorphous, yellow powder. The filtered liquor neutralised by bicarbonate of potassa, immediately furnished an abundant crystallisation of nitropicrate of potassa. The alkaline mother-liquors, separated from the crystals and evaporated to three-fourths, after having been rendered acid by nitric acid, again deposit the yellow powder, but impregnated with a considerable quantity of resin. As to the unattacked resin, it is exhausted by boiling water and again submitted to the action of nitric acid, which reproduces the same phenomena. The existence of nitropicric acid $C^{12}\ H^4\ N^6\ O^{13}+H^{20}$ was perfectly proved by its properties and those of its salts. Nitropicrate of potassa was obtained in very fine well-developed crystals, of a brown color with variegated reflections. The neutral salt of lead, which is very soluble and crystallises in needles, was obtained by the double decomposition of acid acetate of lead and nitropicrate of potassa. It is powerfully detonating. Its formula is:—

$$C^{12}\ H^4\ N^6\ O^{16}+PbO+H^2\ O.$$

The yellow powder acts like an acid; it is very soluble in water, and deposits in the form of an amorphous powder; it is, likewise, very soluble in alcohol and ether. It forms with the metallic bases, colored precipitates, which fuse slightly when heated. The constantly variable composition of this body, as well as of its salts, according to a more attentive examination, proved that the yellow coloration, which is very intense, and adheres with remarkable stability to the animal tissues, was not an integrant part of the amorphous powder, but that a separation might be effected by very frequently repeated solutions and precipitations. This separation is especially due to the property of the yellow nitrogenous matter of resinifying by the contact of the air, or of remaining in a greater quantity in the mother-liquors.

Finally, a white, amorphous powder is obtained, of a slightly acid and pungent taste, more soluble in hot than in cold water, very soluble in alcohol and in ether. It saturates bases, forms uncrystallisable salts with the alkalies, and sparingly soluble precipitates with the metallic oxides; the acids again separate from it the body in the amorphous state.

This matter is remarkable for possessing the same composition as benzoïc acid, and for being completely converted by heat, and without leaving the least residue, into this latter body.

Thus, by heating the dry matter in a small retort, it fuses at once, covered with small crystalline bracteæ; by further heating, it boils, and the whole distils; the product collected is now entirely crystalline; dissolved in water, it crystallises in beautiful bracteæ; in a word, we have exactly benzoïc acid.

This conversion takes place likewise with the impure acid; only the yellow coloring matter is then destroyed, giving volatile products having the odor of bitter almonds, and leaving a considerable residue of charcoal. Sometimes this decomposition takes place with violence, and with a disengagement of heat and light.

Concentrated sulphuric acid dissolves the resins, forming a crimson-red color. By the addition of water, the greater part of the resin is deposited with a violet color. The acid separated by carbonate of lime gives a soluble salt of lime, which indicates the presence of an accompanying acid. The colored resin may itself be decomposed into other resins. This reaction deserves further examination.

ON LINSEED OIL—ITS PHYSICAL AND CHEMICAL PROPERTIES, AND ITS PRODUCTS OF OXIDATION.

BY DR. F. SACC.

(Concluded from page 18.)

PRODUCTS OF THE OXIDATION OF LINSEED OIL.

The beautiful labors of Messieurs Laurent and Bromeis induced me likewise to subject to a nearer examination the products obtained by the action of nitric acid on linseed oil.

One part of linseed oil is mixed with two parts of common nitric acid of commerce, diluted with four times its own volume of water in a porcelain capsule, capacious enough to contain double the quantity of this liquid.

On gently heating over a moderate charcoal fire, the mixture becomes of a fine red color; and on a longer action it begins to boil, the gas bubbles break on the surface of the oil without raising it; no fumes of sulphurous acid are formed. Now the red oil begins to tumefy, and without changing its color, is rapidly transformed, copiously disengaging nitrous vapors, into a mass of a membranous consistence, which is very tenacious, elastic, of a silky appearance, presenting several great cells filled with nitrous acid. This substance is raised over the nitric acid in the shape of a cupola, and would be entirely disengaged from it if it were not to be broken and again put in contact with that acid. It gradually turns yellow, becoming of an unctuous, resinous consistence. Notwithstanding this change,

the oil continues to tumefy so long as the oxidation proceeds. On cooling, the unctuous mass solidifies into a light-yellow resin, in which some crystalline scales are observed. From the supernatant liquid, a large quantity of oxalic and suberic acids crystallise.

By repeatedly fusing the resin with water, with a view of purifying it, and dissolving it in boiling alcohol, the margaric acid, on cooling, crystallises in inconsiderable quantity in proportion to the amount employed. The unctuous orange-yellow mass remaining in solution, is transferred into oleïc acid. I will treat of its composition when speaking of the products of oxidation of oleïc acid, contenting myself now with remarking only that it may easily be recognised by the deep red color which it assumes on coming into contact with the caustic alkalies. On being saponified, and separated again from the soap by hydrochloric acid it presents a semi-fluid, unctuous, brownish-red resin, by the characteristic color of which we may easily recognise the moment when the products of oxidation obtained from linseed oil are pure; as such, they may especially be considered, when on being treated with the caustic alkalies, they are no longer colored, though, on account of the pertinacity with which these combinations adhere to them, it is difficult to wait for this moment.

The combination which was formed by saponifying the raw resin, contained so enormous a quantity of water, that it could not be separated from its mother-liquor, by common salt, as may be done with other kinds of soap. On attempting to remove it, whatever might have been the quantity of the water employed, it is perceived, that the whole is transformed into a more or less gelatinous mass; nothing remains but to press it between paper. It is highly soluble both in water and in alcohol. Decomposed by an acid, with the aid of heat, a dark brown resin rises, occupying the surface of the liquid, and partly highly soluble in water; it is semi-solid, and of a rather crystalline appearance, which may be derived from the margaric acid contained in it. To the touch it appears as a fatty substance, and as a resin, soiling matters coming in contact with it with a dark brown color.

By boiling with water the raw resin tumefies considerably, disengaging a great quantity of pungent fumes, which, to a certain extent, remind one of the odor of mint and are formed during the whole time of the oxidation of the linseed oil. Soon, however, the effervescence ceases, and the whole mass becomes of the appearance of the brown substance, which is obtained by decomposing the soap. This resin, on being boiled, exhibits the remarkable property of being divided into two portions; one rising, occupies the surface, another falling to the bottom of the vessel; this separation is probably produced simply by the presence of margaric acid, it being contained to a greater amount in the portion swimming on the surface than in the sinking one. It is, likewise, partially dissolved in water, to which, even at a low temperature, it imparts a golden yellow color. This solution, on being evaporated, leaves behind the resin, endowed with all the properties stated above.

I now return to the oxidation of the raw oil. The tenacious and membranous mass having been transformed into resin, the violence of the boiling diminishes, and it now becomes necessary to employ concentrated nitric acid, which, from time to time, is renewed, not farther to decompose the acids which it already contains, and to accelerate the oxidation of the remainder. Finally, the moment arrives in which the resin is suddenly transformed into an oily mass, which swims on the surface of the liquid, being covered with a great quantity of small vesicles, which gradually almost completely disappear. It is now allowed to cool; the liquid contains suberic and pimelinic acids; on its surface swims a light yellow substance of a highly crystalline fracture. It is fused with nitric acid in a water-bath until it becomes almost white, which takes place when the oleïc acid, which it pertinaciously retains, is almost entirely destroyed. The adhering nitric acid is then removed by fusing it with water, and it is dissolved in boiling alcohol, from which, on cooling, it separates with a perfectly white color. This substance crystallises from alcohol in the form of small stars, of the lustre of mother of pearl, which, however, are very soon transformed into voluminous masses resembling cauliflowers, collected on a filter, and fused in a water-bath. On cooling it presents a mass of a waxy lustre, consisting, in its interior, of large silky crystalline lamellæ. It is proved by analysis that this body is identical with the margaric acid which is obtained by saponifying linseed oil; like it, it contracts violently in passing from the liquid into the solid crystalline state. On analysis the following results were obtained:—

I. 0·3712 grammes substance gave 1·0312 grammes carbonic acid and 0·4207 grammes water.

II. 0·3158 grammes substance gave 0·8764 grammes carbonic acid and 0·3584 grammes water.

A third analysis was made with the acid,

after having saponified it, separated again, washed and dried.

III. 0·2860 grammes substance gave 0·7906 grammes carbonic acid and 0·3235 grammes water.

To these analyses correspond the following per cent.:—

	I.	II.	III.	Mean.
Carbon..	75·75	75·64	75·38	75·59
Hydrogen	12·58	12·60	12·55	12·58
Oxygen..	11·67	11·76	12·07	11·83
	100·00	100·00	100·00	100·00

These analyses of substances prepared in various manners coincide, all leading to the formula—

$$C^{34}\,H^{29}\,O^{3} + HO,$$

which expresses the composition of margaric acid obtained from other fatty bodies. The identity of the margaric acid prepared from linseed oil with the common one, may therefore be considered as well established.

It now remains to describe the products of oxidation of the oleate of acroleine. Among these oxalic acid first makes its appearance, being, however, invariably accompanied by suberic acid; but its quantity is so inconsiderable, that we may suppose that the basic portion of the linseed oil, the acroleine, is the first to be oxidised. The oxalic acid is separated from the suberic acid by merely combining it with lime.

If, after the first action is passed over, the mother-liquors saturated with oxalic acid be poured off, and the oxidation with nitric acid recommenced, an enormous quantity of suberic acid is obtained. It must be removed with the mother-liquor at intervals of from four to five hours, or much is lost, for, by the continued action of nitric acid, it is soon attacked and transformed into a very volatile oil, which may be recognised by its odor of butyric acid. The suberic acid obtained in this manner is yellow, and contaminated by the resin, which is mixed with all the products of oxidation of linseed oil. It must be recrystallised three or four times, to remove as much as possible of the nitric acid. It is then dried between blotting paper, fused at a gentle heat and very rapidly distilled; in that manner but little charcoal is left behind in the retort. The product of the distillation being contaminated by a black resinous matter, the odor of which reminds one of that of acetone, is dissolved in boiling water, treated with animal charcoal, previously washed by hydrochloric acid, and filtered. From the liquid which runs off, on cooling, the suberic acid separates, exhibiting small needles, which unite into masses resembling a cauliflower. The analyses both of the salt itself and of the silver salt confirm the formula:—$C^{8}\,H^{6}\,O^{4} + HO$ for the hydrated suberic acid, and $C^{8}\,H^{6}\,O^{3} + AgO$ for the silver salt. The fused acid, on solidifying crystallises into long silky needles.

The mother-liquor of the suberic acid containing nitric acid on being evaporated, a new quantity of this acid is first obtained, and then an acid is produced representing crystals of large grains or hard yellow tables, which are purified by distillation. The analyses of this acid led to the formula:—$C^{7}\,H^{5}\,O^{3} + HO$, which, according to Laurent,* is the expression of pimelinic acid.

The analysis of this acid was confirmed by that of the silver salt.

The products of oxidation of raw linseed oil are, therefore, margaric, oxalic, suberic, and pimelinic acids, and a volatile fatty body of an odor similar to that of butyric acid, which is developed in proportion as the suberic acid is decomposed by concentrated nitric acid.

Let us now examine from which of the constituent parts of raw linseed oil these various products of oxidation are to be derived.

The margaric acid exists in the oil ready formed. By the action of alkalies and nitric acid it is separated from the acroleine combined with it.

The oxalic acid is produced by acroleine; it was known long ago that this acid may be obtained by the oxidation of glycerine.

The suberic acid owes its origin to the oxidation of oleïc acid. Pure oleïc acid, on being treated with nitric acid, becomes violently tumefied, and is transformed into a resin; the mother-liquor contains suberic acid. The unctuous resin thus formed is of a fine lemon-yellow color; on being repeatedly fused with water to remove the nitric acid, and then evaporated in a water-bath until its weight is invariable, it becomes dark brown, exhaling an aromatic odor.

The following results were obtained by analysis:—0·7140 grammes substance gave 1·5443 grammes carbonic acid, and 0·5103 grammes water.

	In per cents.
Carbon.....	58·97
Hydrogen.....	7·94
Oxygen.....	33·09
	100·00

The substance, however, employed in this analysis was not entirely pure; it contained traces of nitric acid. The same substance was saponified by potassa, separated again

* Liebig's *Annalen*, xxviii., p. 26.

by hydrochloric acid, well washed, and dried in a water-bath.

The analysis now furnished the following result:—0·4282 grammes substance gave 1·0223 grammes carbonic acid, and 0·3548 grammes water.

	In hundredths.
Carbon	65·10
Hydrogen	9·20
Oxygen	25·70
	100·00

corresponding to the formula—

$C^8 H^7 O^3$.

We perceive, at one glance, that their expression corresponds to the formula of anhydrous suberic acid, in which one equiv. oxygen is substituted by one equiv. hydrogen—

$C^8 H^7 O^3 — H + O \quad C^8 H^6 O^3$

Anhydrous suberic acid.

The most extraordinary pertinacity with which this substance resists the action of nitric acid is remarkable; it may almost be said that these two bodies are combined. On employing, however, highly concentrated nitric acid it is, nevertheless, finally completely transformed into suberic acid. It is this substance which, of all the products of oxidation of linseed oil, on being treated with alkalies reddens most, wherefore, I feel inclined to believe that this property is derived from that body, and from no other. Notwithstanding the greatest care which I took in preparing the oleïc acid, I still found among the products of its oxidation, besides suberic acid, some oxalic acid, but in a quantity too minute to make us suppose that it originated from the oleïc acid itself, rather than from the imponderable quantities of ether, which cannot be completely separated from it.

The glycerine, on being oxidised, furnishing only oxalic acid, and the oleïc acid only suberic acid, the pimelinic acid, obtained from the last mother-liquor of the violent oxidation of the raw oil, can only have originated from the margaric acid. This was to be proved. But perfectly pure margaric acid, which was slowly oxidised with the strongest nitric acid, furnished no pimelinic acid, but produced succinic acid. Pimelinic acid must therefore, be a body which arises from suberic and succinic acids. If, indeed, succinic acid be mixed with an excess of suberic acid, and the mixture boiled with concentrated nitric acid, on removing by evaporation the excess of the suberic acid most beautiful pimelinic acid is produced, containing no trace of the acids from which it was derived. The pimelinic acid is then formed by a metamorphosis of the suberic acid, in the presence of succinic acid, under the influence of concentrated nitric acid. We may easily account for this passage:—The sum of the elements of 2 equiv. succinic acid, and 1 equiv. suberic acid, contains the elements of 2 equiv. pimelinic acid + 2 equiv. carbon, 1 equiv. hydrogen, and 4 equiv. oxygen, which escape in the state of carbonic acid and water.

2 equiv. succinic acid	$C^8 H^6 O^8$
+ 1 equiv. suberic acid	$C^8 H^7 O^4$
	$C^{16} H^{13} O^{12}$
—2 equiv. pimelinic acid	$C^{14} H^{12} O^8$
	$C^2 H^1 O^4$

By this table we perceive why, on artificially preparing pimelinic acid, but few red vapors are disengaged, if the employed suberic and succinic acids were pure.

CONCLUSIONS.

Linseed oil consists of margaric acid and oleïc acid in equal equivalents combined with acroleine. On oxidation with nitric acid the following are the results:—Margaric acid, oxalic acid, suberic acid, pimelinic acid, carbonic acid, and water.

The composition of the oleïc acid of linseed oil differs from that of the oleïc acid obtained from other fatty bodies.

On being oxidised it furnishes suberic acid, which in its turn passes into a volatile fatty body.

Pure margaric acid on oxidisation yields succinic acid, but gives neither suberic nor pimelinic acid.

Pimelinic acid is formed by the metamorphosis of the suberic acid, in the presence of succinic acid. Between the oleïc and suberic acids stands an intermediate particular fatty substance of most peculiar properties. —nt—

MEMOIR ON LACTIC ACID.*

BY M. J. PELOUZE.

THE memoir which I have the honor to present to the Academy may be considered as a continuation of the work on lactic acid which M. J. Gay-Lussac, and myself submitted to it in 1833. Since that time, the history of lactic acid has, doubtless, been enriched with many important observations; but the works of which it has formed the subject have been almost exclusively directed towards its natural state, and the means of producing it artificially. The

* *Comptes Rendus*, Dec. 9, 1844.

study of its chemical properties has been neglected on the contrary, and that which we know, is far from being equal to our knowledge concerning a great number of organic bodies indisputably less worthy of interest. However, lactic acid is one of the most abundant substances in the animal economy and in vegetables, in which it seems, sometimes, to perform an important part. It exists naturally in milk, and is abundantly formed during the spontaneous acescence of that fluid. MM. Bernard, and Barreswil, have just proved its existence in the gastric juice.

It results from some observations of M. Gobley, not yet published, that it is also found in the free state in the white of egg.

It is met with in all the vegetable juices, whose spirituous fermentation has not followed a regular progress, in the spoiled and fermented farinas of all cereals; in the tanners' liquor *(jusée)*, and in the sugar-water of starchmakers; it is formed in abundance when a sugar, to whatever class belonging, is put in contact, at a temperature of 68° to 86° F., with an alkaline and an earthy carbonate and a ferment, particularly with the caseous matter of milk.

The butyric fermentation, which immediately follows the lactic fermentation of sugars, gums, and starch, increases the interest which lactic acid merits, in both chemical and physiological points of view.

Lactic acid is a colorless liquid, soluble in all proportions in water and alcohol, of an acid taste, almost insupportable, it is so powerful and pungent. Its composition, which is very simple, was determined at the same period (1833) by Mitscherlich and Liebig on one hand, and by M. Gay-Lussac and myself on the other hand. Its formula is:—

$$C^6\,H^6\,O^6 = C^6\,H^5\,O^5 + HO.$$

These numbers express one equivalent of monohydrated acid:—

(C = 75; H = 12·5; O = 100.)

The action of heat on lactic acid is very remarkable.

At a temperature near 266 F., but which cannot be exceeded without inconvenience, this acid distils over a colorless liquid, which is no other than water holding in solution a small quantity of lactic acid. After a very considerable lapse of time, when no more water is disengaged, the operation is terminated; the residue acquires a somewhat yellow tint; it becomes solid, easily fusible, of excessive bitterness, almost insoluble in water, and very soluble, on the contrary, in alcohol and ether. Not a trace of any gas, whatever, is disengaged in this reaction, which consists in a simple dishydratation of lactic acid.

The composition and properties, of the solid residue of which I have just spoken, leave no doubt as to its true nature. It is anhydrous lactic acid, as it exists in the lactates. Indeed, it is formed of six equivalents of carbon, five of hydrogen, and five of oxygen. Prolonged ebullition with water, or long exposure in that liquid cold, or in a damp atmosphere, converts it into ordinary lactic acid. This conversion is, so to speak, instantaneous under the influence of soluble bases, and in this respect my experiments have been very numerous. Milk of lime gives, with insoluble lactic acid, the ordinary salt, with its six equivalents of water. I have determined its capacity of saturation, and have extracted the monohydrated acid. Lactic acid must, therefore, be added to the very limited number of acids which lose their saline water by heat, that is to say, that proportion of water which they exchange for bases in forming salts. I noticed this property twelve years ago, in maleïc and paramaleïc acids, and, more lately, I have observed it, with Professor Liebig, in œnanthic acid.

The dishydratation of lactic acid is effected, however, much more slowly than that of the foregoing acids, and this is the reason, no doubt, that it has escaped us. I will add, also, that this phenomenon is manifested at a temperature which is almost always employed, without hesitation, for drying most organic matters, and this observation shows the circumspection with which it is necessary to proceed in experiments of this kind.

Anhydrous lactic acid, submitted to the action of dry ammoniacal gas, absorbs one equivalent of it, and forms a peculiar compound ($C^6\,H^3\,O^5$, $H^3\,N$), in which the ammonia does not cease to be perceptible, by means of the reagents usually employed for detecting its presence.

I have proved that anhydrous œnanthic acid also forms a compound of the same order; a compound which may, perhaps, be compared with the amiduretted salts.

Lactic acid or, more correctly, the anhydrous acid, of which I have just spoken, resists the action of heat to nearly 480° F. At this period gases, which, before, were not developed, now begin to be disengaged. They consist of oxide of carbon, mixed with only four or five hundredths of its volume of carbonic acid; the proportion of the latter gas gradually augments and, towards the end of the experiment, its bulk reaches to about half that of the oxide of carbon. It does not appear that any carburetted hydrogen is formed.

Several volatile substances are developed.

at the same time as the gas, and are condensed in the receiver. I will speak first of a beautiful crystallisable matter which M. J. Gay-Lussac and I described under the name of *anhydrous lactic* acid, an improper name which I propose to give to the matter described above, and which presents, indeed, the composition of the organic matter of the most completely dried lactates. This crystallised matter, which I will henceforward call *lactide* (a denomination already proposed by M. Gerhardt), has performed an important part in several discussions relative to the constitution of the organic acids; as, moreover, it has no analogy in chemistry, and as it is curious in many respects, I have thought it would be well to control our former analyses, and to again verify its conversion into ordinary lactic acid. In both these respects I have proved the accuracy of our former experiments. The formula for lactide is $C^6 H^4 O^4$, that is, lactic acid minus two equivalents of water, and this composition explains at once its formation during the dry distillation of that acid, and its conversion into lactic acid itself, under the influence of water or hydrated bases. There exist, therefore, two neutral matters, $C^6 H^5 O^5$ and $C^6 H^4 O^4$, which are not lactic acid, but which are derived under the loss of 1 or 2 equivalents of water, which this salt experiences when distilling, and which are capable of regenerating it either directly or indirectly, by absorbing precisely the 1 or 2 equivalents of water which are required for this purpose.

The hydrated acid produced by the action of water or humid air on lactide is limpid, completely colorless, and of perfect purity.

The very peculiar composition of lactide led me to expect that, in acting on ammoniacal gas, it would give rise to a body of the amide series, for the latter are, analytically speaking, only anhydrous ammoniacal salts, from which the elements of 1 equivalent of water have been subtracted. The result answered my expectation.

When the body $C^6 H^4 O^4$ is exposed to the action of ammoniacal gas, it gradually liquefies, and absorbs this gas with a disengagement of heat. A new substance results from it, whose composition was established by analysis and synthesis.

Lactamide (which is the name that I propose to give it) is formed of 1 equivalent of lactide, $C^6 H^4 O^4$, and of 1 equivalent of ammonia; but this ammonia exists in it in the latent state, as in bodies of the amide series. The aqueous acids and the alkalies disengage it from it only with heat, and very slowly; it dissolves in water without undergoing any alteration, and it is only under a pressure corresponding to a temperature above 212° F. that it is converted into true lactate of ammonia. Milk of lime decomposes it, gradually disengaging from it the ammonia, and oxalic acid separates ordinary lactic acid from the calcareous salt.

Lactamide does not appear capable of combining with bases or with acids; it has no action on colored reagents. Alcohol dissolves it in considerable proportion, and allows it to deposit by concentration or by cooling under the form of fine crystals of perfect whiteness and transparence, and whose primitive form is a right rectangular prism.

Independently of lactide, whose existence I have just noticed in the products of the distillation of lactic acid, the latter also gives by decomposition another substance, which I propose to call *lactone*, because it appears to me to be to lactic acid what acetone is to acetic acid.

It is obtained pure by submitting to a moderate temperature the products of the distillation of lactic acid. When the temperature has reached 266° F., the distillation is stopped; the distilled liquid is washed with small quantities of water; one portion is dissolved in this water, another floats on its surface: it is removed, and put in contact for several days with chloride of calcium. It is again finally submitted to distillation.

The lactone which arises from the decomposition of lactic acid, is hydrated; its formula is

$$C^{10} H^8 O^4 HO.$$

There is nothing surprising in this, for it is formed in presence of a considerable quantity of water. It has so great an affinity for that liquid, that it may be rectified over chloride of calcium, without being dried. I have already said that to obtain it anhydrous, it must be allowed to remain for several days over that substance.

Anhydrous lactone is presented under the form of a colorless or slightly-yellowish liquid, whose color gradually becomes deeper in contact with the air. It has a hot and burning taste, and a peculiar aromatic odor. It is lighter than water, and dissolves in it in considerable quantity. It burns with facility, producing a beautiful blue flame, very feeble, and without any deposit of carbon. It enters into ebullition at about 197° F.

The formula of anhydrous lactone is $C^{10} H^8 O^4$, which represents 2 equivalents of lactic acid, from which 2 equivalents of carbonic acid and 2 equivalents of water have been removed ($C^{12} H^{10} O^{10} = C^{10} H^8 O^4 + 2CO^2 + 2HO$), or two equivalents of lactide $2C^6 H^4 O^4 = C^{12} H^8 O^8$ deprived of 2 equivalents of carbonic acid only. Its formation during

the distillation of free lactic acid has in it nothing extraordinary. Professor Liebig and I proved, a long time ago, that acetic acid gives great quantities of acetone by the action of heat alone, and this is another point of resemblance between acetone and lactone.

Among the gaseous or volatile products of the dry distillation of lactic acid, I have already noticed oxide of carbon, carbonic acid, water, a small quantity of ordinary lactic acid, lactide, and lactone. I may add that there is also formed a little acetone, and an odorous liquid insoluble in water. The distillation, commenced at 480° F., is complete only at 570° F.; at this latter term there remains in the distillatory vessel only charcoal very difficult of incineration, whose weight represents about $\frac{1}{20}$th part of the acid employed.

In an experiment which lasted about 8 hours, 80 grammes of monohydrated lactic acid (C^6 H^6 O^6), distilled at a temperature between 480° F., and 570° F., gave 48 grammes of liquid substances,* 5·5 of carbon, and 26·5 of gas, of which oxide of carbon forms the largest proportion.

A peculiar circumstance, which it may not be useless to mention, enabled me to discover a very interesting property of lactic acid. A certain quantity of this acid had been given to me by M. Gélis, who had extracted it from the lactate of lime arising from the fermentation of glucose in presence of chalk and caseum. This acid was decomposed at a lower temperature than the acid produced in similar circumstances with sugar of milk, and, which was no less surprising to me, it gave oxide of carbon entirely free from carbonic acid, for it did not cause the slightest turbidness in lime-water, and it was only after having disengaged considerable quantities of the former of these gases, that, finally, it furnished also a bitter carbonic acid.

I for a long time believed in the existence of two lactic acids, but I finally ascertained that this difference in the products of the action of heat was owing to the presence of a small quantity of sulphuric acid in the acid prepared by M. Gélis. This chemist had obtained the acid of which I have just spoken, by decomposing lactate of lime by a slight excess of sulphuric acid, evaporating and redissolving the residue by alcohol, in order to free it from sulphate of lime.

The fact once well proved, I could always reproduce oxide of carbon with acetic acid and the lactates, arising from any source whatever.

If lactic acid or a lactate, that of iron, for example, be mixed with five or six times its weight of sulphuric acid, and if the mixture be exposed to a gentle temperature, a brisk effervescence is soon manifested in the mass; it is due to an abundant disengagement of pure oxide of carbon. The mixture is colored of a deep brown; if it be treated by water, when the disengagement of gas has ceased, a black matter is separated, which resembles ulmic acid in appearance.

The reaction is so clear and easy, that I hesitate not to propose it as one of the best modes of preparing oxide of carbon.

6 grammes of crystallised lactate of iron, representing 3·775 of monohydrated acid, give very nearly 1 litre of oxide of carbon. This proportion represents nearly $\frac{1}{4}$ of the lactic acid itself. The black matter of which I have just spoken constitutes about another third, and, without affirming it, I think that water is the third substance formed by the action of sulphuric acid on lactic acid.

To ascertain whether the formation of oxide of carbon should be attributed to the decomposition, by sulphuric acid, of a certain quantity of formic acid, I diluted the sulphuric acid with such a proportion of water, that it was no longer capable of acting on formic acid; but in no case was I able to prove the presence of this latter body.

It would, doubtless, be very difficult to find a sufficient explanation of this singular mode of decomposition of lactic acid, but the fact is, nevertheless, very interesting, and very worthy the attention of chemists.

LACTATES.

I have little to add to the history of the lactates; M. Gay-Lussac and I presented it, in some detail in the memoir to which I have already alluded.

The lactates of iron, magnesia, and zinc contain 3 equivalents of water of crystallisation, are sparingly soluble, and are, doubtless, isomorphous; however, I would not dare to make this latter assertion positively,

* These liquid products deposit, on cooling, variable quantities of lactide. When it is mixed with the products of the decomposition of lactic acid, it seems to acidify in the humid air much more quickly than when it is pure. After some days the distilled liquids, treated by water, are almost entirely dissolved in it; there is precipitated only a small quantity of solid matter almost entirely formed of anhydrous lactic acid. The filtered liquid may be said to contain only ordinary lactic acid.

The hydratation of lactide is much more easy than that of anhydrous lactic acid; this explains why it gives only very small quantities of this latter body.

because the crystals which these three salts give are very small, and it is very difficult to measure their angles.

Lactate of lime contains 6 equivalents of water, and it is sparingly soluble in that liquid, but it dissolves in considerable proportion in alcohol, from which ether separates it under the form of a white, crystalline precipitate. The alcoholic solution of lactate of lime is precipitated by phosphoric acid, whilst, in water, lactic acid displaces, on the contrary, phosphoric acid from phosphate of lime.

Lactate of ammonia is deliquescent and uncrystallisable.

Lactic acid forms, with oxide of copper, a beautiful blue salt, whose primitive form is a right rectangular prism of the third system.

This salt crystallises with facility; it contains 2 equivalents of water, which it loses at 248° F. When it is submitted to the action of heat, it disengages a mixture of oxide of carbon and carbonic acid; the copper is speedily reduced to the metallic state, and the matter contained in the distillatory vessels enters into fusion, and is decomposed, furnishing the same products as the distillation of free lactic acid. When the last third of the distilled products is put in contact with water, it sometimes happens that they are suddenly solidified, and deposit a matter which is no other than lactide.

This lactide is very soluble in lactone, and to this circumstance must be attributed the small quantity of crystallisable matter which is often obtained among the products of the distillation of free lactic acid or lactate of copper.

Lactate of copper, previously deprived of its 2 equivalents of water of crystallisation, gave me, by dry distillation, 41 hundredths of liquid substances, holding in solution a great quantity of lactide, 29·5 of metallic copper, 3·3 of charcoal, and 26·2 of gas (CO and CO^2).

Lactate of copper presents a peculiarity worthy of attention; it sometimes crystallises in large prisms of a deep green, which do not differ, either in form or in composition, from the blue salt of which I have just spoken. Redissolved in water, these green crystals are changed into blue crystals, the formula of both is CuO, C^6 H^5 O^5, 2HO.

Lactate of copper, treated by an excess of caustic potassa, gives rise to a deep blue liquor; with lime, one part of the oxide of copper is precipitated, another part remains in solution, even when a considerable excess of oxide is present. In similar conditions acetate of copper is always entirely precipitated, and the liquor in which the reaction is affected, is perfectly colorless. This character enables us clearly to distinguish lactic acid from acetic acid in the secretions in which these two acids do not exist simultaneously.

Tartaric acid, which prevents, like the sugars and lactic acid, the precipitation of oxide of copper by caustic potassa, offers no obstacle to the complete purification of this oxide by milk of lime. It is the same with paratartaric and citric acids. The salts of copper are completely precipitated by hydrate of lime, notwithstanding the presence of these acids.

Lactic acid and the sugars from which it is derived, are the only bodies, among those which I have just mentioned, in the presence of which oxide of copper is not precipitated, or is only incompletely precipitated by lime. With the others the precipitation is always complete. I notice these circumstances, because they enable us to distinguish lactic acid from some of the bodies which sometimes accompany it.

In treating of a substance so important as lactic acid, no observation, provided that it be accurate, should appear devoid of interest. It may be conceived, for example, how useful must be such properties as those of which I have just spoken, for accurately detecting the presence of lactic acid in a secretion like the gastric juice, in which it exists only in very minute proportions, and mixed with many different materials.

I will here add, without fear of contradiction, that it is to be regretted that the very numerous analyses which have been made of the organs of secretion of animals, have not always been preceded by a more profound examination of the constituent principles of these organs and of these secretions. At a period when the characteristic properties of lactic acid were not sufficiently known, the existence of this acid in the gastric juice was, by turns, asserted and denied. It is in consequence of some of the experiments related in this memoir, and of some others which are their own, that MM. Bernard and Barreswil have just solved, in a manner which appears definitive, the so-long-debated question as to the true cause of the acidity of the gastric juice.

KRÜGER'S REMARKS ON CUPRIC ACID.*

BY H. ROSE.

Cuprate of lime may be obtained by mixing well prepared chloride of lime with water, and then adding a solution of nitrate

* Sitting of the Royal Academy of Sciences of Sitzung, Berlin, 18th July, 1844.

of copper. A green precipitate is at first thrown down, which soon becomes darker, and finally presents a fine crimson-red color. During this process oxygen is disengaged; this disengagement continues for several weeks, the color of the precipitate, at the same time, being gradually changed to blue, and is finally wholly converted into hydrated oxide of copper.

M. Krüger did not succeed in transferring the acid from the cuprate of lime to other bases, by treating it with solutions of other salts. Nor could the combination be washed, either on a filter, or even in close vessels; oxygen invariably escaped, and hydrated oxide of copper remained. It being impossible to remove the admixed chloride of calcium, chlorine was disengaged from it by acids.

Cuprate of baryta can be obtained in a similar manner. It always forms a deep red precipitate.

If hydrated oxide of copper is divided by a solution of caustic potassa or soda, and if a current of chlorine, with a very moderate celerity passed through, the vessel being, at the same time, kept as cold as possible, the liquid, at first, becomes of a dirty green color; but on interrupting the current of chlorine, it is changed into a fine red color, resembling that of ferrate of potassa. This color, however, of the cuprate of potassa, or soda, soon disappears, oxygen very rapidly escapes, and the black precipitate of oxide of copper, which still contains hypochlorous acid, thrown down.

The alkaline cuprate cannot be made constant either by concentration or dilution of the caustic liquor; nor can it be formed by a galvanic pile, by a process similar to that employed in the production of ferrate of potassa. —nt—

ON THE BEHAVIOR OF SULPHOCYANIDES OF METALLIC SULPHURETS AT A HIGH TEMPERATURE.*

BY C. VOELKEL.

A LITTLE treatise on this subject, by Gerhardt, appeared lately in the *Comptes Rendus*, No. 4, 1844, and THE CHEMIST, June, 1844, in which he maintains that the sulphocyanides of metallic sulphurets are resolved at a high temperature exactly into a metallic sulphuret of bisulphuret of carbon and mellon, stating, at the same time, that I *confirmed*† the formula of mellon to be $C^3 N^4$, that it is, however, impossible to substitute any other for the same. If it results from the equation:—

$2 (C^2 N^3 S + MS) = CS^2 + 2 MS + C^3 N^4$,

that by heating a sulphocyanide of a metallic sulphuret a body might be formed of the formula $C^3 N^4$, or $C^6 N^8$, this by no means proves that it really is produced. But, it seems that M. Gerhardt was so convinced of the possibility, that he deemed it perfectly superfluous either to make experiments on that score himself, or to consult the observations of others.

According to Wöhler's* and Meitzendorff's† investigations, and my own, the case is totally different. We all, and especially Meitzendorff, who investigated a great many sulphocyanides of metallic sulphurets, exposing them to a high temperature, found that by heating them, bisulphuret of carbon, nitrogen and cyanogen are formed, whereas in the residue, besides the metallic sulphuret, a small quantity of another yellow body remains, which was formerly considered to be the so-called radical mellon, $C^6 N^8$, which, however, as I showed in my second treatise (Poggend. *Annalen*, vol. lviii., p. 153), is not of that composition, for, in addition to this it contains one or two per cent. of hydrogen, and yields by analysis from four to eight per cent. less of carbon than, according to the formula $C^6 N^8$, it ought to furnish. My inferences from this are the reverse of those of Gerhardt, namely, that the body is not composed of $C^6 N^8$, and notwithstanding Gerhardt says that it is not possible to substitute another, I do not alter my opinion, based on facts. I intend to explain the formation of the so-called mellon in a manner which is in accordance with analysis, unlike M. Gerhardt, who has no reasons.

This body, on being analysed, yielding hydrogen, can only be a product of the decomposition of a substance containing hydrogen, which, however, being collected only in small quantities, by heating the sulphocyanides of a metallic sulphuret, is not an essential but merely an accidental product. Yet from the circumstance that almost all sulphocyanides of metallic sulphurets, on being heated yield hydrogen, it appears that these bodies, even on being dried at a higher temperature, retain a small quantity of this gas, probably as water. We may easily be convinced of the truth of this

* Poggendorff's *Annalen*, ix., 1844.

† [In the memoir referred to (THE CHEMIST, June, 1844) M. Gerhardt expressly states that "Voelkel has *disputed* the accuracy of this formula." The discrepancy is owing, probably, to a typographical error in the present article.—EDS. CHEM.]

* Gilbert's *Annalen*, vol. lxix., p. 272.

† Poggendorff's *Annalen*, vol, lvi., p. 63.

fact by heating a sulphocyanide of a metallic sulphuret; for instance, a silver salt $C^2 N^2S + AgS$; by exposing it for rather a long time to a temperature of 392° F., and afterwards to a still greater heat, a small quantity of hydrosulphate of sulphocyanogen is disengaged. By the decomposition of this body the yellow substance containing hydrogen is formed, the composition of which may vary in proportion as it is less or more heated, and as the metallic sulphuret remaining itself reacts upon it; and this is proved likewise by analysis. The sulphocyanide of a metallic sulphuret by itself, on being exposed to a high temperature gives only bisulphuret of carbon, nitrogen, cyanogen, and a metallic sulphuret—

$$2\,(C^2N^2S + MS) = CS^2 + C^3N^3 + N + 2MS.$$

It might be objected that the body $C^3 N^4$, or $C^6 N^8$, is still formed by the first action of heat on the sulphocyanides of metallic sulphurets, and is only at a higher temperature resolved into cyanogen and nitrogen. It appears, however, from observations of Wöhler and Meitzendorff that such is not the case. Wöhler found that on gently heating the sulphocyanide of sulphuret of mercury, bisulphuret of carbon and nitrogen escaped, and that the residue contained carbon and nitrogen in the proportion of cyanogen. Meitzendorff, moreover, found likewise that on heating the sulphocyanides of metallic sulphurets, among the volatile products at the commencement cyanogen and nitrogen are noticed. From this we learn that the body $C^3 N^4$ does not appear among the products of decomposition of the sulphocyanides of metallic sulphurets, or, if it be formed, it is decomposed again at a temperature only a little higher than that which produced it. By no means, however, can it be contained in the yellow body remaining after the sulphuret of mercury has been volatilised by calcining the sulphocyanide of sulphuret of mercury, or in the residue left after calcining hydrosulphate of hypersulphocyanogen, or the hydrosulphuret of ammonia and sulphocyanogen, these latter bodies being only decomposed by strong calcination, and then disengaging cyanogen, &c.; whereas the sulphocyanides of metallic sulphurets in the operation of calcination yield cyanogen and nitrogen. The observation made with the hydrosulphuret of ammonia and of sulphocyanogen, may be repeated with these bodies. The decomposition begins at a lower temperature, but can be completed only when a strong heat is applied.

Gerhardt states, moreover, that on heating per-sulphocyanic acid, mellon is formed according to the following equation:—

$$2C^2 N^2 H^2 S^3 = CS^2 + 2H^2 S + C^3 N^4 + S^2.$$

From my investigations (third treatise) it appears, however, that this is not the case. It seems that Gerhardt has made no experiments with regard to this matter, and that he has arranged the whole subject at his writing-desk.

The same must be said of his statements relating to the behavior of the hydrosulphuret of ammonia and sulphocyanogen under a high temperature. He says, "this salt furnishes bisulphuret of carbon, sulphuretted hydrogen, ammonia, and a grayish residue, which Liebig designated by the name of *melam*. This body is, according to my experiments, only a mixture of mellon and the alkaloïd which the celebrated chemist of Giessen called melamin; it possesses all the characters peculiar to such a compound. If melamin contains $C^3 H^6 N^6$, we have—

$$\text{I. } 4CH, NS, NH^3 = CS^2 + 2H^2 S + 2NH^3 + C^3 H^6 N^6.$$

We may account for the presence of mellon in this residue by the circumstance that melamin itself, on being exposed to a high temperature, is resolved into mellon and ammonia.

$$\text{II. } C^3 H^6 N^6 = 2NH^3 + C^3 N^4."$$

The objection made in my third treatise against the equation of Liebig is still more forcible against the above, as under I.; for, according to all observations, on heating the hydrosulphuret of ammonia of sulphocyanogen, a great quantity of free ammonia escapes; but in Gerhardt's equation we find no free ammonia at all, the two atoms of ammonia forming, with two atoms of sulphuretted hydrogen, hydrosulphuret of ammonia. We are at a loss what to say, on reading such things, of the incorrectness of which one might suppose the author ought to have been aware in writing them.

As regards the statement, that the so-called melam $C^6 N^{11} H^9$, which according to my experiments is nothing but impure *polien*, being a mixture of melamin and mellon, it is really more than one could have believed. Melam, according to Liebig, is totally insoluble in water, and according to my observations with reference to *polien*, most sparingly soluble, whereas melamin, obtained by Liebig, by treating perfectly washed raw melam with potassa, is rather easily soluble in water. Hence, according to Gerhardt, a body insoluble in water should be a mixture of bodies, one of which is easily soluble and the other insoluble!! How is it possible to be so inconsiderate as to write thus, nay, to maintain to have found it?

The manner in which Gerhardt cultivates the science is peculiar to himself. It is known that by concordant analyses of Liebig

and Knapp, it appeared that ammelide has the following composition: $C^6 N^9 H^9 O^8$, or $C^4 N^6 H^6 O^2$. It is the bad fortune of this formula not to suit Gerhardt; he therefore says, in a short and dictatorial sentence, the formula which Liebig gives to ammelide will not be retained.

He has no mercy for Berzelius's theory of organic radicals; he calls it a vague and confused system, substituting for it another, by which, at the writing desk, we may without trouble arrive at excellent results, several of which, in addition to those quoted, are met with in the said treatise.

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ON THE METEOR OF KLEIN-WENDEN.*

BY ALEXANDER DE HUMBOLDT.

We hope to enhance the interest of this sitting by mentioning the remarkable phenomenon of a meteor which was lately communicated to M. de Humboldt, corroborated by numerous certificates, collected with laudable circumspection by the landrath Baron de Byla. The meteor, from which a portion has already been separated, as now before us, weighs 5lbs. 23½ loth (5lbs. 13oz.), and fell on the 16th Sept., 1843, at half past 4, p.m., in a potatoe field near the village of Klein-Wenden, north-east of Almenhausen, in the southern part of Wipperthal between Klein-Wenden and Münchenlohra. To the detailed certificates of M. de Byla, of Nordhausen, is added a delineation of the situation of the whole neighborhood, made by the master architect of the roads, M. Monecke. This aërolith differs from the ordinary ones which descend from a small blackish cloud, reports similar to those of small arms, being perceived at the same time: when the aërolith of Klein-Wenden fell, the sky was totally serene—neither cloud, nor any phenomenon of light, or of a fiery globe, were visible. A tremendous report, like that of a large cannon, was heard (less in Erfurt), and a noise and crackling sound succeeded, which may be compared to that produced by a wagon laden with stones when put in rapid motion. The stone was seen falling from south-east to north-west; it sunk into the dry soil about four or five inches, and (as has been invariably noticed in other cases) was so hot that it could not be touched for some minutes. One stone only was found, though it was at first hoped that at Almenhausen another stone might have descended, the crackling having been loudest there. The meteor of Klein-Wenden has the remarkable quadrilateral prismatoïdal form, which has been noticed by Schreibers in a great many meteors which have fallen in very distant countries, and at different periods. When found it was lying with the broad base on the ground, its oblique surface presenting an almost pyramidal acumination, being directed upwards. A chemical analysis of this little asteroïd, which descended from the upper regions of the universe has not yet been made. Mr. G. Rose finds a striking resemblance between this meteor and that of Erxleben. That of Klein-Wenden contains a grayish-white, fine granular principal mass, in which the nickel-iron is disseminated mostly in small grains, and but rarely in large ones. Besides these, isolated brownish-gray grains, of the size of peas, and of unequal fracture, are seen. Mr. G. Rose observed that the principal mass, on being treated with acids, was converted into a species of gelatine. This aërolith, together with the documents on the manner of its descent, will be kept in the rich collection of meteors already in the possession of the Royal Mineral Cabinet.

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* Read before the Royal Academy of Sciences of Sitzung, Berlin, Jan., 1844.

CHEMICAL EXAMINATION OF A METEOR WHICH FELL ON THE 16TH OF SEPTEMBER, 1843, NEAR KLEIN-WENDEN, IN THE CIRCLE NORDHAUSEN.*

BY DR. RAMMELSBERG.

The meteor of Klein-Wenden belongs to the great class of those remarkable bodies which are characterised by their amount of metallic iron with some nickel admixed, another less numerous class of them being deprived of these metals, to which belong, for instance, the meteors of Alais, Juvenas, Ionzac, Stannern, Byalistock, Lontalax, &c.

The general mass of the investigated stone at the first glance, appears to be of a gray color, but on examining it with a lens, we very clearly distinguish yellowish-green transparent particles, of the appearance of olivin, and black, shining grains, perfectly resembling the granular augit. There are no crystalline formations, and the magnetic pyrites is a brownish color.

The specific gravity of this stone is 3·7006, which almost coincides with that of

* An extract read before the Royal Academy of Sciences of Berlin, June, 1844, by G. Rose.

the meteor which fell on the 15th of April, at Erxleben, near Magdeburg, the latter having been weighed by Klaproth, Buchholz, Hausemann, and Strohmeyer, amounting to 3·60—3·64. Meteors being, like our terrestrial rocks, mixtures of several minerals, their chemical examination will be valuable when it points out the minerals of which they are composed, and their relative proportion. To separate them, recourse must be had to mechanical as well as to chemical means; the particles, however, being very finely divided and mixed, in the first we are limited to the use of the magnet, which attracts the nickel-iron, and partially the bisulphuret of iron. For the further separation of the remaining substances, no other means is left but acids; in the employment of which we are guided by the fact, that acids decompose certain silicates (for instance olivin) and separate silica, whereas others (for instance augit, Labradorite) do more or less resist the action of acids. But this method, though in such cases the only one known, furnishes no accurate results, for (to dwell on the instance of the three minerals named) olivin is not very easily decomposed, far less easily, for instance, than the majority of zeolithes; whereas, on the other hand, even when the acid is not highly concentrated, neither boiling nor long digestion being employed, especially that of the Labrador, one portion is always decomposed. Nothing, therefore, remains but to correct the results obtained by calculation, taking as a basis the composition of the minerals here occurring, ascertained in another way.

The analysis was performed according to the plan laid down by Berzelius, in his beautiful Treatise on Meteors.

In two experiments were obtained 18·37 and 20·34 per cent. of magnetical parts by the magnet, the specific gravity of which was = 7·513. The analyses gave—

Iron	88·892
Nickel	10·319
Tin	0·348
Copper	0·212
Sulphur	0·122
Phosphorus	0·107
	100·000

By deducting 0·328 of bisulphuret of iron (magnetkies) the remainder is found to have exactly the composition of meteors of the Pallas mass, as determined by Berzelius.

The portion which was not magnetic, being treated by hydrochloric acid, was found to consist of silicates, of which 48·25 were decomposable, and 51·75 not decomposable.

The former, however, contained the greatest quantity of bisulphuret of iron and some nickel-iron, which being subtracted, the remainder appears to be olivin, and likewise exactly of the composition of the olivin contained in the Pallas mass.

The portion undecomposed by acids, by analysis was found to consist of 1·15 chrome iron, and 50·6 silicates. The latter, deducting 21·46 of still undecomposed olivin, were found to consist of 30·83 per cent. Labrador, and 47·35 per cent. augit.

Finally follow the numeric results, according to which, 100 parts of the meteor consist of—

Nickel iron	22·904
Chrome iron	1·040
Bisulphuret of iron	5·615
Olivin	38·014
Labrador	12·732
Augit	19·704
	100·009

and the composition of these substances is as follows:—

Nickel iron.		Chrome iron.		(Magnetkies) Bisulphuret of iron.	
Iron	88·980	Oxide of chromium	59·85	Iron	62·77
Nickel	10·351	Protoxide of iron	27·93	Sulphur	37·23
Tin	0·349	Magnesia	12·22		100·00
Copper	0·213		100·00		
Phosphorus	0·107				
	100·000				

Olivin.		Augit.	Labrador.	
Silica	39·60	54·64	Silica	52·81
Magnesia	47·37	23·69	Alumina	29·44
Protoxide of iron	10·72	19·66	Lime	12·46
Protoxide of manganese	0·19	..	Potassa	2·99
Lime	2·12	2·01	Soda	2·30
	100·00	100·00		100·00

On the whole, therefore, the meteor contains—

Sulphur	2·09
Phosphorus	0·02
Iron	23·90
Nickel	2·37
Tin	0·08
Copper	0·05
Oxide of chromium	0·62
Silica	33·03
Magnesia	23·64
Protoxide of iron	6·90
Alumina	3·75
Lime	2·83
Protoxide of manganese	0·07
Potassa	0·38
Soda	0·28
	100·01

—nt—

ON RESPIRATION.

BY MAGNUS.

At the sitting of the Royal Academy of Berlin, June 17, 1844, Magnus communicated some remarks on respiration. A short time back Gay-Lussac submitted* to the Academy of Sciences at Paris, a critical review of a work on the gases contained in the blood, viz., oxygen, hydrogen, and nitrogen, published by Magnus, seven years ago. According to this review, which, however, contains no new experiments, the theory of the function of respiration established as the result of that work, would be destitute of any sure experimental basis, and must be considered as a mere speculation, or as derived from experiments which are rather calulated to prove the reverse of the inferences deduced from them.

The various theories relative to respiration, may be comprised under the following heads :—

1. The older theory of Lavoisier, that the inspired oxygen combines in the lungs with a portion of the carbonic acid of the blood, and escapes as carbonic acid with the air expired.

2. Theories, according to which the inspired oxygen produces in the lungs new combinations, which, afterwards uniting in the capillary vessels only with carbon and nitrogen, are converted into other combinations, and return with the venous blood to the lungs, where, on the accession of oxygen, they are decomposed, and separated as water and carbonic acid. In these theories, which, in opposition to the following, may be called the *chemical* ones, the separation of the carbonic acid and real oxidising process invariably takes place in the lungs, and in this they differ from the following, which may be called the *theories of absorption.*

3. The theory by which it is maintained that the inhaled oxygen is merely absorbed by the blood, and conveyed to the capillary vessels, in which it is employed to oxidise certain substances, converting them into carbonic acid and water, which are received by the blood, and return with it to the lungs, where, coming in contact with the air of the atmosphere, they are excreted ; after which a fresh quantity of oxygen is absorbed, which undergoes the same changes. In addition to these, as a fourth theory, may be considered—

4. The oxygen enters into chemical combination with the blood in the lungs, which is decomposed in the capillary vessels, there producing water and carbonic acid ; these bodies are received by the blood, without, however, forming with it a chemical combination, and are removed in the lungs on coming into contact with air.

After having shown in the above-mentioned work,* that, on passing hydrogen, nitrogen, or atmospheric air through venous blood, carbonic acid is disengaged, the quantity of the latter body remaining the same, whatever kind of gas was employed for that purpose, and that *in vacuo*, on sufficient rarefaction from the same liquid, carbonic acid is likewise obtained, and that, consequently, venous blood contains carbonic acid in a real state of absorption, an assertion which, a short time before, was controverted ; it seemed probable that the function of respiration takes place according to one of the theories of absorption.

To establish this assertion, it is not sufficient to prove that carbonic acid is present in both venous and arterial blood, and that oxygen and nitrogen are found in both, because the blood might contain all the three gases merely in a state of absorption, without their participating in the act of respiration. But if this be the case, the arterial blood should contain as much oxygen, and this body be therein in the same proportion with reference to the carbonic acid, as occurs in venous blood. This circumstance serves as a means for examining whether or not the absorbed gases really act any part in the function of respiration. If, therefore, to show that in

* *Comptes Rendus*, v. xviii., p. 546 ; and The Chemist, June, 1844.

* Poggendorff's *Annalen*, xl., 583.

arterial blood the carbonic acid is displaced by an equal volume of oxygen be still a desideratum, still the proofs given that the proportion of oxygen, relative to carbonic acid, is greater in arterial than in venous blood, fully warrant the conclusion that the respiration is, at least to a certain extent, based on absorption. This alone was the object of that work. Of this manner of arguing, however, Gay-Lussac makes no mention whatever. He recalculates the results of the experiments, and reduces the quantities of gases obtained to the same volume of blood; though, throughout the description of these experiments, it appears that the quantity of air separated from the blood was at one time greater, and at another less; and after having communicated those experiments, it was expressly stated (p. 600), "that, the circumstance that the quantities of air obtained in the several experiments are not entirely concordant, is to be attributed to the circumstance that the above described access given to air by means of a tube screwed on, could not be repeated in all experiments in a like number of times, because the period during which the froth was settling likewise varied."

Gay-Lussac observes, that it is against his arguments that only the tenth part of the carbonic acid was subtracted from the blood. "But," he continues, "it is notwithstanding to be supposed that the fractional quantities of carbonic acid, obtained by Magnus, are proportional to the whole amount of it contained in the various kinds of blood." It must be well remarked that on this ground the whole conclusion is based. It would be true, on the supposition that the air was always under the same circumstances subtracted from the blood; but, from the passage quoted above, it appears that this could not be done. The following was the mode of operating:—a vacuum was produced over the blood, the gases filling it passed into another vessel, and the vacuum thus again restored, the same process was repeated as many times as, on account of the froth remaining on the blood, could be managed. The circumstances, therefore, under which the gases were subtracted from the blood were certainly not always the same. In addition to this, it appears by a simple calculation that, on separating from a liquid several gases absorbed by it, their relative amount in the first portions differs from that occurring in the subsequent ones. A fact, of which no one is better aware than Gay Lussac, he having proved it by experiments, mentioned by himself in the very review in question, on the quantities of oxygen and nitrogen absorbed by water, when exposed to atmospheric air, instituted by himself, in common with M. de Humboldt; and though in those experiments the gases were removed by boiling, it is yet evident, that by this operation nothing else happens than that the pressure under which they exist is removed.*

We cannot conceive, therefore, how Gay-Lussac could maintain that the quantities of carbonic acid obtained in the single experiments should always be proportionate to its whole amount occurring in the blood, it having consisted sometimes of six, but generally of from ten to twelve proportions yielded to the vacuum.

But if this proportion does not exist, the principal argument of the whole critical review falls to the ground. For, to reduce the quantities of the gases obtained to an equal volume of blood, is then incomprehensible.

Gay-Lussac, moreover, takes a mean from the experiments calculated in that manner. It is, however, not judicious to proceed thus, for the blood of the same animal varies at different times, especially if venesection has been performed some days previously, and this is still more the case when various animals are employed in the experiments; but, besides these, if in experimenting with venous blood, some animals furnished blood containing a less amount of air than others did, the sums of gas of all the experiments with venous blood, relative to the blood employed, will be less than that of the experiments with the arterial blood. But it is clear that a smaller quantity of air withdrawn must contain less carbonic acid than a larger one. According to Gay-Lussac, the whole quantity of the arterial blood employed in his experiments drawn from various individuals, of calves and horses, amounts to 608 cubic centimetres; and the gas separated from it to 63·4 cub. centim., or 10·43 per cent. of the blood, whereas the sum of the venous blood amounts to 8·63 cub. centim., from which 66·3 cube centim., or only 7·68 per cent., were separated. It is not surprising that in calculating the amount of carbonic acid, according to these numbers, more of it is found in arterial than in venous blood. But this

* These gentlemen, having disengaged by boiling the air from water of the Seine, and collected it in fractioned portions, found it to be of the following composition:—

Portion.	Oxygen.	Nitrogen.
First.....	23·7 ..	76·3
Second....	27·4 ..	72·6
Third	30·2 ..	69·8
Fourth....	32·5 ..	67·5

—*Journal de Physique par Delamétherie*, and *Gilbert's Annalen*, xx., 133.

result is merely accidental, for it might have happened that much more, or much less, air, might have been separated from the venous blood. The conclusion, therefore, that it appears from these experiments, that less carbonic acid is contained in venous than in arterial blood, is erroneous, because the experiments were neither meant to prove, nor could they prove, in what ratio the gases stand with reference to the blood; but it was merely stated, that in arterial blood the proportion of oxygen, relative to carbonic acid, was greater than in venous blood. And this they indeed show, for on considering the sums of the experiments, according to the very calculation of Gay-Lussac, we find—

	Arterial blood.				Venous blood.			
Carbonic acid..	39·5	or	62·3	per cent.	47·5	or	71·6	per cent.
Oxygen	14·7	,,	23·2	,,	10·1	,,	15·3	,,
Nitrogen.....	9·2	,,	14·5	,,	8·7	,,	13·1	,,
	63·4		100·0		66·3		100·0	

That M. Gay-Lussac did not mention this result is the more striking, it having been expressly stated in the treatise (p. 600). "From this table it appears that the ratio of oxygen, with reference to the carbonic acid, is greater in arterial than in venous blood."

After having shown the real bearing of the proportion on which the whole critical review of Gay-Lussac is chiefly based, we must not leave unnoticed the other remarks contained therein. In the treatise quoted it was proved, so far as it could be done by the data obtained, that the carbonic acid absorbed by the blood is sufficient to yield the whole quantity of the carbonic acid expired by an adult person. Gay-Lussac, however, goes beyond this; based on the suppositions there made (though they might have been too high by double their given amount), he calculates how much oxygen is absorbed by the arterial blood. He is of opinion that this blood must contain not only a volume of oxygen equal to that of the carbonic acid expired, for the production of which it was employed; but, in addition to this, one-third more of oxygen, to generate the water expired; from which it would follow, that one-third more of oxygen is invariably inspired than carbonic is expired. This, however, is opposed to all experiments on respiration.

The circumstance that the substances secreted in the capillary vessels need not to consist merely of carbon and hydrogen, but may likewise contain oxygen, has evidently been lost sight of. The composition of expired and inspired air positively shows that these substances still contain a sufficient quantity of oxygen to form water with the hydrogen, if this liquid was not already separated as such in the capillary vessels.

In a second calculation based on various suppositions, Gay-Lussac arrives at the conclusion that in the act of respiration the venous blood gives up 13 per cent. of its volume of carbonic acid, and that to effect this, 17 per cent. of it must be contained therein. He, however, says—"This (the 17 per cent.) is the minimum of carbonic acid, which venous blood should contain; and as arterial blood also contains it this minimum would be the difference of the quantities of carbonic acid contained in each blood."

It is inconceivable how this could have been asserted, it having been stated just now that the difference of the carbonic acid of the two kinds of blood amounts as a minimum to 13 per cent., in consequence of which, in arterial blood four per cent. would remain as a minimum. By the first calculation Gay-Lussac obtained the result, that arterial blood on coming into contact with atmospheric air may absorb 22·45 per cent. of oxygen in volume, or 24·2 times more than pure water absorbs under similar circumstances. This solubility of oxygen in blood, says Gay-Lussac, is not impossible, but it ought to have been proved, or at least made probable. This reproach, not considering the stated error, is well-founded, and the author is at present occupied with instituting fresh experiments on that score; but in case these do not happen to corroborate the numbers calculated by Gay-Lussac, they will not lessen the validity of the argument, that carbonic acid absorbed in blood acts a part in the respiration.

In concluding his review, Gay-Lussac disproves that the change of color in the venous blood was for the greatest part accounted for by the loss of carbonic acid occurring in the lungs. He cannot admit these views, he says, for two reasons. The first is, that it had not been demonstrated that venous blood yields carbonic acid to the lungs; and yet, as was before shown, this is proved by the very fact that the ratio of carbonic acid, with reference to oxygen, is greater in venous than in arterial blood. The second reason is, that, even granting that carbonic acid is yielded to the lungs,

yet a large portion of it remains in arterial blood, and we are not warranted in explaining this very remarkable change of color by the difference. But in this respect nobody is more convinced than the author himself, for, after having stated in his treatise that the blood on yielding carbonic acid becomes lighter, he says (p. 608)—"On the removal of the carbonic acid, however, venous blood never became of so light a red as arterial blood, but it seems that the absorption of various kinds of gases is likewise productive of various changes of color. It is probable, therefore, that the red color of arterial blood depends not only on the want of carbonic acid, but also on the absorption of oxygen."

At that time the investigations of Peligot on the action of protoxide of nitrogen on the salts of protoxide of iron were not yet known, by which the assertion that liquids by merely absorbing may entirely change their color was fully established. We cannot conceive why Gay-Lussac overlooks this view of the change of color, confining himself to observing, that the author noticed that venous blood becomes lighter by giving up carbonic acid, and considered this as a coöperating means for the production of the change of color.

From these remarks it appears—

I. That the proportion supposed by Gay-Lussac to exist between the portions of carbonic acid and resulting in the experiments instituted, and the whole amount of these gases obtained in the different kinds of blood does not take place, and that therefore the result derived by Gay-Lussac from that circumstance is inadmissible.

II. That from these experiments it evidently appears that the absorbed carbonic acid acts a part in respiration, because they show that the ratio of carbonic acid to oxygen is greater in venous than in arterial blood.

III. That the calculations made by Gay-Lussac relative to the amount of carbonic acid and oxygen existing in blood cannot be considered as correct.

In conclusion, it must be mentioned that immediately after the communication of the said critical review, M. Magendie laid before the Academy the results of an experiment instituted by himself, which serves as a new corroborating proof of the author's views on respiration, for it shows that the carbonic acid absorbed relative to the blood is greater in venous than in arterial blood, being in the former 78 per cent., and in the latter 66 per cent.

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REMARKS ON LANGLAIS'S RECENTLY-DISCOVERED NEW ACID OF SULPHUR.*

BY H. ROSE.

It is known that this acid is formed when a solution of bisulphite of potassa is digested with sulphur at a gentle heat; besides sulphate of potassa, nothing but the potassa salt of this new acid is produced. We cannot conceive what part the sulphur acts in this process, the constituent parts of the bisulphite of potassa alone being sufficient to form the said products, for $2KO, 4SO^2 = KOSO^3 + KOS^3O^5$.

Mr. H. Rose remarked that the anhydrous sulphite of ammonia, on being treated with water, undergoes a similar decomposition. He showed, some time ago, that ammoniacal gas combines with sulphurous acid gas in one proportion only, viz., anhydrous bisulphite of ammonia, $\dot{N}H^3 + 2SO^2$, which on being dissolved in water, may be resolved into sulphate of oxide of ammonium, and a salt composed of oxide of ammonium and the new acid, which Berzelius most appropriately called *trithionic acid*.

Mr. H. Rose moreover remarked, that trithionic acid may probably be formed likewise under other circumstances, for instance, by treating chloride of sulphur with water, in that case sulphuric acid being invariably formed, even then, when an excess of sulphur is dissolved in the chloride of sulphur, or when the combinations of chloride of sulphur SCl^2 with certain chlorides, like chloride of tin ($SnCl^2 + 2\,SCl^2$) come in contact with water.

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ON THE SCHERGIN-PIT.*

BY DR. A. T. MIDDENDORFF.

On the 13th February, in the evening, we arrived in Jakutsk, and owing to the preparatory measures taken by Mr. Schergin, the operations in the pit could be commenced the following day.

On opening the pit it was found that, at the depth of less than one fathom, its entrance was obstructed by ice, which was produced by some rivulets, coming from N.W., filtering through the enclosure. Our labors began with the removal of the ice

* *Bericht, etc. der K. Pr. Akad. d. Wiss zu Berlin.*

* Read before the Imp. Academy of Sciences of St. Petersburgh the 23rd Feb., 1844.

bridge, the thickness of which exceeded nine feet English. It was now open, but to reach the bottom 1¼ cubic fathoms of ice which covered it were again removed. My next care was to open lateral excavations into the sides of the pit. With this view a moveable scaffold was constructed, and fitted to the cavity of the pit, fastened on its four corners by ropes, to be raised or lowered according to necessity, and secured by wedges against lateral movements, and two laborers, each placed in a separate cask of an axle-tree, descended by means of this scaffold to effect the lateral cavities at the requisite height. This labor seemed at first so difficult that I could not hope to finish it before late in the ensuing autumn; but to my great satisfaction, it gradually became very comparatively easy; the method of working was improved, and the men practised, and I am already enabled to lay before the Imperial Academy two results of readings from the thermometer.

The sides of the pit were excavated in eleven places, each time to the extent of 7½ feet English (including the enchasing), to the following depths—382, 350, 300, 250, 200, 150, 100, 50, 20, 15, 7 feet English. The directions given to these excavations was as horizontal as possible, the continuation of the diagonal of the pit.

Each excavation was provided with a wooden bar, in both extremities of which a niche was made, and in each of these a thermometer was put with the globe covered with tallow. The distance from the sides of the pit of the globe of one of the thermometers was 7 feet English, and that of the other 1 foot. Each lateral excavation, as soon as completed (and in the evening of each working day), was most carefully closed with felt.

Only the undermost lateral excavation could be operated on from the bottom of the pit; with reference to the other excavations, it became necessary to secure the scaffold against any tottering motion, thus to be enabled, on being stationed in the casks, to read the thermometers. In that manner an examination of all the thermometers required each time a whole day; but this was the only way to obtain the desired accurate data.

After the completion of the last lateral excavation, the pit was covered again, then reopened after 34 hours, and the first reading of the thermometers was proceeded with. The single excavations, however, besides the said general covering of the pit were, as stated above, as soon as one of them was completely provided with thermometers, and instantly secluded, by means of felt, from the air of the pit, hence the times during which the single excavations were closed, are the following:—

Thermometer of excavation at the depth of	Time during which it was closed. Days.	Hours.
382′	23	0
350	17	0
300	13	0
250	11	0
200	9	0
150	7	0
100	5	0
50	3	0
20	2	0
15	0	34
7	26	0

On the 24th March (old stile) the thermometers were inspected: this operation lasted from 6h. a.m. till 11h. p.m., and the following were the results:—

Lateral excavation.	Thermom. distant 7′ English.	Thermom. distant 1′ English.	External tempera in shade.	Hour of observation.
	No. 526	No. 514		
382′	2° 4′	2° 4′	13° 3′	6½ a.m.
	No. 525	No. 524		
350′	2° 6′	2° 6′	7° 2′	2p.m.
	No. 547	No. 546		
300′	3° 15′	2° 95′	7° 9′	4
	No. 531	No. 529		
250′	3° 45′	3° 4′	8° 6′	5½
	No. 515	No. 534		
200′	4° 0′	4° 5′	10° 1′	7½
	No. 511	No. 516		
150′	4° 6′	4° 55′	10° 6′	8
	No. 532	No. 509		
100′	5° 45′	5° 65′	11° 0′	8½
	No. 510	No. 527		
50′	6° 4′	6° 9′	13° 6′	8¾
	No. 523	No. 518		
20′	8° 8′	9° 1′	14° 5′	9½
	No. 517	No. 520		
15′	10° 5′	8° 8′	14° 5′	9¾
	No. 538	No. 528		
7′	14° 43′	13° 1′	14° 7′	10¾*

At twelve o'clock in the day, the temperature of the air was 6° 8′. The temperature of a collateral excavation at the depth of 7′ from the surface of the earth, No. 508, examined at the same time, was found to be =14° 7′.

The pit was then covered again, and left in this state for eight days, and when exa-

* It is not stated to what kind of thermometer the numbers occurring in this article relate; but, probably, that of Réamur was made use of. —nt—

mined on the 1st of April the following were found:—

Lateral excavation.	Thermom. distant 7′ English.	Thermom. distant 1′ English.	External temper. in shade.	Hour of Observation.
	No. 526	No. 514		
382′	2° 35′	2° 4′	10° 0′	6¾ a.m.
	No. 525	No. 524		
350′	2° 65′	2° 65′	(?)	12½ pm
	No. 547	No. 546		
300′	3° 3′ (?)	2° 95′	3° 9′	2¾
	No. 531	No. 529		
250′	3° 4′	3° 4′	3° 9′	4
	No. 515	No. 534		
200′	4° 0′	4° 1′	4° 6′	4¾
	No. 511	No. 516		
150′	4° 65′	4° 65′	(?)	5¾
	No. 532	No. 509		
100′	5° 45′	5° 6′	5° 1′	7
	No. 510	No. 527		
50′	6° 55′	7° 6′	5° 1′	7¾
	No. 523	No. 518		
20′	9° 1′	9° 2′	5° 6′	7¾
	No. 517	No. 520		
15′	10° 5′	10° 3′	5° 8′	7¾*
	No. 533	No. 528		
7′	13° 7′	12° 3′	5° 9′	8

The temperature of a collateral excavation at the depth of 7′ from the surface of the earth, No. 508, examined at the same time with the last observation, was found to be = 13° 7′.

The temperature of the external air fell in the meantime to 6°.

The Imperial Academy will have remarked that in the distances of the uppermost excavations the instructions given were somewhat deviated from; the reasons were local impediments; but I feel convinced that the desired observations at the depth of 3′, 5′, and 7′, could be instituted with more accuracy in a collateral opening, in which the factors producing the temperature were of a more simple constitution. With this view I ordered, at the distance of several fathoms from the pit, in the same yard, an excavation to be made, descending to the depth of 7′, the corresponding data of which were stated above. To avoid in some measure the access of the external air, I repeated these experiments every second or third day.

Month, Day, and Hour of Observation.	Ther. in tallow, No. 508, at the depth of 7′ from the surface of the earth.	Temperature of the external air in the shade during observation.	
Mar. 10, 9 a.m.	16° 6′	11° 5′	
13, 11 a.m.	16° 3′	8° 2′	(At 6½h. a.m. the temperature was 12°.)
14, 5 p.m.	16° 3′	6° 3′	(In the morning the greatest cold was 13°.)
15, 10 a.m.	16° 2′	9° 0′	(At 5½h. a.m. it was likewise, 9°.)
16, 5 p.m.	16° 5′	3° 0′	(The greatest cold observed in the morning was 7° 2′, and might have been perhaps 8°.)
19, 6 p.m.	15° 9′	9° 9′	(After 6h. a.m., 12° 1′.)
21, 1½p.m.	15° 4′	10° 8′	(In the morning, 15° 2′.)
22, 11 a.m.	15° 1′	14° 1′	(In the morning, 19° 8′. This temperature changed but slowly; thus half an hour before observation it was 15° 2′.)
24,†10¾p.m.	14° 7′	14° 7′	
27, 10 a.m.	14° 35′	8° 3′	(In the latter nights the temperature was about 20°.)
29, 10 a.m.	14° 0′	8° 6′	(In the night the temperature was 14°.)
April 1, 8 p.m.	13° 7′	6° 0′	

In conclusion some remarks were added, chiefly respecting a slight incorrectness of the normal thermometer employed, and likewise on an inconsiderable difference between this latter instrument and the thermometers covered with tallow placed in the excavations and the statement that the entrance of the pit is 36′ above the lowest stand of the Lena, an adjoining river. The rest of the report is more of an official bearing, and, as these observations are to be continued, we will hasten to acquaint our readers with any important news resulting.

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* Immediately after.

† The external temperature was, March 23, in the morning, 19° 2′; at 5h. p.m., 9° 6′.

ON THE TENDENCY OF MATTER TO HARMONY.*

A LECTURE DELIVERED BEFORE THE SOCIETY OF NATURAL HISTORY, IN UTRECHT.

BY G. J. MULDER.

GENTLEMEN—

A STRIVING after harmony is beyond doubt no less a property of matter than of the human mind. It is not characterised by the slow development of a germ into an infinite whole, nor by the continued increase of its constitutional value; nor by the production of new fruits being as many component parts of one whole, which has produced them. No, gentlemen; of all this, matter is not capable, being of a lower and not of a higher order, it is destitute of that which belongs to beings originating from infinity. In vain, therefore, do we seek in it what is peculiar and can be peculiar to the human mind only; but that which the human mind and matter have in common is, that they both strive after harmony.

But, it may be asked, how can we attribute any striving to matter, it being wholly dependent on external causes? Is it not of a passive constitution, and does not the difference between material objects and immaterial beings precisely consist in this passive state of matter and the active state of the human mind.

It is, therefore, gentlemen, an error, to consider matter as *opposed* to the human mind. They are both active, even when they *seem* not to be so. The forces both of matter and of the human mind may be either dormant or vigilant; the present state of both is liable to be exchanged for a future one, and as both, having once risen, emanated from a common source, the hand of Almighty, they both, at the moment of their origin, acquired powers and faculties which, in matter as well as in the human mind, are indisputably characterised by a tendency to harmony—harmony of forces and faculties which are present in both matter and mind.

At present I wish to confine your attention to this point. Do not think that I shall conduct you through a metaphysical maze, or convince you of the faculty of matter for a tendency to harmony, by any deep philosophical arguments. Neither the one nor the other would be an easy task for *me*. That on which I intend to treat before you, is written in the book of Nature: and in unfolding this book, I will endeavor to show that the same as human minds originally simple and undeveloped sprung from the hand of the Almighty, and were endowed with a tendency to pass into another state, to be developed comformably to their nature and disposition, so matter was likewise presented with a similar tendency, and continues still to satisfy this propensity, to become in its turn that, of which agreeably to its nature and disposition it is capable.

If I should succeed as clearly in demonstrating to you that this tendency resides in matter as we know it does in the mind, then I should believe, having, perhaps, directed the attention of some of my hearers to the material world, I had done something; and thus, at the present hour, matter may perhaps coöperate in the promotion and development of the mind; and this, in my judgment, is the only object for which it exists.

To arrive at the point kept in view, I wish to direct attention to the creation of our planetary system; to consider what must have been the state of the matter constituting it now, previous to the creation of the said system; to elucidate the phenomena which *must* have taken place, when it was condensed into celestial globes; the phenomena which, after the celestial globes were created, were produced by the same forces with which matter was endowed, and are produced up to the present hour. In other terms, I will explain to you the creation of our planetary system by the aid of chemistry. I beg to be heard with indulgence.

I premise two things, which are established beyond doubt: matter once originated, and is endowed with powers which act according to certain laws. To adduce proofs on that score would be unnecessary labor. Matter cannot possibly be infinite; what cannot be infinite did once originate, and at the moment of its origin it received what it could not give to itself. This is incontrovertibly true. The question must, however, be answered, whether, by the condition of the matter of the earth, almost the only one which falls within the sphere of our conception, as it is at present, we are warranted in concluding the state of matter in which it was before the creation of the earth? And whether from that which we notice in our globe, we may draw a general conclusion relating to that system of celestial bodies of which our globe constitutes one?

These questions may be fully answered by the science in its present state.

That the whole earth was once in a state of ignition, nay, partially liquid and ignited, we learn by observations admitting of no doubt. What might have been the causes of that ignition? Two causes only can be conceived: the whole earth was exposed

* Translated from the German, for THE CHEMIST, by Arthur D. White, M.D.

to a heat produced by external influences, and was heated in an enormous fire, in the same manner as we are in the habit of heating objects in our fires; or it was reduced to that state of ignition in which it was once constituted, by forces which are now latent in its component matters. An external heating, an ignition of a cold earth in a fire is absurd. The ignition can only have originated from the matter of the earth, and if this be once proved, as may be done by hundreds of chemical experiments, instituted with bodies of which our earth is composed, then we find the clue, and may answer the question we have raised.

That the whole earth was in a state of ignition, we learn by the inspection of its surface, which is now cooled, and consists of merely calcined substances—of substances which were formerly soft, and thus crystallisable—of soft masses, which, yielding to the laws of centrifugal forces, flattened the globe at the poles. Finally, that the whole earth was ignited, we learn by that which lays beneath its surface, which, to the present time, is glowing under our feet.

The whole mass, therefore, of matters of which our earth consists, was once in a state of ignition, and gradually cooled down, and continues still to cool.

There can by no means have been more than one cause of this ignition, a cause which we still may draw within the precincts of our observation: it is the heat which is disengaged when substances, heterogeneous in a chemical point of view, meet together—it is what we call chemical heat. When heterogeneous substances meet together, be they simple bodies or compounds, heat which exists in matters by the forces, becomes free; free by the chemical combinations into which the bodies enter. And when we see in the creation a celestial body, which is composed of a great many chemically different substances, presenting all the characters which show that they once were in a high state of ignition, we then justly conclude that this ignition was produced when the heterogeneous substances of which it is composed met together, by their chemical combinations. If then it be proved that the earth, which was formerly in a state of ignition, came into that state because heterogeneous substances were combined into a whole; because the principal matters which never touched one another, came in contact, and thus disengaged heat, we learn from it that, previous to that epoch, the substances of which our earth is composed, as principal matters, were, at all events, only little united, or finely divided, without any mutual contact between them, in a state of vapor, and the moment they left this state of vapor, and the particles were chemically combined and united into a globe, they must have disengaged the heat, of which the earth still presents traces—nay, of which it exhibits the most convincing proofs.

Before I proceed I must show that in the elements which occur in our earth, forces really exist which, by simple contact, may produce the strongest heat, and become ignited; and if I have convinced you of this, I may, starting from the explanation just given, which is entirely founded on observation, ascend to the phenomena which have attended the creation of the earth.

We have an incorrect notion of chemical forces inherent in bodies, if after the forces were under a tendency towards satisfaction for thousands of years, we do not go beyond that which we now are used to see in the bodies around us. The stones of our buildings, the ground which we inhabit, the stones with which our towns are paved, are all in a state of repose, and exhibit themselves as endowed with no force, as destitute of any faculty. If, however, we disunite the constituent parts of these substances, they almost all manifest themselves as active, as endowed with forces capable to produce the most striking phenomena, be it that they were treated at a common temperature, or brought into contact with substances, constituted already in activity, and have become somewhat heated by that circumstance. To form, therefore, a proper notion of the forces inherent in bodies, we must examine the elements. A great many metals, for instance, cannot be brought into contact with chlorine without becoming ignited. If finely-divided antimony be thrown into chlorine gas, a phenomenon of fire is instantly produced. Phosphorus, put on bromine, produces a violent action; these two bodies combine, disengaging heat and light.

But the elements themselves, even when isolated, and exposed to mutual influence, often do not exhibit themselves such as they really are. Their activity is likewise embarrassed. Whatever exists in densely-accumulated masses is chemically inactive. A real chemical action requires that all the particles be free; and if we may reduce the bodies to such a state, we see that substances, apparently destitute of forces, manifest themselves as being endowed with the strongest forces.

Who would suppose strong forces in massive iron? And yet this metal, on being finely divided, to which state it may be reduced by decomposing oxide of iron by hydrogen, acts like platinum, presenting the character of it, becoming ignited, on coming in contact with oxygen or air. Uranium, nickel and cobalt, possess the same

property, and there is no doubt that all the elements, if we could but bring them back to the state in which they were previous to their condensation or combination, would present forces of a similar kind.

Phosphorus, dissolved in bisulphuret of carbon and thrown on paper as soon as the menstruum is evaporated becomes ignited. Lead, obtained by heating tartrate of lead, and, therefore, in a finely divided state, cannot be brought into contact with air without combustion.*

Alum, or sulphate of potassa, on being calcined with charcoal, and hence becoming sulphuret of potassium, possesses this property in a high degree.

If, therefore, to the isolated and finely-divided elements, of which the earth is composed, simply the opportunity is afforded to come into mutual contact, they combine, manifesting violent phenomena.

Elements, which are more or less combined already, did not lose that faculty of producing violent actions. On the contrary, if in their compounded state they are brought into contact with others, they exhibit again similar effects. Lime, or baryta, being put in contact with hydrochloric acid, becomes ignited. Sulphuric acid thrown on potassa produces vi lent action, &c.

A great many elements, therefore, in masses, and other bodies when finely divided, as well as their simple combinations, cannot be brought into contact without disengaging heat, and producing ignition. Of these bodies, and of no others, our earth is composed; and the same as the state of ignition which the earth once presented, is to be derived only from their union, so this state of ignition in its turn informs us, that the elements of which the earth is composed, were once in contact, but produced, at the moment of their union, the phenomena of fire, of which our earth retains so many proofs.

Hitherto, gentlemen, I have spoken entirely on observation. I now call for the assistance of your imagination to ascend towards that which cannot be founded on observation, that I lay no more stress on that than I think worth the endeavor to present to the mind objects which are not observable and need not to be particularly mentioned. That any notion or image formed may be correct, it must not be opposed to anything established, and but explain that which falls within the dominion of observation.

Let us imagine that our planetary system do not exist, but in its place an enormous mass of elements, which never were in contact, and reduced to a finely-divided state occupies the space which stretches beyond the orbit of Uranus, the planet most distant from the sun; a mass resembling a fog, or vapor, of a lenticular form, or presenting the appearance of a disc, wandering in space, containing the elements of the sun, the planets and their satellites, nothing, however, in a condensed state, but every particle in the condition in which it was created, this is, undoubtedly, simple, finely divided, neither physically nor chemically united with others, but endowed with forces which they possess up to the present time. Let us imagine, in other terms, a haze to exist in the sky, such a one as in some parts is, indeed, in our days, found in space;* a haze constituted in the middle of an absolute cold, composed of particles so small as mutually to attract themselves in the distances in which they exist; a haze containing all the matters which the sun, planets, and secondaries now contain.

This haze may have wandered millions of years in the state in which it was originally constituted, changing, however, its position in space, as everything made of matter changes its place, and turning round its axis, to maintain its single particles in equilibrio. In this mass of vaporiform matter, not only reside forces which may lead to condense, solidify, be put together and form masses, but forces, likewise, requisite to disengage heat, and to produce thousands of chemical phenomena.

A cause is merely wanted to excite these forces and bring them into activity, like iron, which must be approached by a magnet, or influenced by electricity to become magnetic; that is, not to receive *a new force*, but to *excite* that which was *latent* in the iron. The cause, then, which in the finely-divided mass of the haze is to excite activity, must be of a homogeneous nature with the forces which it is to excite, according to the general rule of nature, that equals may produce their equals. Dormant chemical forces must be excited in the masses of the vapor; the exciting cause must, therefore, be a chemical one, such which emanates from exchange of matter.

This cause was probably the approach of a celestial body, a fiery globe, such as are innumerably observed in the universe; of a mass which, hurried away near the haze, or, though at a distance, might have so far

* $C^4 H^4 O^5 + PbO = Pb + 4CO + 2H^2 O$. A little coal, however, remains united with the lead.

* Sir John Herschell counted 2,500 hazes of this kind, which are visible in the South of England.—*Phil. Trans*, 1833.

influenced it as to heat its particles to a certain extent, which, up to that time, were constituted in an absolutely cold medium to raise them to a temperature sufficient to displace the particles, to dispose the most active ones placed near each other mutually to combine.

From this moment the expanded hazy mass is disturbed from the state of repose—disturbed for centuries, no, for a hundred thousands of centuries—for ever, perhaps, as long as the matter exists. Homogeneous forces, which, like those that first emanated from the most active particles, were dormant in all others, excite similar forces in the whole mass. The movement of the particles changes the respective distances; those, then, approach and attract one another. What is situated in the centre of the whole condenses first into small masses, which by the chemical powers of the elements are brought into a state of ignition; these small masses condense into one whole, which in its turn may excite into activity the remaining hazy mass. All, placed near around or at a distance, hurries together forming larger or smaller masses, invariably attended by violent phenomena of chemical action. The turning motion of the haze is communicated to all condensed masses and the nucleus of the whole formed of the parts of haze formerly constituted in its centre, becomes the axis round which the rest is thrown.

The hypothesis of the creation of our planetary system which I now develop before you, has been already promulgated, though somewhat modified, by philosophers and astronomers. The first traces of it are found in the atomistical system of the Grecians, but Kant has lately based it on philosophical grounds. Herschel inferred it from the progressive development of stars from hazy spots which in our days exist, and which by the authority of Laplace obtained credit. We endeavored to develop it by chemical reasoning. To be clear, however, I must add one word to the chemical theory. In the theory of Laplace no cause of the condensation is adduced, which may be proved by arguments; and it is said that condensation is gradually going on. Laplace imagines the haze to be ignited, as an atmosphere of a sun, and from that by the centrifugal forces first rings to be formed, and these falling into pieces of fluid liquid, ignited masses were condensed into globes.

(*To be continued in our next.*)

AN ACCOUNT OF SOME EXPERIMENTS ON THE DEVELOPMENT OF FRICTIONAL ELECTRICITY.

BY REUBEN PHILLIPS.

Some time ago I made a number of rather desultory experiments to endeavor to ascertain the use of the amalgam to the electric machine; some account of which may, perhaps, interest a few.

The apparatus I first used was nothing more than a piece of glass tube about sixteen inches long, and $\frac{1}{10}$ of an inch through, which was excited by being rubbed with silk, on which was spread some of the substance whose powers of excitation were to be tested. The strength of the action was measured by the loudness and frequency of the snapping noise which accompanied the friction.

I found that many substances, not metallic, were capable of producing a strong action, such as lime and tallow, lime, acid (as the oxalic) and tallow; ductile sulphur also gave a very strong action.

The notion which forced itself on my mind from these, and a great many similar experiments was, that frictional electricity is due, in these cases, to the chemical combination of atoms; for instance, boracic acid and lime, one class of atoms, in this case boracic acid, attaching themselves, perhaps, by the attraction of cohesion to the glass; and the other body, lime, being more disposed to stick to the silk. If now chemical combination ensue, the glass must take the plus electricity liberated by the acid, and the silk the minus from the lime; for, from the phenomena of voltaïc electricity, it seems that, combining atoms cannot discharge the electricities they produce while combining through themselves, but that it must pass a longer path through adjacent bodies.

If this were a true view of the case, then, obviously, two surfaces, capable of union, being brought together, should develop electricity more promptly than the indirect method previously used.

The metals seemed to be the best class of bodies to operate on. I therefore took a piece of bolt copper seven and a half inches long, and half an inch through, having its surface smoothed, and its ends rounded; it had about an inch of one end cemented into a a glass tube. When the exposed portion of the copper was rubbed longitudinally with a piece of silk, in the same manner as it is usual to excite a glass tube, it was found almost, or quite, impossible to detect evidences of electricity; of course, removing the rubber prior to approaching the rod to

the electrometer. But when a piece of silk was extended between two supports, and the copper drawn over it, the direction of the motion being parallel to the axis of the rod, it was found an easy matter to develop signs of electricity, the copper taking a resinous charge.

Conceiving that some atomic change, either chemical or mechanical, as disintegration, must accompany the production of frictional electricity, I made many experiments in the following manner:—One end of a piece of wide ribbon was uniformly greased by rubbing a tallow candle on it, after which a quanity of powdered sulphur, was spread on it, and rubbed smooth. Zinc, lead, or copper, and, without doubt, many other metals rubbed on this, easily produce much electricity. After some time I arrived at the following method, which surpasses in electric energy anything I have hitherto seen of the sort. A piece of copper, in this case, six inches long, is fastened to a wooden cylinder, ten and a half inches long; the copper rod has a smooth surface, and is well rounded at the ends; the surface which touches the ribbon is part of a cylinder $\frac{1}{20}$ of an inch through, which, however, is, I think, much larger than necessary, and which is fixed parallel to the axis of the wooden cylinder; the wooden cylinder is covered with tin-foil, smoothly laid on; the diameter of this cylinder is $1\frac{8}{10}$ inches. The ribbon being prepared as formerly stated, let this rubber be moved along its sulphured surface, taking care that the copper precede the tin, and that both the tin and copper rest on the ribbon. A snapping noise will be produced every time this is done after some sulphuret has been formed.

I am very much inclined to believe that cheap electrical machines, of great power, might be constructed by causing a band of silk to pass over two rollers; one, which might be hollow, and from twelve to eighteen inches through, could be moved by a winch; the other cylinder might be much smaller, and might be eighteen inches or two feet distant from the larger; two rubbers could easily be placed between them, on the outside of the silk. The silk would take the plus electricity, leaving the rubber minus.

As to the extent of the rubbing surface, which it is advisable to give to electric machines. When two surfaces are being electrified by friction, I think it will readily be conceded that the quantity of electricity on the two surfaces in contact, must go on being augmented until the electrical intensity is so high, that the quantity produced is equal to the quantity which forces its way from one surface to the other; therefore, any extent of rubbing surface greater than is sufficient to charge the rubbing surfaces as high as may be, is a waste of power.

The intensity of charge, which surfaces of different materials take, varies much, as is seen in the case of copper and tin; and I will here mention, that I have in my possession a bar of copper, which produces effects, comparable only to the compound rubber of tin and copper. I have made many experiments to discover the cause of this difference, which seems to exist only in the exterior portions of the copper, for I filed away some of the surface, which, although perfectly polished, had lost the qualities the rest of the surface of the copper possessed. I am much inclined to think that the effect is due to a small addition of tin; at all events, a bronze which I made possessed very similar properties; however, the proportion of tin in my bronze is very much greater than it can be in the copper rod.

The above piece of copper is that which forms part of the rubber of tin and copper above described, but I have convinced myself that the effects produced by this compound rubber are not peculiar, and I think that almost any specimen of commercial copper may be used. I hope shortly to submit a portion of this anomalous copper to analysis.

It is worthy of observation, that both copper and lead, when rubbed against sulphur, are capable of forming sulphurets at the common temperatures of the atmosphere.

I now wish to remark, that while the ribbon is in contact with the metallic rubber, it evinces no signs of electricity. Let one end of the compound metallic rubber be secured in a vice, then, on wrapping the ribbon about it, and drawing it to and fro, sparks will rapidly be produced; if, while the silk is held tight against the rubber, a light body, as a piece of sewing thread, be approached to that part of the ribbon in contact with the metal, no attraction will be perceived; but let the ribbon be lifted a very little from the rubber, and vivid signs of electricity manifest themselves, which again disappear, as soon as the metal and ribbon are brought into contact.

I will conclude with a few words on connecting the rubber of electrical machines with the ground; I think the explanation is in unison with the well-known properties of the electric fluids.

When the cylinder of an electric machine in good working order is turned, the rubber not being in communication with the earth, the electric intensity rises, until the quantity of electricity produced equals the waste; the plus electricity produced being dispersed over

the cylinder and prime conductor, and its equivalent of minus existing on the rubber; that is, supposing that both the plus and minus surfaces radiate equal quantities. Suppose the prime conductor now communicates with the ground, the whole (nearly) of the plus electricity escapes, but the quantity of minus electricity remains; then, as the cylinder revolves, successive portions of previously unelectrified glass come under the rubber, which contains a minus charge, which induces plus electricity on the surface of the glass, so intense, that the electricity developed by the friction can only supply the waste of the two surfaces; as soon as the glass has passed the rubber (under which term I include the flap) it regains its former condition, that is, supposing the rubber has been unable to give some of its electricity to the air, &c.

Let the rubber be connected with the earth, the glass being also free from electrical excitement; then, on turning the cylinder, the plus electricity produced becomes accumulated on the glass, and the minus on the rubber, until the rubber has acquired its maximum charge, which charge is retained in its position by induction, produced by passing portions of the revolving glass, which always (in this case), under a given part of the rubber, contains the same quantity of electricity. The minus electricity produced by the friction, after the constant quantity required by the rubber has been supplied, passing off to the earth.

This is the reverse of the former case, in which the permanent charge in the rubber, induced temporary electricity on the glass: in this case the glass induces electricity on the rubber.

Postscript.—I ought to have observed, that the use of the copper in the compound rubber is merely to deposit a certain something on the silk, most probably either metallic copper, or a sulphuret; any one may easily convince himself of this by rubbing a piece of copper on the prepared silk until the silk begins to look brown, and then stroking it with a wooden cylinder covered with tin foil. As to the proportions of tin and copper for forming a bronze rubber, I should be disposed to try six parts of copper to one of tin; but my experiments on the subject have been, as yet, very limited.

Topsham, near Exeter,
13th Jan., 1845.

TEMPERATURE IN A PIT NEAR NEUFFEN.*

At the south-west of the town of Neuffen (48° 33′ N., and 27° 2′ E., from Ferro), in Wurtemberg, at the north-western foot of the Alps, a pit was excavated, being 1,186 Wurtemberg feet deep,† with the entrance 1,295 Parisian feet above the level of the sea, or 1,003 Parisian feet higher than the ridge of the Swabian Alp. Count F. de Mandelsloh, of Ulm, observed the temperature of this pit by a geothermometer of Magnus, and obtained the following results:—

Time of observation, 1839.	Depth in Wurtb. feet.	Temperature.	
		Of air.	Of pit.
Feb. 27, 8½ h. V	100	1° 8′ C	10° 8′ C
— 27, 9¾ h. V	200	1° 0′	13° 7′
— 26, 1 h. N	300	2° 5′	16° 5′
Apr. 10, 4 h. N	400	9° 0′	18° 4′
Feb. 27, 12½ h. N	500	4° 0′	20° 4′
— 26, 3 h. N	600	2° 5′	23° 5′
— 27, 2¾ h. N	700	4° 0′	25° 4′
— 27, 4¾ h. N	800	3° 0′	27° 8′
Apr. 10, 6½ h.	900	8° 0′	31° 2′
— 11, 6¼ h. V	1000	4° 0′	33° 5′
— 11, 3 h. N	1080	8° 0′	36° 3′
— 11, 11¾ h. N	1180	6° 4′	38° 7′

From these results the author infers that 100 Wurtemberg feet increase by + 3° 28C., consequently, 1°C., of increase of temperature corresponds to 30·49 Parisian feet. [This does not manifestly appear, because the mean temperature of the soil of Neuffen was not subtracted, but the temperatures observed at some depth are indeed uncommonly high.—P.] The temperature of a rivulet, which passes near the pit, at the time of the first, third, and sixth observations, was respectively +4·0, +7°O, +5°C.

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* Leonhard and Bronn's *Neues Jahrbuch*, 1844, p. 440; and Poggendorff's *Annalen*, 9, 1844.

† 1,186 Wurtemberg feet = 1,046 Parisian feet.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ADULTERATION OF TEA.*

BY H. LEPAGE.

THE communication made by Guibourt in the sittings of the Pharmaceutical Society (of Paris) the 5th June last, the statements of Thompson, of London, on the same occasion, and the notice inserted in the August number of the *Journal de Chimie Médicale*, relating to certain agents employed in China to color tea, induced me to publish the following suggestions.

If the Chinese employ a mixture of prussian blue and sulphate of lime to color tea, they evidently do not the same with all tea sent to Europe; for according to what is stated in the excellent work of Guibourt, in his *Histoir Abregé des Drogues Simples*, and which I had many opportunities of confirming, if green tea, *hyson*, and *gunpowder*, and the *bouy* tea, or *black tea* of commerce, are macerated in distilled water, and filtered through paper free from any salt of lime (a fact which it is highly important to ascertain previous to operating), the liquid which passes through is not rendered turbid, even after a long time, either by oxalic acid, or by oxalate of ammonia; whereas, if only 0·01 of sulphate of lime be added to 25 grammes of normal tea, and the filtered liquor, obtained in the above manner, allowed to remain in contact for some time only with the mentioned reagent, yields a manifest turbidity, which, on the addition of a little nitric acid, disappears.

With reference to Prussian blue, I think that it is possible to discover the presence of this body in tea, by a simple and expeditious means; we avail ourselves of the discovery of Mohr, who informs us that this cyanic compound is dissolved in a solution of oxalic acid, to effect which it is sufficient to wash the matter under examination with distilled water, thus to detach the coloring powder, to throw the whole on a sieve, but moderately straight, to drain, and collect the water obtained by washing in a cylindrical vessel, and after having allowed it to settle, and the liquid to become clear, to decant the same, then to add to the deposit two or three crystals of oxalic acid, and a sufficient quantity of pure distilled water, stirring a little at the end of the operation to promote the solution of the acid and its solvent action on the Prussian blue, &c. On having added in fractioned doses to 25 grammes of tea of good quality, 0·02 of a mixture of three parts of sulphate of lime, and one part of finely-powdered Prussian blue, and having treated this in the manner stated above, I invariably obtained, on adding to the deposit some particles of oxalic acid, and a sufficient quantity of distilled water to complete the volume of about 100 grammes, a bluish green liquid; which the more manifestly presented this latter tint the more the yellow coloring matter of the tea was contained in the precipitate of the water obtained by washing. This experiment cannot certainly be considered as a conclusive one; it is before all necessary to repeat it with teas which have been impregnated with Prussian blue by persons who are in the habit of practising this fraud. I communicate the present article with the view of inviting chemists, who are under favorable circumstances, to repeat these experiments, and decide upon their validity.

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* *Journal de Chimie et Médicale*, Nov. 6, 1844.

ON THE INFLUENCE OF SALT UPON VEGETATION.*

BY M. HENRI BRACONNOT.

MANY agriculturists are of opinion that common salt, when thrown on the soil, promotes vegetation and augments the crop. An adherent to this opinion having assured me that the action is most remarkable with reference to colza, I endeavored to ascertain the correctness of the assertion. I first caused seeds to germinate on flannel, with water containing various proportions of salt, in order to ascertain the proportions in which I could employ it with some hope of success, for it is known that its deleterious action on vegetation was admitted by the ancients, because the Bible says that Abimelek sowed salt in the ruins of Sichem, that that soil might no longer be productive. Being convinced that experiments on a small scale often furnish us with more conclusive results than those undertaken on a large scale, on account of the

* *Annales de Chimie et de Physique*, 1845.

influence of various circumstances then taking place difficult to be appreciated, I confined myself to the following experiment:—

On the 13th of September I took three common flower-pots, marked with 1, 2, and 3; into each of them I put 7 hectogrammes (about 1½ English) of earth, taken from a field previously well dried. I have sown in each of the pots 12 seeds of colza, and five seeds of pois de senteur (Lathyrus odoratus), which I had at hand.

On No. 1 I poured two decilitres (nearly ⅔ pound English) of rainwater, in which 2 grammes of salt (30 grains English) were dissolved. On the second I poured the same quantity of water, containing one gramme of salt; and on the third a similar quantity of water without salt in it. These three pots were in the same room.

The 20th of September I compared these three plantations. No. 1 exhibited marks of germination in two seeds of colza; the two little plants arrived scarcely at the height of 2 millimetres (1/13 of an inch English). No. 2 presented eight plants of colza, 10 to 20 millimetres high (⅜ to ¾ inches English). In No. 3, 12 seeds of colza had germinated and produced plants to the height of 30 millimetres (1 inch English). All the seeds of lathyrus produced stems 20 millimetres high (¾ of an inch English); whereas the same seeds of 1 and 2 offered yet no marks of germination.

The 9th of October my plants required to be watered again. I allowed each of the three pots to absorb from below upwards 1 decilitre (⅓ pound), without any admixture of salt, imitating thereby the manner in which horticulturists treat delicate plants. The following is the state in which I found the plants at that epoch:—No. 1. Of the 12 seeds of colza sown, six refused to germinate, and the others presented plants 4 centimetres (1½ inch) high, which were vigorous, notwithstanding their inconsiderable development; one seed of lathyrus, which did germinate, produced only a slender plant, no more than 5 centimetres (1¾ inch) high. No. 2. Three seeds of colza had not yet germinated; the other nine produced well constituted plants, from 7 to 8 centimetres (2¾ to 2¾ inches) high. Three seeds of lathyrus did germinate; their plants were 12 centimeters high. No. 3. The 12 plants of colza were from 12 to 13 centimetres high (4 to 4½ inch), and the 5 lathyrus 22 to 23 centimetres (7⅓ to 7⅔ inch).

The 19th October Nos. 1, 2, and 3 were again watered with rain water, from below upwards. Finally, on the 25th, on comparing the plants one with another, I found that in No. 1, half of the seeds of colza presented vigorous, but short and squat plants.* The one seed of lathyrus which resisted the salt, produced only a feeble and little elevated plant.

The plants of No. 2 continued to grow vigorously, but they were evidently surpassed in their development by No. 3. I remarked that the earth contained in the latter pot, lost its humidity easier than Nos. 1 and 2, which I have chiefly attributed to the more abundant absorption and transpiration of plants which have vegetated without salt being employed.

Some agriculturists state, that if the salt employed be mixed with lime, the results of the vegetation are more strikingly favorable than if employed unmixed; this they account for by the carbonate of soda produced, which highly promotes fertility. I have sown seeds of colza and lathyrus, moistening them with water, containing in solution a small quantity of carbonate of soda or potassa, which scarcely rendered blue reddened litmus-paper. The plants obtained, however, seemed to be neither higher nor more vigorous than those which I had moistened with pure water. It might be objected that these experiments on the small scale were neither sufficiently numerous, nor carried to such an extent as to serve as a proof against the great use made of salt in the agriculture of several countries; but it will be admitted that the facts which relate to this point, are in general involved in so much obscurity, that they frequently are not better interpreted by experimenters than by mere empirics. But it appears to be demonstrated, that if the quantity of salt present exceeds a certain amount, it either entirely prevents germination or retards it most sensibly, giving birth to certain animalculæ, which may be compared with those, the growth of which is put a stop to by the use of alcoholic liquids. The facts stated just now are, for the rest, in accordance with those observed in certain land plants when they grow in saline marshes, their exterior is then so different from the usual one, that botanists sometimes consider them as a particular species. To quote only one example, the *atriplex patula* at a distance from the saline soil of Dieuze and Vic de Marsal is considerably developed; but in the said neighborhoods it is so short and squat, that it is taken for a distinct species, and called *atriplex salina*. I ex-

* These plants having been carefully washed, were most manifestly of a saline taste; on incineration, a remarkable quantity of salt was indeed found in their juice, whereas a similar quantity of the same plant, developed without salt, furnished only a slight saline residue.

amined the influence of salt water on branches when cut off. I took two champagne glasses, placed in the first eight centilitres of rain-water, and in the second a similar quantity of water, containing one per cent. of common salt. In each of these vessels I put an equal weight of branches, in a state of blossom, of the following plants:—Reine marguerite, Senecon élégant, Corcopsis élégant, Escholtia, and Phlox. After eight days, the greatest part of these plants faded; but this change took place to a greater extent in the salt water. But it seemed to me still more remarkable that the plants showed a kind of repugnancy against the salt water, which they refused to absorb; because, having measured the turbid and putrid water which remained, I found 6 centilitres left, notwithstanding the spontaneous evaporation; whereas, in the other vessel, almost the whole of the rain water was absorbed, except one ½ centilitre, which was no more in contact with most of the branches, and completely preserved its limpidity. What are we to conclude from these facts so easily ascertained, and which seem to be opposed to all others known? Is it to be admitted that, by a kind of choice, the branches cut off absorb the pure water in preference to that rendered feebly saline? We have seen that grains which did germinate in the latter liquid produced plants, though shorter than the usual ones, yet absorbed a notable quantity of salt. And, in addition to this, it is proved that branches cut off absorb indiscriminately poisons and nourishing matters offered to them. By comparing the results obtained one with another, I soon discovered the cause of this apparent anomaly. I have said that the little residue of the rain water which was not absorbed by the plants which had vegetated, preserved its limpidity; whereas similar plants, placed in water rendered feebly saline, did not sensibly absorb water, rendering it turbid and evidently putrid. On examining the extremity of the branches which had been immersed, I likewise observed that the bark was there reduced to a state of putrefaction; and on transversely cutting through the woody tissue it seemed to be obstructed, which circumstance must have impeded absorption; whereas nothing of the kind was found in the same plants which I allowed to vegetate in rain water. I must remind you that this property of salt to produce putrefaction, when employed in small quantities, was known to the ancient chemists, because Becher speaks of it in his works. Pringle made experiments with it nearly a century back, proving that salt in small quantities accelerates the process of putrefaction; hence this physician concluded that it assists digestion by its septic properties. To this property I think we are to attribute that soft water, on being mixed with salt water, becomes putrid; and the agriculturist Jaubert, who undoubtedly was not aware of the experiments of Pringle, has not without reason prescribed the employment of a small quantity of salt in the preparation of artificial manure. And it will be conceived how salt could act upon certain kinds of soil. I think it might likewise be advantageously employed to accelerate the steeping of hemp.

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OF AN IMPROVED COMBINATION OR ALLOY OF METALS, APPLICABLE TO VARIOUS PURPOSES FOR WHICH BRASS AND COPPER ARE USUALLY EMPLOYED, IN THE CONSTRUCTION OF MACHINERY.*

THIS invention of an improved combination or alloy of metals, is intended to be used in the construction of machinery in general, in those places where brass and copper are usually employed.

The improved alloy, it is stated, may be beneficially used as a substitute for the ordinary metals, in consequence of its not being liable to heat, or subject to other destructive results, caused by friction and ordinary wear and tear; also, by greatly decreasing the consumption of oil or grease, and being of increased durability, and much lighter weight in the same bulk of metal. All these advantages will be sufficiently evident to the practical engineer and mechanic, as well as the great variety of purposes for which this improved combination or alloy of metals may be employed in the construction of machinery, such as steps, bearings, pedestals, journals, bushes, axle-boxes, connecting-rod ends, cocks, taps, pump-barrels, pump-rams, plungers for buckets, &c., and also as a substitute for the more elementary parts of machinery (formerly made of brass or copper), such as rollers for calico and other printers, bowls, &c.

The manner of carrying this invention into practical effect, is to be according to the following formula:—Firstly, take 32 parts of copper, 15 parts of block tin, and 1 part of sheet brass, and mix or combine these in the following manner:—The copper being fused or melted, in a crucible, or other

* Patent granted to James Fenton, of Manchester, engineer.—Newton's *Journal.*

suitable vessel or furnace, the sheet brass is added thereto, and afterwards the block tin is thrown in; the alloy is then poured off in ingots, and a metal is produced, which the patentee terms "hardening metal." Under this head he claims the novel and peculiar use of these metals, to form "hardening metal;" but the quantities may be varied, to give the alloy any required degree of hardness, or various other metals may be added, in small quantities, to effect the same purpose; he likewise claims the use of these, in connection with copper and block tin: the above constitutes the first part of the process employed by the patentee in the manufacture of his *ultimate* alloy or alloys.

Secondly, take 2 parts of the hardening metal, previously described, 19 parts of zinc or spelter (or so many parts of calamine as shall be equal to the quantity of zinc or spelter), and three parts of block tin, and mix and combine these in the following manner:—First, fuse or melt the zinc, spelter, or calamine, in a crucible, or other suitable vessel or furnace, which must be sufficiently large to contain, along with the zinc or spelter, the hardening metal previously described, and the block tin last specified. The hardening metal should be fused or melted in a separate crucible, or other suitable vessel or furnace, and then mixed or combined with the zinc, spelter, or calamine: the alloy must be well stirred, with a suitable implement, in order to render the combination of these two metals or semi-metals as complete as possible. The block tin is then added, to give the ultimate alloy or alloys the requsite degree of ductility or toughness. The whole must be again well stirred with a suitable implement, in order to render the combination of this, the ultimate alloy or alloys, as complete as possible. It may then be cast, or employed in the usual manner in the various forms required for the construction of machinery.

While the zinc or spelter is being melted, the surface of it should be well covered with a coating of powdered charcoal, in order to prevent the volatilization of the semi-metal.

Under this head the patentee claims as follows:—"The use of these metals and semi-metals, above described, to form my ultimate alloy or alloys; but the proportions may be varied, to suit particular cases, and a variety of other metals may be added, in small quantities, the use of which I also claim, though not absolutely requisite to form my ultimate alloy or alloys. I further claim the use of the semi-metal zinc, spelter, or calamine, as the basis of my ultimate alloy; and although I have found the manner of combination above described the most effective in preparing the alloy or alloys which I substitute for brass and copper, in the construction of machinery, I claim the use of the said alloy or alloys, although combined in any other manner or proportions whatever; such combination or alloy being made, either in the exact proportions herein set forth, or in any other, within such limits as are substantially the same, and will produce a like result, as a substitute for brass and copper, to be used in the construction of machinery."

Specification drawn by Messrs. Newton and Son, 66, Chancery-lane.

A SUBSTITUTE FOR WOOD ENGRAVING.*

BY RICHARD LEWIS BEAN, ESQ. M.R.C.S. LONDON.

HAVING been engaged lately in some photogenic experiments, I tried the following method of engraving, which although not of use in photography, appears to be an excellent substitute for wood engraving, as it takes so little time, (two or three hours,) and only costs a few pence; those who can draw a little may avail themselves of it, and, I have no doubt, surgeons and others publishing would find it of great service, as the trouble and expense are so light.

I take a piece of black glass, or glass with a black ground behind; melt common wax, so that there may be a coat about the thickness of a sixpence; when this is cool, rub it over with a preparation of salad oil and white lead, mixed into an ointment; this is to give a white ground for etching upon. Trace the drawing so as to leave a red outline upon the ground; proceed to etch with needles (taking care to make the grooves perpendicularly through the wax); when this is done, lay some water gently over the wax, and if there are any minute globules of air sticking to it, they are to be removed by gently heating with a lamp, great care being taken not to melt the wax; sprinkle some of the finest sulphate of lime, (plaster of Paris,) which is best got at the casting shops; let it combine with water, and set. When this is done, it should be made of a convenient thickness, by adding more to the back of it; now dry, and deepen the broad lights in the manner as on a wood engraver's block; boil in glue, which will sink into the substance of the plaster, and enable it to bear pressure in printing. After this, proofs must be taken, and gradual improvements made.

Union-terrace, Camden Town,
Nov. 27, 1844.

* *Lancet*, Dec. 14th 1844.

SPONTANEOUS COMBUSTION OF GUANO.

The steam-packet Waterwitch, arriving at Hull, from London, lately, brought into port the master and crew of the bark Ann, Story, of Sunderland, who had been picked up by the packet in an open boat, to which they had taken on the destruction of their vessel, near Harborough-sand, on the preceding evening. It appears by the statement of the ship-wrecked men, that the Ann, a new bark, on the first voyage, was returning from Ichaboe, with a cargo of guano, and unfortunately struck on the sand, and, while beating over, shipped a quantity of salt water, which, penetrating the cargo, caused almost instantaneous combustion. A volume of smoke rising through the fore hatchway warned the crew of this new danger, and induced their taking immediately to the boat, without saving anything but themselves; and scarcely had they done so, when a tremendous explosion of the gas engendered by the partially fired guano, blew the stern out of the vessel, which then filled, and sunk in deep water. It is said that three vessels, belonging to the same owners, started together on this voyage, one of which was lost on the passage out, and the third had not arrived at Ichaboe when the unfortunate Ann left the island.

ON AGRICULTURAL CHEMISTRY.*

BY DR. WARWICK.

The learned doctor commenced a highly-interesting and valuable dissertation on guano, the mode of detecting its adulteration, and of applying it to the best advantage, and proceeded to explain the nature and properties of chemical manures, saltpetre, nitrate, and sulphate of ammonia, or gas-water, gypsum, and those important and essential elements in the production of wheat, phosphate of lime, and silicate of potash. He next adverted to paring and burning, as a means of improving the soil. Fallows then came under consideration, which he described as portions of land on which no seed was sown for a whole year, that it might have the benefit of exposure to the air. He recommended the cultivation of spurry, often described as a weed, as an excellent pasture, and as improving the land in an extraordinary degree. Draining was the next topic touched upon. As a certain quantity of moisture is essential to vegetation, so an excess of it is highly injurious. He urged the well draining of cultivated land. Nothing he said is so bad as stagnant water. Although water is the food of plants, it is not so, simply as water, since distilled water has been proved not to be nutritious, but as a vehicle for the carbonic acid from the atmosphere, and, unless the water be removed when its nutritious properties are exhausted, it starves the plant in a double sense. The learned lecturer suggested several modes for defraying expenses of draining, particularly recommending that a small portion of a farm should be done first, and that the value of the extra produce derived from the operation (calculated by Mr. Pusey at five bushels per acre) should be laid out in draining another portion, and so on, until the whole were done. If this plan were adopted, the sons of the present generation would live to see England the exporter of corn, instead of being harassed with disputes about the sliding-scale. Dr. Warwick next adverted to geology, of which science he gave a popular outline, illustrated by drawings of the various strata found in England, and put his audience in possession of a very simple mode of analysing soils, and detecting the qualities in which they are deficient. The economy of seed-wheat came next under notice—the lecturer warmly recommending the plan suggested, as tending not only to a saving of seed, but to the improvement of the plant. The comparative nutriment contained in different vegetables, and the ease with which various sorts of food were digested, was the next subject of consideration. French beans were placed at the head of the list of vegetables as containing more nutriment than any other—wheat ranked third best. As to animal substances, mutton contained 29 per cent. of nutriment; beef, 21; chicken, 25; pork, 24; cod and sole, 21; haddock, 18, &c. As to digestion, boiled rice occupied an hour; sago, 1 hour and 45 minutes; tapioca and barley, 2 hours; stale bread, 2 hours; new bread, 3 hours; boiled cabbage, 4 hours; oysters, 2·5 hours; salmon, 4 hours; venison chops, 1·5; mutton, 3; beef, 3; roast pork, 5·25; raw eggs, 2; soft-boiled eggs, 8; hard ditto, 3·5. The lecturer mentioned a curious fact, the result of experiment, respecting the growth of potatoes, namely, that if the crown of the potatoe, usually cut off and thrown away by cooks, were planted, it would produce far more than if a whole potatoe were planted—by the adoption of which suggestion not only would the crop be improved, but the whole of the seed now used would be saved. The doctor said he had tried the experiment with the crowns of potatoes,

* *Journal of Royal Agricultural Society.*

cut off in the course of the daily preparations for cooking in his own kitchen, and thrown together in a heap, till it was time to plant them. Another thing he mentioned was, that the shoots which the gardeners often rubbed off when they planted seed-potatoes were, in truth, the most prolific part, and he instanced an experiment where some shoots only were planted in competition with whole potatoes, the former coming up earlier and giving a much better crop. Dr. Warwick, after giving a history of the celebrated Warwick green dye, of which he was the discoverer, and stating that, in consequence of that and other aids he had been enabled to give to manufacturing science, calico-printers, who at first sneered at the idea of his teaching them anything, became very glad to see him, and took care to have all their sons thoroughly educated in chemistry—strongly urged his hearers to make their sons chemical agriculturists, as there was no knowing what might be the result.

ANALYSIS OF BOLIVIAN GUANO.

		Containing of Ammonia.
Water	8·50	
Sulphate of potash, with a little sulphate of soda	3·00	
Muriate of ammonia	3·00	0·95
Phosphate of ammonia	14·32	4·62
Carbonate of ammonia	1·00	0·34
Sulphate of ammonia	2·00	0·50
Oxalate of ammonia	3·23	0·89
Soluble organic matter and urea	8·95	
Silica	1·25	
Organic matter	9·52	
Urate of ammonia	14.73	1·23
Oxalate of lime	1·00	
Subphosphate of lime	22·00	
Phosphate of magnesia and ammonia	4·50	0·32
	100·	9·80

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

ON THE DIFFERENT EFFECTS PRODUCED BY PHOSPHORIC ACID WHEN PURE AND WHEN IMPURE.*

BY DRS. WEIGEL AND KRUG, OF CASSEL.

DISTINGUISHED physicians have asserted that under the influence of the continued administration of medicinal phosphoric acid in ordinary doses, a modification of the rancous membrane of the stomach is manifested, which modification, after death, shows itself in the form of red or reddish-brown stains. These practitioners, owing to this observation, have deprecated the continued use of phosphoric acid, as being a dangerous preparation.

But all physicians are not convinced of the correctness of this fact, which doubtless happens but seldom, because, according to their experience, phosphoric acid has been employed for a long time with remarkable success.

The contrariety of these contradictory opinions led Drs. Weigel and Krug to believe that the difference of action was owing to a slight difference in this chemical constitution of the medicine; indeed, pure phosphoric acid, administered in medicinal doses, is not capable of producing such an effect. But every well-informed pharmacien knows that owing to want of care in preparation, phosphoric acid may very easily contain phosphorous acid, and even, in certain circumstances, arsenious acid.

These gentlemen, guided by this idea, determined to undertake a series of comparative experiments on the effects of pure phosphoric acid, and on those of this acid contaminated with either phosphorous or arsenious acid.

The phosphoric acid was prepared by treating pure phosphorus by nitric acid, and by analysis it was found to be chemically pure.

The phosphoric acid contaminated with phosphorous acid was prepared by treating pure phosphorus with an insufficient quantity of nitric acid; it contained about 10 per cent. of phosphorous acid.

The phosphoric acid contaminated by arsenious acid, was prepared by treating arsenical phosphorus by a sufficient quantity of pure nitric acid; it contained about ¼ per cent. of arsenious acid.

The experiments were made on rabbits of the same age, and subjected to the same diet.

The authors could have wished to ad-

* Casper's *Wochenschrift für die Gesammte Heilkunde.*

minister to these animals the three kinds of phosphoric acid in the state of solution, and for a very long time, as is ordinarily done with human beings; but the difficulties of making them swallow it rendered a change in the mode of proceeding necessary. Thus these acids were given in the state of concentration, and by drops only, applied to the base of the tongue. To facilitate this operation, the jaws were held open by means of ligatures applied behind the teeth.

The results of these experiments demonstrated that pure phosphoric acid, in the ordinary doses, cannot produce injurious effects on the sides of the stomach, because, even in the concentrated state, it gave rise to no caustic action, notwithstanding the weakness of the animals experimented on.

It also appears that the same quantity of phosporic acid, which in the pure state was administered without inconvenience, occasions, when contaminated by phosphoric acid, very serious symptoms, for instance, gangrenous phlegmasia of the mucous membrane of the stomach. The cause of this peculiar effect may perhaps be found in the development of a highly poisonous compound—phosphuretted hydrogen; for phosphoric acid, which contains phosphorous acid, disengages, on being heated in the air, a certain quantity of this gas. Perhaps, also, the symptoms may be owing to a greater oxidation of the arsenious acid during its stay in the stomach.

Phosphoric acid contaminated with arsenious acid is, even in small doses, a violent poison.

Drs. Weigel and Krug are, therefore, convinced that if, after the internal employment of phosphoric acid, there be observed on *post mortem* examination red or reddish-brown stains on the tunica villosa of the stomach, this peculiarity must be attributed solely to the employment of phosphoric acid contaminated with phosphorous acid, which may easily be the case when sufficient nitric acid is not used in its preparation.

ON CARRAGEEN MOSS.*

BY M. E. MOUCHON.

M. E. MOUCHON, pharmacien at Lyons, has published in a recent number of the *Journal de Chimie Médicale*, several formulæ intended to facilitate the administration of carrageen, and consequently to extend its therapeutical employment. Carrageen is a substance which, without contradiction, deserves the attention of practitioners, on account of the abundance of its gelatinous, demulcent, and nutritious principle; moreover, the formulæ of M. Mouchon appear so rational that we hasten to lay them before our readers.

I. GELATINE OF CARRAGEEN.

Carrageen was submitted to three successive decoctions to almost completely exhaust it of its gelatinous matter, then the decoctions were evaporated on sand-baths, in a glazed saucepan to the consistence of a thick syrup, and the concentration was finished on the stove to almost perfect dryness, in moulds of white iron, covered with a thin layer of cacao butter, and furnished with gelatine, in such a manner as to form thin, transparent, and easily pulverised plates, especially by mixing the matter with sugar.

One gramme (15½ grains) forms 180 grammes of water into a jelly, and constitutes with 90 grammes of water, and as much sugar, an equal quantity of a sufficiently consistent product.

II. SACCHARIDE OF CARRAGEEN.

To obtain this product we have only to add to the gelatinous liquid, concentrated to the consistence of a thick syrup, four times as much sugar as carrageen. The operation is completed in a sand-bath, care being taken to stir the syrup often until every portion may easily be powdered.

III. TABLETTES OF CARRAGEEN.

℞—Saccharide of carrageen.. 500 grammes.
Gum tragacanth ..... 4 ,,
Common water ..... 45 ,,
M. and F.S.A. tablettes of 2 grammes, which may be aromatised at will.

IV. SYRUP OF CARRAGEEN.

℞—Carrageen ..... 30 grammes
Spring water .. 2,000 ,,
The carrageen is exhausted by boiling for half an hour, strained with pressure, and
Simple syrup of 30° .. 4,000 grammes
are added.

The mixture is heated to reduce it to the weight of the syrup employed.

This syrup appears to be able to be substituted with great advantage for syrup of gum arabic. It has a certain analogy with syrupum *heliciis* and with syrup of gum tragacanth, which may possess in certain respects the same medicinal properties.†

V. PASTE OF CARRAGEEN.

℞—Carrageen..... 123 grammes
Common water.. 12,000 ,,
It is boiled for half an hour with two-thirds of the water, and strained, and the

* *Gazette des Hôpitaux Civils et Militaires*, Dec. 24, 1844.

residue is submitted to a second decoction with the remaining water. After new pressure, the two decoctions are united, and there is added to them

White gum Senegal .. 1,000 grammes, which must be rapidly mixed in and dissolved with heat. The solution is strained; and there is added to it

Loaf sugar..... 1,000 grammes; then the product is exposed to the long-continued action of a boiling hot sand-bath; and, when it is judged sufficiently concentrated, it is laid in thin layers in moulds of white iron, on the surface of which cacao butter has been spread. The concentration is finished in a good oven, which completes it in a few days to such a point that the paste may easily be detached.

This paste, which is very agreeable to the taste, especially when it is aromatised with tincture of orange or any other aromatic, may be compared to the paste of lichen, and deserves even to be preferred to it.

VI. JELLY OF CARRAGEEN.

℞—Carrageen..... 2 grammes
Common water.... 250 „

It is boiled for half an hour, strained with pressure, and there is added to it,

Sugar, in pieces..... 40 grammes.

It is concentrated until the solution is reduced to 125 grammes, which is again strained, and aromatised with a few drops of tincture of orange or lemon.

This jelly has the advantage of being preserved intact for a very long time. When it is prepared without sugar, it contracts, after being kept for a few days, a slight fishy taste; but this never occurs when sugar has been added.

ON THE USE OF HEMP IN ALGER.*

BY M. LARUE DE BARRY.

HEMP, *tchkrouri*, is cultivated in Alger, in the regency of Tunis, and throughout the whole Orient. The smokers of hemp make use only of the leaves; and the stalk, so precious in our country, they throw away as useless. It is true, that it would be scarcely proper for the manufacture of tissues, for hemp, like all other vegetables of these climates, is short, meagre, and not vigorous.

Notwithstanding it is forbidden by religious tenets, the Arabians smoke every day the tchkrouri; some being good mustims make their *kif* (action of smoking the hemp) in their own houses, where they may be seen only by their wives and slaves, two sorts of beings that by the sectators of the husband of Aïcha are less esteemed than their horses; but they do commonly smoke publicly. With this view they unite in establishments called café of *achechs* (of the smokers of herbs), and stretch themselves on mats in a half sitting position; a negro, on seeing them accommodated, brings fire to light their pipes, and a cup of coffee, previously sweetened with cassonade. Some content themselves with fuming only one pipe of hemp on partaking of the coffee, but others take it with immoderation, and do not cease to smoke until they are intoxicated. Under the Turkish dominion the smokers of tchkrouri were punished with much severity, and the more so when the offender was a female. The last Bey of Constantine ordered the Moorish females to be beheaded when taken in *flagrante delicto*, whereas men came off with a few blows from a stick; but persons of fortune or of influential connexions, remain unremarked by the authorities.

Among females the use of tchkrouri is almost entirely restricted to public women; they prefer to take it in the shape of confections (madjoun). The madjoun tchkrouri is prepared by mixing honey and powder of hemp leaves, and boiling it by the means of a hot bath (bain-marie) during two or three hours. This preparation has the same effect on the nervous system as is produced by the vapor inspired by the tube of a pipe.

The hemp leaves are prepared by the Arabs in the following manner, they are first dried, then cut into small pieces by a convexed knife, and afterwards triturated in a mortar until a coarse powder is obtained. When it is to be smoked, this powder is put into a red earthen pipe to which a long tube of cherry-tree wood is attached. An Arabian, on taking his *kif*, is generally so much absorbed by this occupation, that he is quite a stranger to whatever passes around him; his eyes are fixed on his pipe, he imbibes the vapor but slowly; and better to relish it, he allows it to remain for some time in the cavity of the mouth, and finally swallows it. In this manner he smokes for hours without uttering a single word; the discordant sounds of the music, which is generally performed in the café des achechs, seems to make upon him neither a good nor a bad impression.

The use of hemp, however, occasions remarkable disorders of the imagination. The mustims, the moment they are under its intoxicating influences, experience vertigo, extasies, excess of hilarity, without objects producing them; vague and impetuous movements; oblivion of all that happened to pass. Some behold uninterruptedly a deep precipice opened before them; others imagine themselves to be seated on a wall, menaced with ruin. I

* *Journal de Chimie Médicale*, Jan., 1845.

noticed one day a Spahis, who on leaving the café was to go across the street, but stopped short with horror on the aspect of a little standing water. On having recovered from his fright, he approached the spot cautiously, measured its extent with his eyes, then retired a few steps, and jumped over the little lake; when after this he looked back, he laughed at the effort which he made.

The smokers of hemp entertain a platonic love for little boys, caressing them a thousand times; they treat them with much kindness, and deem it an infinitely great pleasure, a species of ecstasy, to fix upon them their eyes, dilated by the narcotic. They are particularly fond of partaking of porcupine meat in the company of children. Finally, according to these smokers, the use of the hemp is productive of delicious sensations, by which all objects of nature appear in a new aspect, and with fresh colors. This intoxication is attended by agreeable reveries, and the image of the sweetest pleasures, but the sensibility becomes dull, and it seems that under its influence they lose the faculty of transmitting excitability to the muscles. A man in this state receives contusions, and is even wounded to an extent, that in a normal state might have been dangerous. I saw one day an individual putting into his mouth ignited coals; another seizing upon a red hot iron; a third laying on the ground, allowing persons to dance on his belly, without complaining of it.

From the abuse of hemp result a taste for indolence and apathy, gross propensities, impure and ungovernable passions, cowardice, considerable weakness, and premature senility.

—nt—

FATAL EFFECTS OF SOAP-OINTMENT IN THE CURE OF THE ITCH.*

BY DR. BRAUN.

Maria Dursch, 20 years of age, maid servant in Fürth, died the 28th of December, 1843, at 2 p.m, suffering but for a short time under fits of fainting, and in the full possession of her senses, after an ointment against the itch was three times rubbed in. She was of a middle stature, of a vigorous fattish constitution, exhibiting no particular morbose disposition, without having any chlorotical appearance. She entered the hospital merely to be cured of the itch. She complained only of the energetical action of the remedy on her skin, and by the *post mortem* inspection nothing was found which could have been considered as the cause of the disease (death ?); everything was in the normal state.

An effect of the soap-ointment so rapidly fatal as this is certainly as yet a rarity in medical literature, though it may have taken place more than once. Dr. Bock, in his medico-statistical report on hospitals and charity establishments of Nüremberg, page 96, says:—"In curing adults of the itch, by the soap-ointment, we likewise meet with difficulties. Some patients do not bear the ointment treatment at all; a violent reaction takes place in the sanguine and nervous system, rendering it necessary to interrupt the cure; in others, most extensive furunculous eruptions are produced in the trunk and extremities, which retard the cure. Whereas the skin of others is so torpid, that after the ointment has been twelve times rubbed in, no erythrema appears, but sixteen or twenty rubbings were requisite to obtain that effect. In children, the cure with the ointment cannot be applied, either on account of the delicacy of the constitution, or because it does not answer; frequently it is followed by pertinacious herpetical excoriations. Baths with iodide of potassium or sulphuret of potassium, are better, answering the purpose, and are active in the most obstinate forms of the itch of children." The soap which he employs is prepared of liquor of potassa and cod oil, it is less annoying by its odor, and more uniform than the ordinary one.

The dangers attending the treatment with the gray soap may, therefore, serve to render us less stormy in curing an organ so vastly extended as that of the skin; the importance of which becomes manifest in the case of combustions or exanthemata, and to direct us not merely to endeavor to produce striking effects.

—nt—

* Henke's *Zeitschrift f. d. Staatsarzreikunde*, 3, 1844.

ON A CERATE WITH STEARINE; WITH REMARKS ON ARCANA.*

BY BARBIN.

Mr. Editor—Several of my colleagues have shown me a cerate of extreme whiteness, but when I asked them what was the formula for making it, they invariably remained silent. But now I no longer doubt that the formula, which will be given below, is identically the same as that which is at present in use, and I do not hesitate for a moment to communicate it to you, thinking that it is more gene-

* *Journal de Chimie Médicale*, Jan., 1844.

rous to publish any new acquirements, however little merit they may possess, than to keep them secret from pecuniary considerations. The number of individuals trading in specifics is but too great. The speciality is the lepra of our profession; it is the cancer which slowly and tacitly detracts from the well-being of pharmacy, and the evil must the more alarm us, as it emanates from the most civilised town, and, as it is too often, emboldened by being left uncensured. The grade of bachelor in pharmacy seems to have raised pharmacy; the suppression of patents ought to have rendered the traders in specifics powerless; but until now nothing was devised that could have put a stop to their speculations. What is in fact more discouraging for a young man, who has consecrated to his studies almost his whole life, who exhausted all his pecuniary resources to obtain a pharmaceutical diploma, than to see his prospects menaced by the progress of the specific monopolists. For it must not be overlooked, they endeavor to concentrate in themselves all pharmacy, prejudicing thereby real pharmacists and the whole society. Under these unfortunate circumstances, Mr. Editor, your assistance, joined with that of men having at heart the progress of science, becomes necessary. By your position you are enabled to fight with advantage against anything calculated to deprive us of our rights. Forgive this digression; I return to the formula. Take—

Stearine	6	ounces (180 gr.)
Water of roses.. ..	12½	ounces (375 gr.)
Oil of sweet almonds	1	pound (500 gr.)

F. S. A.

As it is seen, for the white wax stearic acid is substituted, exceeding by 2 ounces (60 grammes) the doses of the *Pharmacopœia*.

By this process we have the advantage of obtaining a ceratum, which is perfectly white and uniform, being unctuous to the same extent as *cold cream;* and may be prepared in an hour's time. In addition to these there is some economy in this preparation, which I would, however, not have mentioned, if not considering it as efficacious as that of the *Pharmacopœia*. I had thought it might be employed in hospitals instead of the yellow wax, which is much dearer than stearine.

Angers, 13th Nov., 1844.

[Note of the Editor—By inserting the above, we remark that it would be useful to nominate a commission to examine all new pharmaceutical products, and to give their opinions on the manner how to administer them, their efficacy and use, for we do not think that at present the ceratum with stearine might be substituted for that prepared with wax.] —nt—

THE GASTRIC FLUID, ITS NATURE AND PROPERTIES.*

M. Blondlot has recently published in Paris a Treatise on Digestion, detailing very numerous experiments made upon a dog, in which a fistulous opening into the stomach was maintained for upwards of two years. The gastric juice was obtained in very large quantities. Submitted to distillation, the fluid passing over did not exhibit the slightest acid re-action, whilst the residue in the retort was always strongly acid. It is therefore certain that the acid of the gastric fluid is neither hydrochloric nor acetic acid, since both these are volatile. The gastric fluid of other animals gave the same result on being distilled. When chalk or any other carbonate of lime is added, no effervescence ensues, which further proves the acid not to be the lactic. M. Blondlot concludes that the acid re-action of healthy gastric juice is owing to the presence of superphosphate and biphosphate of lime. He adds—1st. That there is no other acid which can remain acid, and fail to decompose carbonate of lime. 2nd. That sulphuric acid, added to gastric juice, precipitates an abundance of sulphate of lime, and oxalic acid precipitates oxalate of lime. 3rd. Potass, soda, ammonia, and lime water, produce abundant precipitates of neutral phosphate of lime. 4th. That calcined ash of gastric juice is not deliquescent, dissolves without effervescence in hydrochloric acid, forming chloride of calcium; it therefore contains neutral phosphate of lime, the excess of acid being drawn off in the calcination.

M. Blondlot also made many experiments to determine whether, during digestion in the healthy stomach, lactic acid is formed by the transformation of sugar, starch, or other substance, and his conclusion is, that it is never found. He could never find even a trace of it, although he analysed the fluid expressed from the contents of the stomach, after remaining on the stomach various periods. He conceives that the acid of the gastric juice prevents the lactic acid fermentation, just as other acids are known to do under other circumstances. In confirmation of this, M. Blondlot relates many experiments upon birds and ruminating animals, which show that the formation of lactic acid in these creatures takes place only in those parts of the alimentary canals where no acid is present—namely, in the crop of birds, the

* We are indebted to our excellent contemporary the *Lancet* for this and the following abstract of the memoirs published by the respective authors in the *Comptes Rendus*.

first and second stomach of ruminants, and the cœcum of man, and other animals. He first proves that the acid found in these cavities is not secreted by their walls. Feeding sheep, goats, chickens, and pigeons, on food destitute of sugar, and examining the fluid found in the cavities mentioned, he found it invariably alkaline. On the other hand, the addition of sugar to the food produced an acid fluid in the same cavities which proved to be the lactic. The contents of the cœcum are not more acid than those of the small intestines, except sugar has been taken in the food; but when sugar has been taken, it undergoes the lactic fermentation in the cœcum. These experiments agree with those of Mr. Ross, published in the *Lancet* for January 20 and February 10, 1844. Tiedemann and Gmelin found acid in the crop of a pigeon, which had fed for several days on nothing but meat; but this, as M. Blondlot shows, probably had regurgitated from the stomach—an incident requiring precautions to prevent, after death.

M. Blondlot believes that the digestive property of gastric juice depends, not on its obvious chemical constitution, but upon a peculiar organic principle. If exposed to a temperature of 104° to 122° F., or higher, it loses entirely and irrecoverably its digestive powers, although to all appearance, and even as to its composition, as made known by analysis, it remains unchanged. With the exclusion of the air, gastric juice may be kept for two years without loss of its activity; but with the free access of air, it putrifies in five or six days, although the chyme which it forms from nitrogenous organic substances may be preserved for two or three months without change. The precipitation of all the lime it contains does not affect its activity, nor are its chlorides indispensable, but whatever acts upon its organic constituents, heat, strong alcohol, or strong acids, or which removes them, such as animal charcoal, chlorine, tannic acid, or acetate of lead, destroys all its digestive properties.

M. Blondlot also shows—A. That coagulated albumen resists the action of the gastric juice only from its compact form. When coagulated in very small particles, as white of egg beaten into a froth and poured into boiling water, it is digested as quickly as soft fibrine. B. That the action of the stomach in coagulating milk is not due to its digestive principle solely, but to its acid, which acts like lactic acid. C. The effect of the gastric fluid upon bones, whether entire or not, is to disintegrate the animal matter slowly, beginning at the surface, and to reduce the earthy matter into a fine chalky powder, but without dissolving or decomposing it. The earthy matter not being dissolved, proves that no hydrochloric acid has acted upon it, but it all is discharged with the fæces.

The physiological results of M. Blondlot's experiments confirm those of M. Beaumont, which are already familiar to our readers.

ANOTHER CHEMICAL INVESTIGATION OF THE GASTRIC JUICE.

Since the work of M. Blondlot was published, two other French chemists, MM. C. Bernard and C. Barreswil, have made an experimental investigation into the properties of the gastric juice. They start with the assumption that this fluid owes its digestive properties to the union of two principles: 1st, an acid; 2nd, a peculiar organic matter destructible by heat. What is the nature of the acid? "The principal fact which has been adduced to prove that the acid reaction is owing to the presence of biphosphate of lime is, that it may be treated with carbonate of lime without effervescence. Our experiments show that this arises from the dilution of the acid, which allows the carbonic acid to be dissolved as it is formed. When, therefore, the gastric juice is concentrated, it causes a considerable effervescence with chalk. Moreover, gastric juice dissolves neutral phosphate of lime, whilst this salt is entirely insoluble in solution of the biphosphate." On distilling gastric juice, the first distillate exhibits no acid reaction. If a mere trace of acetic acid or acetate of soda is added, and afterwards distilled, it gives an acid reaction; the normal acid is not therefore acetic. This also appeared, at first sight, to prove it could not be hydrochloric acid; but on distilling water rendered slightly acid by hydrochloric acid, nothing passes over at first but pure water, the acid not distilling until the end of the operation. On distilling gastric juice a neutral limpid liquor passes over, which does not precipitate nitrate of silver; but when about four-fifths has distilled over, the distillate is perceptibly acid, yet, nevertheless, it does not render solution of nitrate of silver turbid; but at the end, and when only a few drops of the gastric juice remain in the retort, an acid liquid passes over which precipitates salts of silver; this was, doubtless, hydrochloric acid. Does this acid exist free in gastric juice, or has a chloride been decomposed in this operation? When the least trace of oxalic acid is added to gastric juice which we know contains lime, a turbidness is produced from the formation of an insoluble oxalate of lime; but if to water acidified with 2,000ths

of its amount of hydrochloric acid, and containing chloride of lime, the same re-agent be added, no turbidness ensues. This clearly proves that hydrochloric acid exists as a chloride in the gastric juice, and not in a free state.

When concentrated by evaporation, gastric juice is strongly acid, effervescing with chalk, and not losing its acid reaction in presence of an excess of the chalk. This proves the presence of *phosphoric acid.* On saturating the acid with lime and oxide of zinc, and filtering the solution, the neutral filtrate contains both zinc and lime, therefore the phosphoric acid is not the only free acid in the juice. What is the acid combined with the zinc and lime in the filtered solution? It is one which, as we have seen, passes over the end of the distillation, and does not precipitate salts of silver. These characters belong to lactic acid. On distilling water slightly acidulated with lactic acid, and a small quantity of chloride of sodium added, it presents a complete analogy to gastric juice; first, pure water passes over, then an acid which does not precipitate salts of silver, and the last drops carry over hydrochloric acid. So that it is evident that the presence of hydrochloric acid in the last product of distillation of gastric juice is owing to the decomposition of the chlorides by lactic acid.

Hydrochloric acid cannot exist in a free state in presence of a lactate, a phosphate, or an acetate. "We have observed," says the authors, "in the acid of the gastric juice all the characters of lactic acid, as pointed out by M. Pelouze; both give soluble salts of lime, barytes, zinc, and copper, a double salt of copper and lime, deeper in color than the simple salt, a salt of lime soluble in alcohol, precipitated by ether." From the above facts, MM. Bernard and Barreswil conclude that the acid reaction of the gastric juice is not owing to biphosphate of lime, but arises from a free acid, which is not hydrochloric acid or acetic acid. They have always found lactic acid, with a minute proportion of phosphoric acid, the latter being a product of the reaction of the lactic acid on the phosphates present. According to their opinion, lactic acid is a constant production of the stomach. They do not mean to say that the digestive powers of the gastric juice are owing to lactic acid; on the contrary, they think if an acid reaction be indispensable, other acids may supply its place, because among the various salts constantly introduced into the stomach with the food, some will have their acid replaced by the free lactic of the stomach, and the new acid liberated may supply the place of the normal acid.

REMARKS ON ERGOT OF RYE*

BY DR. RITTER.

ERGOT of rye is shown to be useful:—

1. In cases of retention of urine and fæcal matter by commotion of the vertebral column, paralysis of the bladder and rectum.

2. A case of ischuria which had for seven days resisted a number of agents, yielded immediately to a draught formed of an infusion of a drachm of ergot of rye in four ounces of water with the additioin of a drachm of tincture of ergot of rye and syrup.

3. Four individuals, aged from 8 to 16 years, afflicted with nocturnal incontinence of urine, took morning and evening from half a tea-spoonful to a tea-spoonful of tincture of ergot of rye. One was cured; two have improved, insomuch that instead of wetting the bed every night, they do so only once or twice per week. The state of the fourth is ameliorated only when tincture of cantharides is added to the ergot of rye.

4. From observations of accouchment in which the ergot has been administered.

5. Five cases of uterine hœmorrhage taken from more than fifty cases in which this medicine has arrested the loss of blood.

OINTMENT OF SNAILS.†

BY E. SOUBEIRAN.

℞— Snails of vineyards..... 50 pieces.
White wax..... 1 lb.
Oil of sweet almonds.... 4 lb.
Oil of roses..... 2 drops.

The flesh of the snails is first reduced to a pulp, in a mortar, and then a cerate is formed of the wax and oil; the pulp of the snails is afterwards mixed with the ceratum, and finally the oil of roses is added.

The pomatum of snails is employed in fissures of the skin and mamellæ. —nt—

THE EFFECTS OF ELECTRO-MAGNETISM IN RESTORING SUPPRESSED MENSTRUATION.‡

BY DR. COLLINS.

A FEMALE, aged thirty-five, unmarried, rather delicate constitution; her menses were stopped by taking cold some seven months before I saw her. I tried all the usual remedies prescribed in such cases, but I did not succeed in the desired effect. I had

* *Medicinisks Annalen.* Published by Drs. Puchelt, Chelius, and Nägell.

† *Journal de Chimie Médicale*, Jan. 1845.

‡ *New York Journal of Medicine.*

been using the electro-magnetic machine in some other cases with the happiest effects; and I therefore was induced to use it in this case, in which it succeeded most perfectly. I applied the buttons connected with the machine over the region of the womb, holding one on the lumbar region of the spine, and the other in front over the public region; using the negative and positive wires alternatively to the spine and abdomen. I continued the remedy five days, from five to ten minutes each day, when her menses were fully re-established. I have used it in rheumatism with good effect.

POISONING OF A HORSE BY THE COMMON ROSEBAY (OR, OLEANDER, NERIUM OLEANDER, LAURIER-ROSE.)*

BY M. LARNE DE BARRY.

M. DE B—, treasurer at Bona, had a horse which was attacked by a cutaneous disease, chiefly affecting the head; a farrier having considered this as a case of itch, and treating it as such, ordered antipsoric lotions, on the application of which the exanthema entirely disappeared.

After some months a fresh eruption, more violent than the former, covered the head and part of the neck. M. de B., who had been told that the Arabians employed with success the common rosebay in the treatment of the itch, resolved on trying it in the case of his horse; he made a decoction of the leaves of the nerium, and repeatedly washed with it the neck and head of the horse. This preparation was so concentrated, that it could be drawn in threads like molasses. A short time after the application of this remedy, the horse had vertigo, staggered for some minutes, and, under horrid convulsions, it fell and expired within two hours. All the hairs moistened by the said decoction fell out. —nt—

PROCESS FOR THE DETECTION OF ARSENIC.†

BY M. BEVAN.

Of a certain quantity of nitric acid, diluted with four times its weight of water, as much is to be put into a narrow tube, sealed at one end, as will fill half of it. A bright copper sheet is then introduced into the tube, and the acid liquid covered by a layer of melted tallow, which, on cooling, forms a partition wall, separating the immersed portion of the copper sheet from that which is free. On the tallow the arsenious solution is placed, and after six hours the metallic arsenic separates, being deposited on the copper.

By this process $\frac{1}{100}$ of a grain of arsenic may *quantitatively* be determined.

The same results may be obtained in a shorter time by a zinc plate.

—nt—

* *Journal de Chimie Médicale*, Jan., 1845.
† *Ibid.*

THE RELATIVE FREQUENCY OF LEAD COLIC IN DIFFERENT TRADES.*

Manufacturers of white lead	406
,, ,, minium (red lead)	63
,, ,, mine orange	12
Painters of buildings	305
Carriage painters	47
Ornamental painters	33
Painters on porcelain	3
Gilders on wood	1
Painters and varnishers of metals	2
Paper-stainers	2
Grinders of colors	68
Manufacturers of German cards	13
,, ,, glazed cards	6
Belt-makers	2
Perfumers	2
Manufacturers of common earthern pottery	54
,, ,, a finer species of pottery (Faïence)	7
Refiners	25
Plumbers	14
Platers (in tin and lead)	8
Manufacturers of the powder of tin (used in polishing)	4
Workers in tin	4
Jewellers, goldsmiths, and manufacturers of toys	4
Casters in copper	2
,, bronze	1
Type-founders	52
Printers	12
Manufacturers of shot	11
Lapidaries	35
Cutters and polishers of crystals	3
Laborers in glass manufactories	2
Do. in manufactories of the acetate of lead	4
Do. do. nitrate of lead	3
Do. do. chromate of lead	3
Total	1213

* *Lancet.*

RÉSINÉONE OF TAR, A THERAPEUTICAL AGENT.*

BY DR. PÉRAIRE.

Dr. Péraire, knowing long ago that tar is an efficacious therapeutical agent, but that owing to its being disliked by most patients, it acquired only little reputation in medicine, he contrived to separate from this body, so disagreeable in taste and odor, the active matter, which may easily be employed. The important researches of M. Frémy (*Ann. de Chimie et de Physiq.* vol. lix.) paved the way for M. Péraire. This distinguished physician, however, experimented with the tar itself, and not with the resin. The process which he adopted is as follows:—

It is known that on distilling tar, either with water or unmixed, an oil is obtained, which is called *oil of cade*. M. Péraire subjected this product to a fresh distillation, and the results were nearly the same which M. Frémy obtained from the resin.

Previous to commencing the operation, a fragment of potassa is put into the oil of cade, to saturate any acids present. On placing the mixture in a retort, and applying heat, it behaves in the following manner:—Like the resin, the tar oil boils at 158° F., and on distillation a colorless oil passes over, which is the *résinéone;* by raising the heat to 297½° F., *résinéone* is obtained in a more abundant quantity; and at 482° F., *résinéine* is obtained. If at each period of the operation the receivers were exchanged, the three products of tar may be separately collected.

Dr. Péraire subjected these three substances to chemical experiments. He found that résinéone has precisely the same properties as the tar, and that the healing art ought to have recourse only to the colorless oil, substituting it for the impure matter; résinéone and résinéine seemed to be much less active.

The recently prepared résinéone of tar is a liquid, colorless, essential oil; of a penetrating odor. Placed on the tongue, it provokes a sense of heat, and an acrid taste. Shortly after this sensation disappears, and a balsamic odor is perceived, of an agreeable nature. The résinéone is endowed with all the properties of essential oils; it is inflammable, soluble in ether. By distilling five parts of résinéone with 100 of alcohol, an alcoholate is formed. By mixing these two substances in a cold state, the same product is rapidly obtained; the preference is to be given to the latter process. By this means it is easily determined quantitatively. The protracted influence of light imparts to résinéone a slight yellowish tint. By mixing 995 parts of sugar with 5 parts of résinéone, a saccharate is formed, containing ½ per cent. of résinéone. This saccharate is given three or four times a day, in doses of spoonsful, with s cupful of an infusion. It is employed with advantage in catarrhs of the lungs, chronic bronchites, and bronchorrhœa.

The saccharate may likewise be administered in the form of a looch, in the following manner:—

White looch of the Pharmacop. (French)..... 125 parts.
Saccharate of résinéone..... 30 do.

Five parts of résinéone mixed with 995 parts of oil of sweet almonds formed an oleate; in this form the résinéone easily enters into the composition of loochs. 15 parts of it are given with 125 parts of a potion.

By mixing 990 parts of sugar with 5 parts of résinéone, and 5 parts of gum, a mixture is obtained, well qualified for pastilles.

The alcoholate of résinéone, containing ½ per cent. of résinéone, is to be taken on sugar, at the doses of six drops four times a day. It is recommended in chronic catarrhs of the bladder.

Externally the résinéone seems to have rendered remarkable services in cutaneous diseases.

A pomatum may be formed of it in the following manner:—

Ceratum, without water..... 30 parts.
Résinéone ..... 4 do.
Mix.

M. Péraire has administered this mixture with success in several cases, which are commonly called dartres (tetter). (*Gazette Médicale de Paris*, 14th Dec., 1844.)

We think that with reference to cutaneous diseases, M. Péraire could have employed the résinéone with more advantage in the *squammous affections*, viz., in psoriasis, ichtyosis, prurigo, &c., than in cases of dartres (tetter), or with a humid secretion. At least, tar was hitherto employed with more success in those affections, than in cases of dartres. —nt—

* *Journal de Pharmacie et de Chimie*, Jan., 1844.

ON THE ACTION OF DIGITALINE.*

The Pharmaceutical Society of Paris fixed a prize of 1,000 francs (40*l.* sterling) to be awarded to the author of the best treatise on the *digitalis*. This prize was

* *Lancette Française*, Jan. 11, 1845.

obtained by M. Homolle, M.D., of Paris. The author of this treatise sent to the society a specimen of *digitaline*, with a description of the process for preparing it.

The author observed the action of digitaline on himself and other individuals; from which it appears that digitaline, taken at the doses of from 5 to 20 miligrammes ($\frac{1}{10}$ to $\frac{1}{10}$ of a grain) retards the circulation, and may develop phenomena of poisoning, such as nausea, dimness of sight, headache, and vertigo.

From experiments of M. Martin Solon, it appears that digitaline is still more active, that the author supposed the latter gentlemen proved that, taken at the doses of 1 to 3 miligrammes ($\frac{1}{50}$ to $\frac{3}{50}$ of a grain) per diem, it had a sensible effect on circulation, as a mean 72 pulsations in a minute were reduced to 55; when the doses were augmented to 1 centigramme ($\frac{1}{5}$ of a grain) the phenomena of poisoning scarcely eve. failed to appear.

The diuretic action signalized by the author, was not confirmed. The whole of the observations of M. Martin Solon induce us to assume, that the proper doses of digitaline are from 1 miligramme to 1 centigramme.

ON THE ACTION OF CERTAIN REMEDIES ON VARIOUS FACULTIES OF THE MIND.

BY DR. OTTO, OF COPENHAGEN.*

All exciting substances cause the blood to run towards the brain, and if the doses of the substance employed do not exceed certain limits the intellectual faculties are excited; but it has been observed, that each exciting substance acts in a manner peculiar to itself. Ammonia and the preparations of which it constitutes the base, moschus, castoreum, wine, and ether, excite the imagination, and render meditation more easy. Empyreumatic oils induce melancholy, sadness, hallucinations. Phosphorus stimulates the organs of generation, and cases of impotence are on record, in which it was administered with success. Iodine produces analogous effects; but it at the same time impairs the intellectual faculties. The same is the case with cantharides; whereas with camphor the reverse is obtained. Arsenic induces grief, gold gaiety; mercury fosters a morbid sensibility and a striking reluctance to all occupation.

Of gaseous bodies the author mentions only the protoxide of nitrogen, which on being inhaled produces sensations of delight.

All the narcotics act in the same manner. Opium heightens the erotic instinct, as well as the intellectual faculties, and especially the imagination; its abuse gives frequently rise to priapismus; in small doses it creates ideas and provokes, hallucinations in a manner that the mind seems to be awake, whereas the organs are in a dormant state. Dr. Gregory noticed loquacity to be an effect produced likewise by opium, especially when exhibited in the form of hydrochlorate of morphine; he states to have observed this phenomenon in all individuals to whom he has administered this substance, and that he has experienced the same with his own body. After he had partaken of the salt of morphine, he says he felt an irresistible propensity for loquacity gaining over him, with a remarkable facility of elocution. He even goes so far as to recommend to persons about to speak in public to partake of a small dose of opium.

The other narcotics do not produce in the brain the same effects as the preparations of opium; thus, belladona diminishes the intellectual activity by hyoscyamus, the individuals become afflicted, violent, irascible; cicuta greatly lessens the activity of the intellect; digitalis acts in the manner of aphrodisiacs; hemp induces an inexhaustible fund of gaiety, owing to which the Indians make it an ingredient of several intoxicating beverages; the amanite fausse oronge (agaricus speciosus) brought the ancient Scandinavians into a savage fury, a courage bordering on ferocity; finally, tobacco, even on persons habituated to its use, has an effect similar to that of opium, in a manner that smokers maintain that it excites ideas in them.

—nt—

* *Gazette Médicale*, 14th January, 1845.

ASHES OF HUMAN BLOOD AND SALIVA.*

100 parts of the human blood contain—

Tribasic phosphate of soda	22·1
Chloride of sodium	54·769
,, potassium	4·416
Sulphate of soda	2·461
Phosphate of lime	3·636
,, magnesia	0·769
Oxide of iron with phosphate of iron	10·77

100 parts of the ashes of the saliva contain—

Tribasic phosphate of soda	28·122
Chlorides of sodium and potassium	61·93
Sulphate of soda	2·315
Phosphates of lime, magnesia, and iron	5·509

* *Enderlin, Annalen*, and *Lancet*.

A NEW THERAPEUTICAL PROPERTY OF OATS.*

BY DR. THÉMONT.

ACCORDING to Dr. Thémont, oats are endowed with a highly diuretical virtue. He employs it in the form of a decoction, in the following manner:—

Oats ..... 2 handfulls
Common water 6 pounds

Boil until four pounds are left, and strain it.

The decoction is to be administered in cups from time to time.

Dr. Thémont treated a case of dropsy from hypertrophy of the heart. After the patient had partaken of the decoction of oats for three days, a large quantity of urine was emitted, which having continued for several days, removed the dropsy.

—nt—

PRIZES PROPOSED BY THE PHARMACEUTICAL SOCIETY OF PARIS.*

THE Pharmaceutical Society of Paris proposes the following three subjects as matters of prizes:—

PRIZE ON ALCOHOLIC TINCTURES.

To determine, by exact experiments, which are the most convenient stages of concentration of alcohol, to prepare with it medicinal alcoholic tinctures; which is the quantity of alcohol indispensable to dissolve all the active parts of medicinal substances most commonly employed.

The prize is a gold medal, value 500 francs. (Vide programme, *Journal de Pharmacie et de Chimie*, t. iii., p. 91, Jan., 1833.)

PRIZE ON THE ANALYSIS OF SCILLA.

An analysis of scilla, made by the means of data acquired by science since the labor of Tilloy, cannot fail to yield results full of interest. The Pharmaceutical Society demands that the candidates for the prize would make a new analysis of scilla, and that they would particularly take care to obtain in a pure state the acrid and volatile as well as the bitter and fixed principles, and would compile a complete description of these products. They are to add to their labor some therapeutical experiments, by which the medicinal action of these bodies might become apparent, and the relation which exists between their properties and those of the *squames* of scilla.—Prize, a gold medal, value 500 francs.

PRIZE ON THE ACTION OF ALKALINE BASES ON NITROGENOUS ORGANIC BODIES.

The Pharmaceutical Society does not expect the investigation of the action of alkalies on a great many substances, and without fixing upon any particular substance, it demands the complete study of the alkalies on nitrogenous substances derived from organic matters, the determination of the various reactions which may follow, and the analysis of the products obtained, that it may become possible to derive from these facts some general consequences. The Society particularly requests to have in view the four principal neutral matters of the animal economy—viz., fibrin, albumen, gelatine, and caseïn. Nothwithstanding a work on the products derived from organic substances might be very interesting, it would not constitute a subject for the prize of the Pharmaceutical Society. The memoirs, written in French or Latin, must be sent in, postage paid, to M. Soubeiran, General Secretary to the Pharmaceutical Society, Rue de l'Arbalette, in Paris,* before the 1st of July, 1846, with reference to the two last, and before the 1st of Aug., 1845, with reference to the first.—The prize is a gold medal, value 500 francs.

—nt—

EXPERIMENTS WITH REFERENCE TO THE PHYSIOLOGY OF VEGETABLES, INSTITUTED WITH ARSENIOUS ACID, WITH NEW ANTIDOTES.†

BY M. A. CHATIN.

M. CHATIN presented a memoir in the sitting of the Royal Academy of Sciences of Paris, Jan. 6, 1845, in which he examines the principal functions of vegetables, by means of arsenious acid, and chiefly dwells on the history of the excretive functions.

In concluding this labor, which is only the first part of more extensive researches on the excretive functions of plants which he intends to make, the author points out the following circumstances, from which legal chemistry and therapeutics may derive advantages:—

1st. The complete elimination of arsenious acid by plants, proves that no vestige of it remains in cereals, the seeds of which have been impregnated with arsenic.

2nd. On comparing the results of this

* *Gazette des Hopitaux*, 14th January, 1845.

* *Lancette Française*, 14th Jan., 1845.

* Address, in French, A. M. Soubeiran, Sécrétaire Général de la Société de Pharmacie, Rue de l'Arbalette, à Paris.

† *Gazette Médicale de Paris*, Jan. 11, 1845

labor with those obtained on a former occasion, when experimenting with animals, it will be observed that heat produces the same effect on both animals and plants. Would not the analogy of this fact, which was alone comparatively observed, warrant us in attempting to ascertain, in a therapeutical point of view, whether *calm* and *humid air*, *obscurity* and *electricity*, acting, by a continued influence, do not exert a favorable influence on man and animals, in the first period of poisoning; and, on the contrary, whether dry air set in motion, a flash of light, &c., are useful in that second period of poisoning, in which *absorption* taking place, the *elimination* must be promoted.

3rd. Arsenious acid, absorbed by plants, and having entered into a saline combination with the alkaline bases of the juices of plants, being neutralised by chloride of calcium, and by the alkaline constitution of animal blood, it being probable that the moment the arsenious acid is absorbed, an analagous combination is produced in the latter liquid, it is sufficiently indicated, that chloride of calcium must be the antidote against arsenic, absorbed by man and animals. —nt—

ON CONCHALAGHA; CHIRONIA CHILENSIS.*

BY M. F. LEBEUF.

At the sitting of the Royal Academy of Sciences of Paris, on the 13th Jan., 1845, M. Lebeuf, of Bayonne, sent in a note on the above plant. It is popularly known in Brazil and Spain, and prescribed there in sanguine congestions and different affections in which it is intended to render more dilute the blood, and to regulate the circulation,—a plant, says the author, which, in the country of the precious bark, is no less esteemed than that very substance, deserves the attention of men of science. —nt—

PETRIFACTION OF ANIMAL SUBSTANCES.*

BY M. BALDACCONI.

At the sitting of the Royal Academy of Sciences, in Paris, on the 6th January, 1845, Dr. Montucci communicated a note received from the Abbé Baldacconi, Conservator of Natural History in Sienne, respecting a process contrived by the latter to petrify animal substances. The means employed is a long immersion in a highly saturated solution of twelve parts of bichloride of mercury and one or two parts of hydrochlorate of ammonia. The hydrochlorate of ammonia seems to determine the complete petrifaction, and preserve the natural color of the organs; but does this last long if the bichloride of mercury is employed alone? As a specimen, he presented a petrified liver of a dog, its natural forms and tints of color being preserved. —nt—

* *Gazette Médicale*, 18th Jan., 1845.
* *Gazette Médicale de Paris*, 11th Jan.

WAX EMULSION.*

BY M. J. JACK.

To suspend wax in an emulsion generally one of the fat oils is employed, as, for instance, oil of almonds, &c. M. Jack uses no oil, and prepares a mixture better than those obtained in the usual manner. The following is his method of preparing it:—

℞—Gum arabic ½ oz.
Water .... ½ oz.

Put the whole in a mortar, and apply heat by a spirit lamp; after the gum is dissolved, add,

Melted white wax, 2 drachms.

The wax seems at first to resist, but soon liquifies; the mortar is removed from the fire, well shaken, and gradually added,

Boiling water, 3—4 ounces.

When the whole is mixed and presents a uniform appearance, it is introduced into a medicine bottle, and continually shaken until it is cold; after which operation other substances which might have been prescribed—waters, syrups—are added. Prepared in that manner, this mixture entirely resembles an oily emulsion, and is perfectly homogeneous. —nt—

DAPHNE MEZEREUM IN THE TREATMENT OF PSORIASIS.†

BY DR. CAZENAVE.

Generally M. Cazenave administers a simple decoction of smilax china (squire) or sarsaparilla, to which he adds a small quantity of daphne mezereum. Thus he frequently prescribes in the following manner:

Sarsaparilla cut in small pieces... 2 oz.
Water.....20 oz.

Boil to obtain a residue of two-thirds; add, during the last ten minutes of boiling, of daphne mezereon 15 grains. Strain;

* *Journal de Chimie Médicale*, Jan. 1845.
† *Ibid.*

sweeten with syrup of smilax china; dose, three glasses per diem.

M. Cazenave also prescribes the daphne mezerium as a syrup to sweeten the decoction of sarsaparilla, or he allows it to enter into a mixture, in which a body is contained, endowed with analogous properties, for instance:—

Hydrosulphate of soda.... 1 drachm.
Syrup of daphne mezerium..4½ oz.
Syrup of smilax china.... 4 oz.

M.S.A.—Take one large spoonful every morning and evening. This treatment is generaly combined with steam-baths.

—nt—

CREOSOTE IN NÆVI MATERNI.*

According to M. Thorsten, of Havelberg, creosote is an excellent topical remedy in deformities which are *nævi materni*. With this intention the creosote is to be diluted with more or less water, in which pieces of linen are steeped and applied as compresses, to be renewed every eight hours. By the application of this remedy, the surface of the nævus becomes excoriated, changes and disappears. The scar which follows is smooth and has a good aspect. —nt—

TANNIN A CURE FOR TOOTHACHE.

Mr. Druitt, in recommending the use of tannin as a remedy in cases of sore nipple of salivation by mercury, of aphthæ, &c., says—"But of all the cases for which it is adapted, that common troublesome complaint, toothache, is that in which I believe it is most to be depended on. When the tooth aches, let the patient wash out the mouth thoroughly with the solution of carbonate of soda in warm water; let the gum around the tooth, or between it and its neighbours, be scarified with a *fine* lancet; then let a little bit of cotton wool, imbued with a solution of a scruple of tannin, and five grains of mastic, in two drachms of ether, be put into the cavity, and if the ache is to be cured at all, this plan will put an end to it in nine cases out of ten."

TINCTURE OF CANTHARIDES IN ECZEMA AND PSORIASIS.†

BY PROFESSOR OTTO.

In four cases of eczema, and six of psoriasis, the tincture was employed with remarkable success, in the dose of three drops *per diem*, increasing one drop daily. The treatment was continued from six to seven weeks.

* *Journal de Chimie Medicale*, Jan., 1845.
† *Zeitschrift für die Gesammte Medicin.*

ON THE YEAR RINGS IN TREES.*

Professor Göppert, of Breslau, communicated to the Silesian Agricultural Society a remarkable piece of a trunk of Rothbuhn, hewn in 1841, and in the woody substance of which the number 1809, was found to have been cut in, being covered by 32 year rings; this may serve as a new corroborating proof that we are warranted to determine the age of a tree by the number of rings it offers.

—m—

THERAPEUTICAL EMPLOYMENT OF THE LEAVES AND STEMS OF THE POTATOE.†

BY J. S. PLUSKAL, OF LOMNITZ.

M. Pluskal announces that an experience of several years has proved to him the utility of the items and leaves of the potatoe, either in recent state, or dried, for the preparation of emollient cataplasms.

It was particularly in his excursions into the country, where he had been unable to procure the medicine necessary for fulfilling the various indications presented by the patients, that this surgeon conceived the idea of adopting the employment of this medicinal agent; and that he has become convinced of its efficacy in cases of tumors, sore throat, very painful hæmorrhoids, spasms of the bladder, dry and difficult accouchments, diarrhœas, and dysenteries.

To prepare the cataplasms, he reduces the stems and leaves to the state of pulp, with or without decoction, according to circumstances.

TINCTURE AGAINST CALVITIE.‡

BY DR. LANDERER, OF ATHENS.

℞—Leaves of cherrylaurel .. 60 grms.
Cloaves ..... 8 ,,
Tincture of lavender.... 180 ,,
Tincture of origanum .. 180 ,,

Digest for six days, S.A.; strain with pressure, then add, after filtration,

Sulphuric acid..... 15 grammes.

Mix, and keep a bottle hermetically sealed.

This tincture is employed in frictions on the *cuir chevelu*.

Its effect, according to the author, is perceptible after five or six applications.

* Froriep's *Notizen*, vol. xxxi., p. 204. August, 1844.
† *Oesterreich Medicin Wochenscript.*
‡ *Zeitschrift für die Gesammte Medicin.*

IV. REVIEWS AND NOTICES OF BOOKS, &c.

OUTLINES OF CHEMISTRY, FOR THE USE OF STUDENTS. By WILLIAM GREGORY, M.D., Professor of Chemistry in the University of Edinburgh. Part I. Inorganic Chemistry. London: 1845. 12mo. pp. 236.

DR. GREGORY, whose election to the chair of Chemistry in the University of Edinburgh, rendered vacant by the retirement of the late eminent Dr. Hope, gave us at once pleasure and satisfaction, has, in the publication of this work, conferred a great boon on all desirous of acquiring some insight into chemistry previous to attempting an attack upon the several formidably ponderous works, called by their authors "Elements," or, with singular lack of accuracy, "*Manuals*." Let it not be suspected that we think disparagingly of these really very excellent works; on the contrary, we recommend the study of them—especially Turner's (edited by Liebig and Gregory), Brande's, and Graham's; but we consider the previous attentive perusal of such a book as that now before us, to be far more judicious than a sudden plunge into larger, more elaborate, and more difficult works.

Many persons are absolutely intimidated and deterred from the study of Chemistry, by the enormous size (not to mention the price) of these works. The volume before us, however, is small, concise, clear, and intelligible in style; and few will rise from its perusal without a desire for further insight into the beautiful science of which it treats.

We could write pages on the value of such a work as Dr. Gregory's "Outlines;" but space confines us to the above; we therefore give, for the benefit of such of our younger friends, as may intend to devote themselves to the study of chemistry, the following extract:—

"COMBINATION.

"The force of chemical attraction, or affinity, is unequal in the case of different substances. Thus, the affinity between potasium and oxygen is more powerful than between gold and oxygen.

"The capacity of one body to unite with another is mainly affected by two circumstances; namely, the state or form of the substances in question, whether solid, liquid, or gaseous, and the temperature at which they are brought together.

"Cohesion tends to keep the particle of bodies in close proximity, while the tendency of heat is to separate them from each other. When cohesion predominates over the repulsion due to heat, the body is solid: when cohesion and repulsion are exactly balanced, it is liquid; and when repulsion predominates, it becomes gaseous.

"It is obvious that, since chemical attraction operates between the particles of different bodies, and only when they are at infinitely small distances, the cohesion between the particles of two solid bodies respectively must impede chemical action by preventing the sufficiently-close approximation and intermixture of the particles which have affinity for each other. Hence the solid form is most unfavorable to chemical action, although, in rare cases, the power of affinity is sufficient to overcome the obstacle. Thus, phosphorus and iodine, both in the solid form, act powerfully on one another.

"But if one or both of the bodies be liquid, the particles of both readily come so near as to admit of affinity producing its full effect. Thus bromine acts violently on phosphorus, although the latter is solid; nitric acid, in like manner, dissolves metals, and the same acid acts, with great energy, on alcohol. Indeed, the liquid form is so favorable to chemical action, that the chemist generally endeavors to have one or both of the substances, whose actions he wished to try, in that form.

"Moreover, when two solid bodies, as is generally the case, refuse to act on each other, it is commonly sufficient to cause one of them to assume the liquid form. This may be done in two ways; either by applying a sufficient heat to melt it, or by dissolving it in water or some other solvent. Thus, iron and sulphur, when mixed in the solid form, do not combine; but if the sulphur be melted by heat, rapid combination takes place. Again: citric acid and carbonate of soda do not act on each other when dry; but if water be added to the mixture, the acid dissolves, and brisk action ensues.

"It is evident that heat, being opposed to cohesion, will promote chemical action whenever cohesion or the solid form is the obstacle: and this is the source of the immense value of heat in chemical and manufacturing processes: as, for example, in the smelting of metals from their ores.

"But when cohesion has been still further overcome, and the body has assumed the

gaseous form, a new impediment is offered to chemical action. The particles, by the predominance of the repulsion due to heat, are now so far removed from each other, as not to come readily within the sphere of chemical affinity. Hence, two bodies in the gaseous form seldom act on one another, unless their mutual attraction be very strong, as in the case of hydrochloric acid gas and ammoniacal gas; or by the aid of heat, light, or electricity, as in the case of chlorine and hydrogen gases. As heat is here the cause of the obstacle, the appropriate remedies are cold and compression, which tend to bring the particles nearer. Heat and electricity are supposed to act by producing compression of some particles in consequence of the expansion of others; but heat certainly acts also by increasing the force of affinity.

"Even where only one of the bodies is gaseous, chemical action is much impeded, especially if the other be solid. Still, in many cases, solids and liquids do slowly act on gases; and by such means some of the most important processes in the organic kingdoms are carried on: as, for example, the respiration of animals, and the growth and decay of plants.

"In some cases, as in that of hydrochloric acid gas, the affinity between the gas and water is so powerful, that they combine instantaneously when brought into contact.

"There are other cases in which solid bodies, at the ordinary temperature, are incapable of combining with gases, but in which a high temperature, although it opposes chemical action by its tendency to remove still further the particles of the gas, yet, on the other hand, so much exalts the power of affinity as to be the most powerful promoter of combination. The effect of heat in causing wood, coal, or charcoal, to combine with oxygen, as in ordinary combustion, is a familiar example of this: and as, in this form of experiment, the combustion, once begun, produces a great amount of heat, without external aid, it is in this manner that heat is obtained and rendered available for the useful purposes above alluded to.

"A gaseous body, which, under ordinary circumstances, will not combine with another substance, may be made to do so, if presented to it in the nascent state, that is, while it is separating from another solid or liquid body. Thus hydrogen gas, if formed in contact with sulphur, will not combine with it; but if the hydrogen be formed by the action of an acid on the sulphuret of iron, the sulphur is presented to the gas at the very moment at which the former is separating from the iron (in the nascent state, as it is called), and the gas, which is now disengaged, is a compound of sulphur and of hydrogen.

"It sometimes happens that the combination of two bodies is promoted by the presence of a third, which does not combine with either of the two, nor even with the resulting compound. Thus, if oxygen and hydrogen gases be mixed, they do not combine; but the contact of spongy platinum causes their immediate union, although the metal combines neither with oxygen nor with hydrogen, nor with water, the product of their combination.

"But in all cases where the third body has a powerful affinity for the resulting compound, its effect in promoting combination is very great, and has got the name of predisposing affinity. Thus, zinc does not decompose water; but if sulphuric acid be added, the water is decomposed, its oxygen uniting with the zinc. In this case, according to the usual explanation, the oxide of zinc formed unites with the acid, and the affinity of the acid for the oxide of zinc is called a predisposing affinity, as if the acid had caused the formation of oxide of zinc, because of the affinity of that oxide when formed. In truth, however, all the changes that occur are strictly simultaneous, and the phrase 'predisposing affinity,' is not an accurate one. It is, however, sufficiently convenient and expressive to be a good deal used in regard to similar cases.

"When two bodies, A and B, are in combination, the force with which they are actually held together is not dependent alone on their mutual affinity, but also on their relative mass. In the compound ABB, A is retained by a greater force than in AB. This is the result of the increased mass of B. Or we may view it thus:—In the compound AABB, the first may be removed with comparative facility, the relative masses of A and B being equal. But after the removal of the first A, the mass of B is relatively doubled, and the second A requires for its removal a much greater force. It is obvious that, conversely, in ABB, the second B is less forcibly retained than the B in AB.

"When a body A is presented to two bodies B and C, for each of which it has affinity, although unequal, then, if nothing interfere, A will divide itself between B and C, according to its affinity for each. But the effect of mass is seen here also; for if the mass of C, the body for which A has least affinity, be much larger than that of B, then the division will be no longer in the ratio of the affinities to A, but C will obtain more, and B less, of A.

"When to a compound body AB, another body C is added, having an affinity for

B, both combination and decomposition occur: for AB is decomposed, and at the same time B, which separates from A, enters into combination with C. It does not often happen that the mere force of affinity is sufficient to complete such a change, but such cases do occur: as where iron acts on a salt of copper, or copper on a salt of silver, the one metal taking the place of the other, and the latter being entirely separated. More frequently a part only of B is separated from A and combines with C; and thus there are present, at the same time, the compounds AB and BC, while part of A and of C exists in the free or uncombined state along with them.

"If now the free A be removed from the mixture, the free C, being unresisted, at last effects a complete separation of A. The removal of the free A is effected either when that body assumes the solid form by virtue of its great cohesion, or when it takes the form of gas in consequence of its feeble cohesive power, or of the application of heat. The precipitation of an insoluble oxide by a soluble alkali is an example of the former; the formation of glass by the fusion of silicic acid with carbonate of potash is an illustration of the latter case. Similar cases are of constant occurrence.

"In both cases, the decomposition is the more easy and complete, the larger the mass of the decomposing body C employed. And from this follows the curious fact, that if, in any given temperature, we can alter the form or the mass of either of the bodies which act on one another, the result of the experiment is likewise altered: nay, it may actually be reversed. Thus, if oxide of iron be exposed at a red heat to a current of hydrogen gas, the oxide is decomposed, its oxygen, with the hydrogen, forming water, while the iron is reduced to the metallic state. But if now the iron be heated red hot, and exposed to a current of the vapor of water, the water decomposed, its oxygen, with the iron, forming oxide of iron; while the hydrogen is reduced to the free state.

"Decomposition is rendered complete, not only when the body A, which is separated, assumes the solid form, but also when A remains liquid or dissolved, and the new body BC takes the solid form, or is insoluble. Hence the nature of the liquid, in which a chemical action goes on, exerts a most important influence on the result of the action.

"When two compound bodies, AB and CD, act on one another, both decomposition and combination occur. When complete, the change is called double decomposition, since both AB and CD are decomposed; but at the same time two new combinations AD and CB have been formed. Should the change be only partial, the four compounds AB, CD, AD and CB, will be present together. Double decomposition is of very frequent occurrence.

"In cases where a compound AB cannot be decomposed by a body C, even with the aid of a high temperature, the addition of a fourth body D, if it have an affinity for A, while there is an affinity between C and B, will often enable us to accomplish the decomposition. Thus, oxide of aluminum cannot be decomposed by charcoal even at a white heat: but when a current of chlorine gas is passed over the mixture, the chlorine by virtue of its affinity for aluminum, added to that of carbon for oxygen, effects the decomposition, and we obtain chloride of aluminum and oxide of carbon.

"When a compound of three or more elements is exposed to a high temperature, the elements unite to form such new compounds as are not decomposable by the temperature employed.

"If such a compound be heated along with a body which is capable of forming with some of the elements in certain proportions a more fixed compound, the remaining elements give rise to one or more new and more volatile compounds.

"The two last are the principal laws which regulate the phenomena of the destructive distillation of organic substances.

"Such are the most important facts in regard to the circumstances under which combination and decomposition occur. We now come to the subject of the proportions in which bodies combine together, or

"COMBINATION IN DEFINITE PROPORTIONS.

"The experience of chemists, derived from many thousand analyses, has established the following laws; which, however, are purely the expression of ascertained facts, and involve no hypothesis whatever:—

"1. The quantity, by weight, of the body B, which is taken up by a given weight of the body A, to produce the compound AB, is definite and unchangeable. Thus 8 grains of oxygen are invariably taken up by 1 grain of hydrogen (in round numbers) to produce water: or, in other words, 9 grains of water are invariably composed of 8 grains of oxygen and 1 of hydrogen.

"2. When a body A combines with a body B in more proportions than one, producing more than one distinct compound, the quantity, by weight, of B, which is united to the same weight of A in the different compounds, increases according to one of two ratios. According to one, the series of compounds formed is—

A + B,	1st compound	.. .. ..	B=1
A + BB,	2nd do.	.. .. ..	B=2
A + BBB,	3rd do.	.. .. ..	B=3
A + BBBB,	4th do.	.. .. ..	B=4
A + BBBBB,	5th do.	.. .. ..	B=5

"According to the other, the series of compounds is—

A + BBB,	1st compound	.. ..	A : B=1 : 3
A + BBBBB,	2nd do.	.. ..	A : B=1 : 5
A + BBBBBBB,	3rd do.	.. ..	A : B=1 : 7

"The compounds of nitrogen and oxygen offer an example of the first series of multiple proportions. In the first of these compounds :—

	14	grs. of nitrogen are united with	8	of oxygen.	
In the 2nd.	14	,, ,,	,,	16	,,
,, 3rd.	14	,, ,,	,,	24	,,
,, 4th.	14	,, ,,	,,	32	,,
,, 5th.	14	,, ,,	,,	40	,,

"The compounds of arsenic and oxygen will illustrate the second series. In the first of these compounds—

	75·4	grs. of arsenic are united with	24	of oxygen.	
In the 2nd.	75·4	,, ,,	,,	40	,,

"Here the quantities of oxygen increase in the ratio of 3 to 5.

"3. The proportions, by weight, in which bodies combine, are proportional to each other. That is: if a given weight of A unite with weights of B and C, which are to each other as 3 to 4, for example; then if a fourth body, D, unite with B and with C likewise, the weights of B and C combined with D will also be to each other as 3 to 4.

Thus, 100 grains of potassium unite with 20·41 grains of oxygen; 100 grains of potassium unite also with 41·06 grains of sulphur. Now 100 grains of silver unite with 7·39 grains of oxygen, and also with 14·88 grains of sulphur. According to this law, we find that 7·39 : 14·88 : : 20·41 : 41·06; or, in other words, that the weights of oxygen and sulphur which unite with 100 grains of silver bear to each other the same proportion as the weights of those elements which combine with 100 grains of potassium.

Another consequence of this law is, that the weights of two bodies which combine with the same weight of a third body, will also represent the weights of these two bodies which unite together, if they are capable of combination; or if the proportions should not be precisely the same, they will be found to be multiples or submultiples of those weights.

"Thus, 8 grains of oxygen combine with 1 grain of hydrogen to form water; and 8 grains of oxygen combine with 16 of sulphur to form hyposulphurous acid. Now, sulphur and hydrogen combine together to form hydrosulphuric acid; and that compound is found to contain sulphur and hydrogen in the proportion of 16 grains of the former to 1 grain of the latter. The same elements form another compound, the persulphuret of hydrogen; and in this the proportions are 32 of sulphur to 1 of hydrogen. Now $32 = 16 \times 2$.

"If, therefore, we know the weights of two bodies, B and C, which combine with a given weight of A, we also know the relative weights, in which, or in multiples or submultiples of which, B and C will combine together.

"Now oxygen is capable of uniting with all the other elements (save, perhaps, with fluorine); and, therefore, if we ascertain by experiment the weights of the different elements which combine with a given weight of oxygen, the weights thus obtained will inform us, not only in what proportions (or their multiples) these elements combine with oxygen, but also in what proportions (or their multiples) they combine with each other, provided they are capable of doing so.

"It is obviously of no importance what number we select to represent the standard weight of oxygen, to which the other elements are referred. On the Continent 100 is usually taken, and the combining numbers of the other elements are referred to oxygen as 100. In this country, for the sake of convenience in calculation, advantage has been taken of the fact that the combining weight or number of hydrogen is the smallest of all, and hydrogen has consequently been made the standard, with the number 1. Now as 1 of hydrogen is found to combine (in water) with 8 of oxygen, it is obvious that if hydrogen be represented by 1, oxygen will be represented by 8. Again, on the continental scale, oxygen being 100, hydrogen must be $\frac{100}{8} = 12{\cdot}5$, the proportion being preserved exactly the same. So that it is equally correct to say that water is composed of 100 grains of oxygen, combined with 12·5 of hydrogen, and to say that water consists of 8 grains of oxygen and 1 of hydrogen.

THE MEDICAL PROFESSION.—IMPORTANT MEETING AT EXETER HALL.

On the 27th ult., a meeting of general practitioners was held at Exeter Hall, for the purpose of taking some decisive measures with respect to the Royal College of Surgeons, and the charter recently granted to that institution.

Mr. Hunter took the chair. He adverted to a discussion which had taken place on a previous evening relative to the establishment of an independent college of surgeons, and urged the necessity of taking prompt and energetic measures.

After some discussion on that point, in which Mr. Ross, Mr. Rutt, Mr. Curtis, Mr. M'Cann, Dr. Jones, Dr. Lynch, Dr. Paris, and other members of the profession, took part.

Mr. Wakley, M.P., proposed the following resolution:—"That a petition be presented to the Crown, praying that Her Most Excellent Majesty will not sanction any alteration of the existing charters of the Royal College of Surgeons of England, until after the members of that College, amounting to upwards of ten thousand in number, shall have had an opportunity of stating their grivances and urging their corporate rights before Her Most Gracious Majesty's Hon. Privy Council."

Mr. T. H. Cooper seconded the resolution, which was carried unanimously.

A committee to carry out the object of the meeting was formed, and a vote of thanks was passed to the chairman.

TO CORRESPONDENTS.

"A Medical Practitioner."—Will our correspondent favor us with his address.

"N. P."—Yes; the subject is worthy of consideration.

"A Student."—We do not recollect; and have long since burnt such communications.

"M. D." (Carshalton).—We should be happy to communicate privately, if favored with the means of doing so.

"A Sussex Farmer."—Yes; with the addition of a considerable portion of animal charcoal.

"A Manufacturer."—Sulphate of soda.

"L. M."—Nitric acid diluted with 6 times its bulk of water.

"O. Simpson."—We have not received the communication respecting the P. S., but shall be glad to do so.

"Dr. R***."—The M.R.C.S. of whom you speak appears to us to be in fault.

"Inquirer."—The temperature of the solution must have been too high; it should not exceed 110° F.

"A Geologist" (Devon).—We have received the specimens, for which we thank you, and have placed them among our collection. We will examine a small portion of them and send you a report if you will favor us with your address.

SUMMARY OF METEOROLOGICAL OBSERVATIONS MADE AT OTLEY, YORKSHIRE, IN DECEMBER, 1844.

BY R. THOMPSON.

BAROMETER.

Monthly mean	29·93
Mean at 9 a.m.	29·93
,, at 3 p.m.	29·92
,, at 9 p.m.	29·94
Maximum on 20th	30·32
Minimum on 17th	29·26
Range	1·06

THERMOMETER.

Monthly mean	32°
Mean of maxima	35
,, minima	30
Maximum on 16th	39
Minimum on 7th	18
Range	21

Rain.—1·25 inches.

WIND.

Days		Days.
N. 0		S. 0
N.E. 9		S.W. 1
E. 10		W. 4
S.E. 1		N.W. 0

WEATHER.

	Days.		Days.
Clear	6	Rain	4
Cloudy	5	Misty	4
Overcast	11	Frost	19
Showers	0	Hoar frost	1

GENERAL REMARKS.

The close of this year has been remarkable for drought, several springs being dry, which, even in summer usually yield an abundant supply of water. The rain which has fallen during this month, chiefly fell on one day. Misty and frosty weather has prevailed. Inflammatory and febrile diseases have been common.

Nota Bene.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of the Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th; Advertisements on or before the 27th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE TENDENCY OF MATTER TO HARMONY.*

LECTURE DELIVERED BEFORE THE SOCIETY OF NATURAL HISTORY, IN UTRECHT.

BY G. J. MULDER.

(*Continued from page* 71.)

MAY it not be deemed presumptuous, if a man, initiated in the most beautiful of all sciences—astronomy, dare to differ in opinion from Laplace. I state mine with all the deference which the great superiority of Laplace demands.

It is, of course, understood, that we cannot speak here of any physical demonstration; all that we are capable of, is to adduce arguments which may give probability to our suppositions. From these it appears, that the haze was not originally incandescent; hence the question is still left unanswered: whence came the incandescence? Secondly, that from rings, formed on condensation of the fine ignited parts, portions could never have been produced, which united into one whole. Rings, when once formed, could, on contraction, crumble into pieces; but of pieces which belonged to one ring, a great number of little planets ought to have been formed, all of them revolving during the same period of time. For this reason alone, it seems to me, that this portion of Laplace's theory is not admissible.

It appears, moreover, from plausible reasons, that the portions existing in the centre of the haze were differently constituted from the rest. The sun, the fountain of the life of our planetary system, condensed from the particles of the centre, exhibits itself as a mass of a peculiar constitution, differing widely from the other bodies of our planetary system.

It seems probable that the substances of which the latter bodies—now planets and satellites—are formed, vary not with reference to their constitution, but only with regard to their mixture.†

Is not the assertion that all planets and satellites may consist of the same substances too presumptuous? Let us consider what observation shows, and we shall find that this assertion is derived, from a chemical theory of creation.*

It is well known, that in our planetary system, besides planets, satellites, and comets, another mass of matter revolves round the sun. Each day in the year, on an average, two foreign masses fall on our earth. Last year two such bodies fell near our town—masses which sprung from the universal space. These meteoric masses contain no traces of any other substances than those which occur on our earth. Agreeably to the probable opinion of my friend Van Rees, they are masses which revolve around the sun in the manner of small planets; masses enveloped in an atmosphere of vaporous substances, which, on arriving in our atmosphere, undergo combustion. Let us not forget, gentlemen, that the meteoric masses do not originate from our earth, and yet contain merely substances which are met with on our earth, endowed with the same properties, and of the same composition—nay, according to Rammelsberg's important observations, with the same arrangement of elements. If this, of itself, does not fully establish that all planets and satellites of our planetary system entirely consist of the same substances of our earth, the hypothesis is rendered highly probable by the circumstance, that in the space of the universe, totally beyond the regions of our globe, substances occur which are completely identical with those of which our earth is composed.

It forms no part of my present task to consider, in a mechanical point of view, the particular events which occurred at the period when the vaporous particles were condensed into celestial bodies. To this

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* Translated for THE CHEMIST, by ARTHUR D. WHITE, M.D.

† That their mixture is various, appears from the various densities of planets.

* [In the German, it is said that it is not derived, which does not agree with the context.]

only practical astronomers are entitled. May I be allowed to remark, that I think that the theory now pr pounded is based on that of the eminent Laplace; hence, to a certain extent, supported by his authority; and that the arguments in favor of his theory, which he derived from mechanics, hold good likewise with reference to mine: that, however, chemically acting causes are added, which latter appear to me totally indispensable, when we have to account for the grandest of the phenomena of nature which science can place before our imagination. The above statement is, I think, perfectly in accordance with the four principal properties of our planetary system: —motion of planets in the same direction, and nearly in the same plane; motion of the satellites in the same direction as the planets; rotatory motion of these bodies, as well as of the sun in the same direction as their progressive motions, and in planes which differ inconsiderably; slight eccentricity of the orbits of planets and satellites.*

After the bodies of our planetary system had risen on the condensation of the said haze, one portion of the latter was left unaltered. This yields perhaps the matter which produces the zodiacal light, and constitutes some of the celestial bodies, especially those which consist of a dense nucleus, with an expanded, vaporous, and often finely-divided matter behind them, revolving round the sun, viz., the comets. It is unknown by what cause these bodies are exempted from condensation into globes, a property common to all others. Perhaps their particles are too far distant from one another to exhibit under the influence of the sun any mutual chemical action. Perhaps they consist of such constituent parts of the former haze as admit of no condensation; or, perhaps, forces reside in them which, to be acted upon, require causes of another nature than those to which the matter of the planetary system was and now is exposed.

The comets, with their wonderful tails, likewise confirm this opinion, that the globes which revolve round our sun may have been produced from a finely-divided matter, and are, therefore, satisfactory reasons for this opinion in a subject like ours. Laplace considers them as the aggregates of matters which wander from one planetary system to the other, and therefore not exclusively belonging to ours. It is certain that the great eccentricity and inclination of the orbits of comets oblige us to consider the matters of which they consist, as being placed without the lenticular or disciform haze from which the planets and satellites originated. What are the phenomena which arose from the condensation—from the formation of the globes from vaporous particles? Was this altogether done in one moment, or slowly, or invariably in smaller portions?

Let us take observation for our basis, and see how we may proceed with probability.

It is impossible to permeate the interior of the earth; we must, therefore, confine ourselves to its outward crust. This crust exhibits in places, remote from each other, so many instances of rocky masses of the same composition, having risen from the depth, that we may, with some probability, form conclusions as to the nature of the substances of which the interior of our whole earth is composed.* It is especially the enormous mass of granite which seems to show that the nucleus of the earth was not gradually produced, but was suddenly constituted into a whole, and that from this nucleus the remaining particles of the haze were slowly precipitated and then condensed in small portions; we find a great many substances grouped in the crust of the earth, others aggregated in smaller portions, and others as a gaseous envelope surrounding our globe.

The idea of the condensation, whether it took place gradually or suddenly, is too important for us not to dwell on it a little longer.

The circumstance that the orbits of almost all planets are situated nearly in the same plane, strongly induces us to suppose that the said haze, at the time of its condensation into celestial bodies, was of a lenticular or disciform shape. Of this disciform mass, at the moment when activity was excited in its particles, as many particles were condensed into a nucleus for the formation of celestial bodies as there are planets and satellites. If on this occasion several small nuclei were produced, and these, by the difference in the periods of their revolutions, were united into one whole with the remaining surrounding vaporous particles, only after the lapse of centuries, many a point would remain entirely unaccountable; whereas, a great deal may instantly be explained, if we imagine the condensation to have taken place rapidly, though not in a single moment, yet neither in so gradual a manner as would have been the

* Laplace, *Exposition du Systéme du Monde*, Bruxelles, 1826, p. 541.

* The difference between the density of our earth (5·76) and that of the granite (3·06) is owing to the density of metals occurring in the exterior crust of our earth; metals are almost invariably heavier than granite.

case if produced by refrigeration, called into existence by a cause constituted beyond our planetary system, which first acted on the particles existing in the centre of the mass; a cause acting only on some, perhaps on very few, particles in the centre. The state of repose being once disturbed, even if only between two molecules of the enormous mass, this motion must be propagated throughout the whole with some rapidity, though not in the manner of two molecules of gunpowder, which, when disturbed in their repose communicate their movement with violence and immeasurable speed to great masses of gunpowder.

By this hypothesis—by which it is supposed that the effect was produced throughout the whole nebulous mass rather rapidly and with some violence—we may account for the small number of celestial bodies of our planetary system, masses of condensed matters, of as many nuclei of the principal condensation which directly followed, as soon as the activity was excited; and we may, moreover, account for the uniformity of the principal component parts of our earth, and for the occurrence of such enormous masses of compound minerals united together in the earth, masses of crude crystals which might have been thrown together without any systematic arrangement, in such a manner as to leave spaces to be filled afterwards by others. That the whole mass was not, however, condensed, at the time of the principal formation of the globes, is evidently proved by the fall of meteors, and by the isolated groups of mineral substances of the earth, which are decidedly of a later origin. That which was later condensed might have constituted a separate group on the earth, but could not produce substances universally distributed.

Let us now consider the globe which we inhabit in this respect, and, with this view, enumerate the elements of which, to our knowledge, our earth is composed, and from which it produced the bodies which, on condensation, formed our globe, with all substances, therefore, without any exception, which are found in, above, and near, the globe. In this development I will confine myself to that which is known in the present state of science, leaving at rest the question whether a great many of these supposed elements are not, indeed, compounds, which is very probable. The state of composition, however, is in no way connected with our present reflections.

There are fifty-eight elements met with in and above the earth. In the interior of the earth may exist a great many others, and on its surface may likewise be detected a small quantity of unknown elements. Conformably to the quantity in which they are found, and concluding from what we observe on the outer crust of the whole earth, they may be arranged in the following manner. This order must not, however, be rigorously criticised; whether the place assigned to each element be the proper one cannot be proved by facts established beyond contradiction. In the main, however, this order may be shown to answer.

Organic group, from which afterwards plants and animals were produced.—Carbon, hydrogen, nitrogen, and oxygen.

Elements of universal occurrence.—Silicon, aluminum, calcium, iron, sodium, potassium, magnesium, chlorine, sulphur, and phosphorus.

Elements of less frequent occurrence.—Lead, copper, zinc, tin, mercury, manganese, antimony, silver, gold, platinum, iodine, bismuth, barium, chromium, arsenic, nickel, cobalt, boron, bromine, fluorine, cadmium, and strontium.

Elements which seldom occur.—Palladium, rhodium, iridium, osmium, selenium, tellurium, tungsten, uranium, molybdenum, titanium, columbium, vanadium, zirconium, lithium, beryllium, yttrium, thorium, cerium, lantanum, didymum, terbium, and erbium.

At the period when these substances constituted the planet which we call our earth, everything that could be condensed was penetrated by gaseous bodies; oxygen, hydrogen, and nitrogen.

All oxidisable matters, therefore, must have been oxidised by the following elements at the time of their condensation:—Silicon, or the element occurring in sand, and rarely missing in rocky masses; iron, so largely diffused all over the earth, and above all calcium, potassium, sodium. This oxidation produced the enormous ignition, by which gold, platinum, and such other metals as cannot be directly oxidised, were fused and aggregated into lumps, in the manner in which they are still met with in the primitive rocks. Sulphur, which occurred in large quantities, combined with iron, copper, tin, lead, and antimony, and caused the ignition to augment; the phosphorus was oxidised, and the phosphoric acid produced by the powerful ignition, combined with the lime; the chlorine on being strongly calcined, united with several metals, but chiefly with sodium; the hydrogen suddenly united with the oxygen forming watery vapor, that fluid which now occurs in our planet, and in the atmosphere. If the constituent parts of water come in contact with a glowing spark, be it only of the size of the head of a pin, and the whole world filled with oxygen and hydrogen, they would, by the explosion, be

converted into water. Water was, therefore, suddenly produced from its elements, the moment the first traces of fire appeared, and the first chemical combination was brought about. The carbon, on combining with oxygen, formed carbonic acid, the nitrogen remained uncombined as gas, and these two gases, in conjunction with the aqueous vapor, constituted the atmosphere of that time.

The incandescent globe, formed of the 58 elements, and perhaps of some others, at first consisting only of fused substances, revolved round its axis, and the new sun in the same direction in which formerly the hazy particles wandered. The revolution round the axis flattened the half fused globe at the poles, and condensed that portion of it which is situated under the equator; a phenomenon which is presented to us in so highly conspicuous a manner by Jupiter, the greatest of the planets. The still remaining portions of the haze were condensed on the surface; here selenium, there tungsten, &c., all the elements which are largely diffused in the outer crust of the earth, owing to which this latter presents a greater variety of composition than the portions under it; this may be viewed as if several small planets or comets were successively condensed in various regions of our earth.

By the latter cause more lasting changes must have been induced in the ignited globe. Stony masses continue still to be thrown from the universal space on the earth, though this body be of long standing already; but this becomes less frequent in proportion to the decrease of the stone. At present, as meteors fall on the cold surface of the earth, they remain there as isolated lumps; formerly, when all was incandescent, nay, in a state of fusion, they must have formed the groups of minerals, smaller quantities of which the earth still contains.

The fused masses were soon brought into a state of equilibrium, and the surface, at first changed by glowing oceans, which were in some places precipitated over other substances, and in others mingled together, gradually acquired more solidity. The stone masses crystallised from the fiery streams and vogues, and though the main mass continued to be in a state of fusion, yet by the radiation of the glowing heat, the surface was rendered more solid. In this condition, being in a state of ignition, and constantly receiving from without new groups of substances, enveloped in an atmosphere containing all the water which now constitutes the oceans, a mixture of gases of its own kind, the earth might have revolved for thousands of years. The moment the most prominent parts of the surface of the earth were refrigerated by radiation, water was condensed in the atmosphere. Hence a new source for producing changes.

Water acts chemically upon several substances. There are bodies, which constituted on the then surface of the earth, had never yet been in contact with water, which, when first falling under its influence, must have produced a new series of violent phenomena.

The water condensed from the foggy and damp atmosphere, which covered the most prominent rocky masses, could not flow down without coming in contact with the incandescent matters below. The new phenomena must have been violent, though not equal to those which took place on the formation of the globe. We see in our days what a wonderful influence water exerts on coming in contact with the hot substances, which are still under the crust of the earth. We learn by volcanoes, the remaining fiery gulfs of the internally incandescent earth, how powerful an agent water is when it comes in contact with the incandescent substances existing therein. And though the water which penetrates through the fissures of the crust of the earth is not the sole and exclusive cause of the eruptions in our volcanoes, it greatly contributes to render these phenomena more powerful. Most of the volcanoes are in the vicinity of seas, and ejecting enormous masses of water, they show that the water which permeates the earth, on coming into contact with incandescent substances, must be ranked among the chief causes of volcanic phenomena.

Let us imagine that in ten thousand spots of the earth perhaps the same effect was simultaneously produced, at a time when the most culminating points of the surface of the earth were already refrigerated, and the remainder still in a state of ignition; that the water which was condensed from the atmosphere on the colder prominent spots, ran down into the deeper parts, there coming in contact with white hot masses, permeated the fissures with violent boiling, and the formation of steam at a high temperature, lifting up and overturning all that tended to preclude the free disengagement of the vapor, and we have an idea of the desolation, the prodigious force, and the nature of the revolutions which were then produced on our earth by water.

Another effect of the contact of the water with the ignited rocky masses, was to break the latter bodies asunder. In this manner thousands of rocky masses were cleft, broken, and divided into fine granular masses; in this manner, undoubtedly, it is that the sand which covered so considerable a portion of our globe was formed. This sand once con-

stituted part of great solid rocks, which were destroyed and converted into grains.

A process of dividing solid stony masses, well known in chemistry, consists in igniting them first, and then immersing them in water, by which means they burst and crumble into finely granulated pieces. Sand was undoubtedly produced by the division of the united rocky masses on coming into contact with the condensed water, and there is no doubt that this sand derives its origin from the skeleton of the earth, the granite, and its constituents: feldspar, quartz, and mud.

The refrigeration of the earth gradually gaining ground, rendered the water fit to remain on its surface. Condensed at the prominent rocks, it ran down, forming a number of inland seas; the sides of these were not strong enough to resist the pressure, or the amount of water rushing down from the heights was so great as to overflow, and falling into the lower regions, carried off whatever it met in its way.

The number of alterations that have been induced in this manner, and the stratifications of the constituent parts of the surface of the earth, cannot be discussed in a superficial sketch like the present. Bear in mind that, at one period, of the whole amount of water which now occurs on our earth, not one single drop existed thereon; that all must have remained in the atmosphere, being kept at a distance from the hot earth; that the whole was slowly and gradually precipitated from the atmosphere, flowing down from the heights into the lower regions; that, therefore, the whole surface of the earth, though even then but gradually and in various places, was covered with water; that, finally, this current, after having caused several desolations and changes, occupied the lowest regions of the earth; and it cannot be overlooked, that the alterations produced by water on the surface of our earth, are to be considered as very important ones.

To the said effects of water others joined. The water, on being condensed from the atmosphere, and on coming in contact with various kinds of stones, dissolved the soluble salts, and formed salt-water. This salt-water is now met with in our ocean. The salt-water of such seas as were enclosed all round, on evaporating, left behind it the salt which it separated, by solution, from the stony masses, forming the various saline stratifications which are still found in some places under the surface of our earth.

The continued flow of the water from the higher and colder portions into the lower ones was attended with another equally important consequence. Wherever, for instance, in penetrating into the bowels of the earth, it met with hot masses, it was converted into vapor; and if the formation of the vapor took place *under* the surface, the portions covering it must have been raised. Elevations of the kind were, indeed, in several places, produced. In this manner very high mountains rose from level grounds; or, even from the midst of the water, still presenting traces which evidently prove that they were formerly covered by water.

You see, gentlemen, that during a very long time, in which part of the said changes occured, life could not subsist on our earth. Ignited masses, and oceans of boiling water, render life impossible. Only when a portion of the crust of our earth was cooled, the first vestiges of collected matters of an individual existence, which were developed, lived, and died; in one word, what we call organic beings, became possible, and could exist. Of what were these formed? Of those portions, gentlemen, of the haze which, under violent phenomena, were formerly united with the ignited globe, or glowing atmosphere around it. Recognise in this the origin of *all* matters which belong to our earth, of which, plants, animals, and our own bodies consist.

Now, when we find all around us ready made; when we consider any rather strong movement of the atmosphere as a violent one, when a trifling rain is taken for a more or less considerable deviation, which occupies our mind, is the topic of our conversation and cares; when we measure everything that acts in nature, and may be seen; the delicacy of the life of our body, the care for its maintenance, the weakness of our physical powers; it must cost us an effort to remount back towards the period at which all was violent, apparently desolate, rolling without order; it requires an effort to imagine that each particle of the matter of our body, and, therefore, all of which it is composed participated for thousand of years in the grand changes of form. It is, however, nevertheless true—incontrovertibly true—that no object, no substance, which has ever met our eyes—no constituent part of the very eye: in one word, NOTHING which is made of matter, and known to us, was beyond the reach of this influence.

In our times, the custom still prevails of ascribing the phenomena of such individual beings as are called into existence and disappear again, manifesting certain functions, the phenomena, therefore, of living beings, to peculiar forces. No notion seems to be less founded on the principles of physical science than this. I speak of bodies, and

not of spirits. From matter *that* emanates, which has been communicated to it.

The moment the first traces of beings which we call *living* ones—plants and animals—were produced on our earth, a peculiar force is imagined to have been imparted to them, which set everything in motion, owing to which, not only all originates, but likewise grows, is developed, and maintained in its integrity, and on the decrease or annihilation of which disease and death follow. And this is called the *vital force*. I most willingly bow to the opinion of others; but my own, on this point is, that this force explains nothing, and is a word foreign to science.

I cannot imagine any phenomena manifested by matter which could not be derived from the principal causes of the action originally communicated to it by the Deity, communicated in conformity with defined laws. And when I see that thousands of the principal forces unite, for a common purpose, in one organic whole, this union appears to me as the prototype of the tendency to all changes of form to which matter was exposed. In this sense I will briefly speak to the relation between the living world and the haze mentioned above, and the glowing earth produced from it.

The first living things on our earth were undoubtedly mould and infusoria, which were followed by ferns, enormous ferntrees, radiata, mollusca, vertebrata in great numbers, subsequently by fishes and reptiles, afterwards by collossal mammalia, and afterwards by smaller ones, and by birds in the atmosphere.

In proportion as the waters began to occupy the lowest portions of the surface of the earth, which in its turn depended on the increasing refrigeration of the whole, the temperature became more regular, the atmosphere was of a more permanent mixture, and the condition of a portion of the surface more settled. Seasons commenced; the overflowing *inland seas* were no more, the manifold elevations of mountains ceased, and that which took rise with the progressive development of living beings was no more destroyed, as it were, at one blow.

It is impossible for me to give you a sketch of the state of the earth in the different periods which preceded; to show it to you in a state, in which it presented traces scantily scattered of water-plants, or animalculæ imperceptible to the naked eye; to represent to you its surface as a receptacle of seas and marshes, in which latter our coals originated from plants then living; to depict to you the appearance of the earth, when fishes and shell-fishes abounded in the boundless seas, mountains rose from the waters, being soon inhabited by colossal reptiles. It is impossible for me to show you now how the animals were buried by the overflowing seas; how other spots of the earth drying up yielded other forms of organic beings, and how a commencement having been made with infusoria and mould, after many thousands of years, finally plants and animals resembling those of our own days, were produced from the originally incandescent lump. I will point out merely a few peculiarities from the store of knowledge, which with reference to the earth of old has been delivered to us by geologists, peculiarities having an important bearing on our purpose.

It is established that the first organic beings were of the simplest form; infusoria, which, according to the observation of Ehrenburg, so nearly approached unorganised bodies, that it may be asked whether they were not unorganised, in the same way as of some living beings it is still doubtful whether they are animals or plants; for instance the gallionella ferruginea and the gallionella distans; 187 millions of these animalculæ weigh one grain, or as much as one drop of water.

The first organic beings were the most simple ones, and scarcely differed from inorganic bodies. More compound ones followed slowly. Mollusca lived before fishes were, and for many centuries perhaps they were the highest animals in our planet. Fishes in their turn preceded reptiles, and lived in seas, in conjunction with mollusca. Reptiles again were, during a series of centuries, the highest kind of animals. They were followed by mammalia and birds. Of the older forms, part was preserved, but the greatest portion was destroyed, and of the matter yielded by them new forms were developed; forms which were in so close a relation with external circumstances, that an intimate reciprocity cannot be overlooked.

In this manner, for instance, after more or less permanent ridges of mountains had risen from beneath the water, immediately on them, crawling animals appeared, able to inhabit both water and land; whereas, when the land was brought to a more settled state of dryness, reptiles appeared which could only live on the dry soil and not in water.

In all these and other respects, which want of time prevents me from mentioning, we see the development of the matter of organic beings, the forms of which were determined by circumstances.

The matter, therefore, which was glowing before, yielded organised beings, endowed with forces, traces of which were absent in matter regarded as such beings, the conse-

cutive generations of which have inhabited the earth for thousands of years: the services, therefore, rendered by matter, were of a higher order than simply to put itself in equilibrio. The beings possessed higher powers to a greater or less extent; which, as I feel convinced—differing in that point from the respected speaker preceding me—cannot be considered as having sprung from matter. I call animals *beings*, and see in them sparks of the mind which animates us, in the same manner as our mind is possessed of sparks proper to the Deity. Matter, therefore, had to furnish the means for fixing those spirits on a planet; but it had to perform one more duty. By the harmony of the constituent parts of our earth it became adequate to give room to beings of a higher order; the forces originally present in the haze were modified in a thousand ways, and the whole was, as far as possible, reduced to a state of equilibrium. A matter of this nature only could serve as the residence of an intelligent mind, and having employed one portion of the surface of the earth, as lime and phosphoric acid, magnesia and sea-salt; one portion of the atmosphere: carbon, hydrogen, oxygen, nitrogen, the human body appeared, from the fiery mass which we have imagined in the foregoing, from the haze of which this mass was formed, of the matters which, formerly crude and chaotic, after thousands of changes of forms and combinations, finally entered into perfect harmony. The substances of which alone the *human* body could consist were brought into the state proper to abide, contained a higher spirit than the animal and vegetable genera which preceded it, that it to say, they were united in a state of most complete harmony. In this mass of matter, in which a mind endowed with reason resides, not only has each organ a form and constituent parts of its own, but each elementary particle is of a shape peculiar to itself. In the 800 millions of human beings who now inhabit the earth, there exists not a single one of the millions of blood globules, running through the vessels of each individual, which is not appropriately shaped and measured, and invariably of the same form. All the exterior forms of animals and plants are now positively defined, and every microscopic particle which occurred in one species of animals or plants, is formed with regularity, the first principles of which were already laid down in the haze, the igneous globe. What we now see is the result of change of matter, of the preceding combination of matter; it is the task of natural history to discover the laws, according to which the most beautiful harmony arose out of chaos; it is natural history that must point out the laws by which the Deity produced matter—what were the forces appropriated to it, and according to what laws these forces are to act. I therefore call this science the divine one.

You see, now, gentlemen, the assertion which I stated in the commencement rendered clear in few words, that mind and matter, though different in many points, have yet that in common, that they both strive after harmony. I compare the mind of an anthropophagous savage, as contrasted with that of the noble and well cultivated man with the matter of the former haze, of which our planetary system sprung, relative to that of which the human body is now composed. In the spirit and raw matter the rudiments have been laid down for the development of something higher, of something in which harmony is to predominate, to the same extent as roughness and desolation were prevalent in the original state. The original forces are to be modified, modified in thousands of ways, if harmony in matter as well as in spirit are to be established. This is effected by few and invariably fixed laws, arranged by the Deity and continually brought into execution; there is neither chance nor whim.

If I may consider it as proved that the forces of matter, like those of the mind, are capable of harmonious development, then, according to this order of things, we may expect something more, which, compared with what now exists, may be called *better;* and see it advance step by step until it arrives at its limits.

Let us throw one glance more on our planet. When it was crude and desolate, changes were going on until matters and forces were brought into a state of equilibrium; small beings arose from it; the infusoria prepared the matter and form for the mollusca, these animals for the fishes, these for the reptiles; the reptiles for the mammalia, and the latter, in their turn, for the birds. These beings, supported by plants, were required to render the matter of the earth fit to contain a higher spirit. Will it always be fit for that? It has contained it for longer than six or seven thousand years. The field of knowledge increases—the development of the mind is enlarged. It is not seen to have progressed in each century, but on looking back to history this is clearly demonstrated. The spirit cannot come to a standstill. If, the mind is invariably advancing, the time must come in which it will want a body different from the present. Then it seems to me the moment will have arrived in which

the earth is to undergo new changes of form, and human kind will vanish away in the same manner as minds, which perished before our own eyes, because they were too energetic to remain in a body. I name the mind of our *Vorszelmann de Heer*.

What may bring about this change of form? Who knows that, gentlemen? But, in all probability, not the forces of the matters of our earth; these are, then, perfectly satisfied. A new sun of our planetary system, or a second in the vicinity of the present, may, perhaps, suffice to change all the laws of affinity of the bodies of the earth, and to overturn our whole planetary system. Thus a new order of things may be produced, somewhat higher than the present, and so may everything in the universe which is made of matter continue to be changed according to the laws once established, changed by the action of matters on matters, solely produced by the change of difference.

The life of one man is a miniature of that of human kind; the history of the earth a miniature of that of the universe; the particular circumstances alone vary; their type is the same. Millions of changes of form, perhaps, the millions of worlds are to undergo, before the material world is to be considered as a perfect one. Perfection, however, has its limits. One day all forces and matters may be satisfied and in perfect harmony, and the mind developed in the same ratio. It seems to me that when the millions of globes of worlds have once satisfied the object of their existence by the development of spirits over matter, that then the time will have arrived in which all matter will sink back into annihilation, shrink into nothing, from which she was produced, to allow minds freely to subsist, independent and undisturbed, to be developed by themselves into infinity, according to laws which the Deity has laid down and organised for eternity, and maintains from moment to moment.

ON THE DIFFERENT STATES OF ARSENIOUS ACID, AND ON THE VITREOUS FORM IN GENERAL.*

BY M. BRAME.

THE author thus sums up the investigations which form the subject of his memoir:—

1. The devitrification of arsenious acid results from the tendency of this body to crystalise.

2. In devitrification by time, it appears that there is at first a formation of definite and transparent crystals, and that the latter, as we have seen in previous examples, are converted into smaller crystals, which aggregate, notwithstanding the dilatation which is produced under the influence of the resistance opposed by the still vitreous layers. Heat and the passing contact of solvents break this resistance, and then the small crystals become distinct.

3. In devitrification by heat, favored, doubtless, by the volatility of arsenious acid (at 260° F. to 290° F.) crystals are most commonly formed, as well in the interior as on the exterior of the devitrified fragments; nevertheless, if the temperature do not exceed 260° F., or if higher only for a short time, not a trace of crystals will be found, and the opaque zones, which are formed in the fragments, will have exactly the appearance of those produced by the action of time. Here also, heat, determining dilatation, puts the molecules in motion; the solvents which provoke it act in the same manner.

4. With respect to the causes which produce the zones themselves, it is perhaps attributable to successive deposits of arsenious acid being added to one another in the manufacturing apparatus; perhaps, also, it is owing to successive deposits of pulverulent arsenious acid being fused in the place in which they are formed, which I have been able to prove in a mass of the commercial article. It should here be remarked that vitreous arsenious acid appears to be divided more easily in the direction of the zones than in other directions; now, in all cases, at the points of contact of the superposed deposits, the tenacity should be greater, and, consequently, the resistance of the forces which induce crystallisation, more energetic. Be it as it may, it is remarkable that the devitrification takes place in zones when this devitrification may be attributed to the tendency to crystallisation.

5. The difference of density of vitreous arsenious acid, and of that which has become opaque, is sufficiently explained by the crystallisation; besides, the cracking noise heard during devitrification indicates the forcible separation of the vitreous laminæ by the crystals formed.

6. The difference of solubility, carefully studied by M. Guibourt, will be explained, I think; but my experiments on this point not being completed, I will not speak of them.

7. With respect to the peculiar state which arsenious acid assumes when deposited from a concentrated solution in hydrochloric acid, we see in it a tendency which the crystals of arsenious acid present to be disaggre-

* *Comptes Rendus.*

gated, in order to be afterwards united in a continuous layer: it is therefore an effect which seems to be an inverse of the foregoing; but, as I have already said, there are, perhaps, in this case, peculiar phenomena, which I intend to study with care.

When I had ascertained how easy it was to devitrify arsenious acid by means of a moderate heat, which produced the same effects as time itself, I tried analogous experiments on a certain number of transparent vitreous or crystallised bodies.

Boracic acid heated to near the temperature at which it fuses, then placed in a sealed glass tube, became opaque in a few days.

Fused borax, slightly opaque on the surface, was covered with true crystals, at a temperature below its point of fusion.

At the temperature of 212° F., maintained for three-quarters of an hour, prismatic needles of sulphur, which had just been obtained by fusion, and which were transparent, became as opaque as by the action of time, forming in the interior small brilliant crystals.

Recent barley-sugar was half fused at the temperature of 212° F., and by cooling it gave a hard, whitish mass, which afterwards softened, and which presented a great number of small crystals, internally and externally

Old barley-sugar, but with a vitreous fracture, by no means crystalline, became opaque in a few minutes, at the same temperature. It was scarcely softened on the surface.

The crystals of white sugar-candy are varnished on the surface in the same circumstances, but they offer much more resistance than barley-sugar.

Is it necessary to speak of glass, which, as is known, is devitrified by time, or by the prolonged action of a heat little inferior to that which is required for its fusion, or by repeated fusions and solidifications, or, again, by prolonged fusion and slow cooling; but here, as in the foregoing cases, new experiments and new observations are necessary for completely elucidating the question.

Among the crystals which undergo devitrifaction by time, I may also mention bichloride of mercury. Here, therefore, is a list of a certain number of substances which are devitrified by time or by heat; I will shortly collect all materials necessary for continuing this work, that is to say, a good number of nitreous substances and anhydrous crystals of a certain size; it will probably be ascertained that the cause of devitrifaction is unique; it should be attributed to the tendency to crystallisation—crystallisation of the vitreous body, and transformation of anhydrous crystals into smaller crystals.

But is this the whole of the phenomenon? Some years ago M. Dumas showed me in his lectures that all vitreous bodies retain heat, which they abandon during devitrifaction.

In the same idea, I have just made a very great number of experiments on some vitreous bodies, in order to ascertain whether heat is disengaged during devitrifaction on their crystallisation at a given temperature.

I think I may announce that in several cases I have been enabled to appreciate a considerable elevation of temperature at the moment of transformation. Arsenious acid appeared to present the phenomenon very distinctly.

In conclusion, I beg permission to make rapprochement, which seems to result from what I have just said.

1. Vitreous arsenious acid is devitrified by the action of a moderate temperature, and during this devitrifaction heat appears to be produced.

2. Dissolved in hydrochloric acid, vitreous arsenious becomes luminous during crystallisation, and it produces opaque crystals. Now, according to my observations, it appears probable that the opaque crystals which are small tetrahedrons, are formed by the redoubling of transparent octahedrons. I have not been able in this case to prove the quantity of heat produced by crystalisation; but it is known that, by the contact of ammonia, the vitreous acid is heated a little, acquiring all the properties of the opaque acid; moreover, I have ascertained that it crystalises, and I have seen that dilate hydrochloric acid renders vitreous acid opaque, whilst it causes the opaque acid to crystalise almost instantly—the relation is evident. In all cases there is a production of light during the crystalisation in hydrochloric acid.

Be it as it may, at the moment of the devitrifaction by heat, there is a separation of the molecules, an ebranchment, as by a shock, which produces analogous phenomena.

It is thus that soft iron, submitted to repeated shocks, crystalises and becomes brittle; it is, like barley-sugar, crystaline sometimes suddenly under the influence of a shock or even of a slight motion; it is thus that ordinary sugar, which is broken, becomes luminous in the dark; finally, it is thus that by agitation, the crystals of arsenious acid, deposited from the solution in hydrochloric acid, may give light by agitation, when in a state of repose they do not produce it.

But the bodies which are devitrified, that is to say slowly crystalised, do not, perhaps, produce appreciable heat, although able, perhaps, to produce perceptible light. When heat determines a rapid devitrifaction when there is a crystalisation of a vitreous body, or a transformation of large transparent crystals into smaller crystals, the same as in the crystalisation of a soft body, there may be a disengagement of light or heat, and, in certain cases, perhaps of both.

M. Dumas, in presenting this memoir, remarked that the observations of Brame concerning the rapid devitrifaction of arsenious acid having the effect of furnishing it in rubanées masses, which present the greatest analogy to natural rubanées agates, whose formation is still one of the mysteries of geology.

The observation of M. Brame appears to M. Dumas well worth, in this respect, the attention of geologists.

EXPERIMENTS TO DETERMINE THE QUANTITY OF THE NOURISHMENT PARTAKEN FIXED IN THE BODIES OF ANIMALS.*

BY DR. F. SACC.

EXPERIMENTS WITH CHICKENS, FED WITH BARLEY.

Previous to communicating the results of my experiment, it may be advantageous to give some details concerning the manner and ways in which it was executed, and concerning the circumstances attending it, to enable each of our readers to repeat it, to convince himself of its correctness, and the degree of exactitude which may be arrived at in such experiments.

On the 13th of November, 1843, I obtained a cock and a hen, of English breed. They were hatched in May, in the same year. Both were stout, in perfect good health, and very tame. The cock was of a fine red yellow, the hen was of a brilliant white. On their arrival, they were put into a very capacious wire cage, the bottom being also of wire, resting on a case of zinc, which was to receive all that the chickens let fall. They received their food in zinc boxes, which were carefully weighed; one of which contained the food proper, another very pure, well washed, and sifted quarry sand, a third pounded chalk.

The boxes were provided with funnel-shaped covers, to prevent the chickens from throwing out of them either with their beaks or with their feet. The water was given to them in a porcelain vessel, and every day renewed; of other substances they obtained a fresh supply only once a week. The water employed was that of the common springs, and contained salts of lime, which however were not determined, because, owing to the changes which it was to undergo, the quantity consumed of it by the chickens could not be ascertained. This determination, which might have turned out useful, was, however, not indispensably necessary, because chickens generally drink but little.

During some days my chickens received alternately wheat, oats, potatoes, and barley, but from the 21st of November, 1843, I also gave them water, sand, chalk, and only barley. They were not permitted to leave their cage. I wished to begin the experiment at that time, but was unable to do so.

On the 6th of January, 1844, in the evening, the hen became indisposed, she deposited a large quantity of very liquid, and fetid excrements, consisting merely of the husks of the grains, united by a viscid colorless matter. These were of an alkaline reaction, whereas the brown portion of the normal excrements was neutral, and the white portion, which consisted of uric acid and urates, was perceptibly acid. She, however, continued to eat as usual.

On the 13th of January, at half-past nine, p. m., the hen deposited a large egg, enveloped only in a membrane, being destitute of its shell.

On the 14th of January, finally seeing that the chickens were doing well, I began the experiment. I determined the quantity of barley which they consumed, and its ratio to the excrements deposited. The loss was to show, how much was fixed in the body, burned by respiration, or secreted through the skin.

With this view the trays containing the food were removed the preceding day at nine, p.m. The 14th, at six, a.m., I gave them—

Sand.....	689·868	grammes.
Chalk.....	89·228	,,
Oats.....	600·961	,,

The sand was dried at a red heat; the weight of the chalk was calculated by a sample which had been dried at 212° F., the same took place relative to the barley. The same proceedings were adopted throughout the whole experiment.

The hen weighed.. 266·160 grammes.
The cock weighed.. 772·220 ,,
The hen no longer deposited any tenacious.

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* Liebig's *Annalen der Chemie*, October, 1844.

excrements, nevertheless her abdomen continued to be inflated.

On the 17th of January, in the evening, the hen seemed to be unwell, and again during the night laid an egg without a shell, which weighed 22·660 grammes. And after having been dried at 212° F., weighed 7·897 grammes.

During the week of the experiment, the chickens seemed to be in good spirits and a state of good health. On the 21st of January, at six, a.m., I terminated the experiment.

There remained—

Barley	135·712	grammes.
Sand.....	584·350	,,
Chalk.....	81·856	,,
The hen weighed..	604·175	,,
The cock weighed..	790·725	,,

During the whole time of the experiment the excrements were collected with the greatest care, each day, and were at the same hour removed from the zinc vessels with a platinum knife, care having been taken not to leave behind any trace of them, and dried at 212° F., until their weight was no more diminished in a dry current of air.

The whole weight amounted to 2,290,707 grammes.

From these data it appears that the cock and the hen consumed in one week—

Barley	465·249	grammes.
Sand.....	105·515	,,
Chalk	7·372	,,
The weight of the cock was augmented by	18·505	,,
The weight of the hen was diminished by	21·985	,,
But taking into consideration the weight of the egg	22·660	
The weight of the hen was indeed augmented by	0·675	,,

From this, we learn that the power of assimilation was less in the hen than in the cock, which must evidently be attributed to the state of suffering in which she was when depositing the eggs.

Having stated the rough numbers of the experiment, I pass on to the results of the chemical analysis of the food as well as of the excrements.

In the state in which the barley was given to the fowls, it contained 13·370 per cent water. On incineration 100 parts of the dry barley contained on the average, 3·3259 parts of ashes. On analysis this gave 0·0028 of sulphur, which was manifestly present in the form of sulphates. On combustion of the dry grains of barley with nitric acid and nitre, as a mean, were obtained,—

In 100 parts.....	0·1409 parts sulphur.
Deducting the amount of sulphur from the ashes..	0·0028
There remains	0.1381

as the amount of sulphur of the organic constituents of the grains.

The determination of the sulphur of the barley was necessary for ascertaining whether on the determination of the carbonic acid, the amount of sulphur was to be taken into consideration.

It, however, contains so little, that it may safely be entirely neglected.

The contents of the ashes having been determined, the analysis of the barley was attended with no further difficulties.

Two combustions of the barley (freed from ashes) gave the following results:

	I.	II.
Carbon.....	45·817	46·887
Hydrogen....	6·453	6·598
Nitrogen	2·281	2·281
Oxygen	45·449	44·239
	100·000	100·000

The difference of the quantity of carbon in the two analysis, which amounts to one per cent, must at first seem surprising. Considering, however, that in the second analysis the amount of hydrogen was likewise considerably augmented, it will be found that these irregularities do not exceed the limits of common errors of calculation, which may occur in analysing substances like barley, which contain variable quantities of ashes. The deviation relative to the carbon is, therefore, chiefly to be attributed to the circumstance, that larger grains were employed in the second analysis than in the first, which decidedly contained less ashes.

From the above stated analytical results it appears, that the barley grown in 1843, on a soil of basalt, in the neighbourhood of Giessen, was of the following composition:

Carbon.....	45·4690
Hydrogen....	6·4815
Nitrogen	2·2810
Oxyen	42·4435
Ashes	3·3250
	100·0000

The excrements dried at 212° F. in a dry current of air weighed 229·4690 grammes, 100 parts of these excrements, on an average, left behind 22·9821 parts of ashes, which contained 0·000013 parts of sulphur.

Treated by nitric acid and nitre, the excrements gave 0·000008 grammes sulphur, consequently only little less than was contained in the ashes themselves. These al-

most imperceptibly small quantities of sulphur being insufficient to interfere with the correctness of the analysis, were neglected as in the case of the barley.

To the ashes of the excrements must be added the sand mixed with them, which having been separated by mechanical means was found to be 68·9676 grammes.

These excrements contained, therefore:

Organic matters ..	46·9104
Ashes and sand ..	53·0896
	100·0000

The source of error which arose from the variable amount of ashes in the barley occurred also in the excrements, for not only did they not always contain the same amount of inorganic constituent parts derived from the barley, but they contained, likewise, small pieces of silex, from which, despite all care and trouble, the barley could not be entirely freed. Another cause of error was found in the unequal distribution of uric acid and its salts on the surface of the excrements. To these two sources I attribute the variations met with in the analyses most carefully instituted by myself.

The determination of nitrogen gave in two experiments:

I.	II.	III.
3·980	4·121	4·050 per cent.

The carbon I determined with chromate of lead. In all the experiments the excrements employed were in a perfectly dry state.

The following table contains the results of my analysis; in this the ashes were subtracted.

	I.	II.	III.
Carbon	49·066....	46·820....	44·796
Hydrogen ..	6·622....	6·252....	5·908
Nitrogen ..	4·050....	4·050....	4·050
Oxygen	40·262....	42·869....	45·246
	100·000	100·000	100·000

Average.

Carbon	46·8970
Hydrogen.....	6·2607
Nitrogen	4·0500
Oxygen.....	42·7923
	100·0000

100 parts of excrements dried at 212° F., therefore, contained:

Carbon	21·9996
Hydrogen....	2·9369
Nitrogen	1·8999
Oxygen.....	20·0740
Ashes and Sand	53·0896
	100·0000

After having premised these details of numbers I pass on to the inferences to be drawn from them.

The fowls consumed 465·2490 grammes, containing:

Carbon	211·5440 grammes.
Hydrogen ..	30·1551 ,,
Nitrogen	10·6123 ,,
Oxygen	197·4680 ,,
Ashes	15·4695 ,,
	465·2489 grammes.

They, moreover, partook of:

Sand	105·5150
Chalk	7·3720
	112·8870

On the other hand, they deposited excrements amounting to 229·7070 grammes which contained:

Organic matters.....	107·4579
Inorganic matters	121·6128
	229·0707

The organic constituent parts of the excrements contained:

Carbon	50·3946
Hydrogen.....	6·7275
Nitrogen	4·3521
Oxygen.....	45·9863
	107·4579

The inorganic constituent parts contained:

Ashes....	52·6452	121·6128
Sand	63·9676	
		229·0706

From this, it appears that the fowls absorbed:

Carbon	161·1494 grammes
Hydrogen	23·4276 ,,
Nitrogen.....	6·2602 ,,
Oxygen	151·4844 ,,
Ashes and Sand ..	6·7437 ,,
	349·0653 grammes

The increase in the weight of the cock and the hen (inclusive of the egg) amounted to	19·1800
Subtracting the fixed inorganic constituent parts	6·7437
There remains	12·4363

by which the weight of the bodies of the chicken has been augmented.

In regarding these numbers we are surprised to find that the chickens assimilated far greater quantity of carbon and oxygen than hydrogen and nitrogen; more than one third of the nitrogen received passed into the excrements.

This fact was to be anticipated from the great quantity of uric acid and urate of am-

monia which they contained: it is, however, nevertheless interesting and important to ascertain whether in sarcophagous birds the nitrogen received with the food stands in the same ratio to the nitrogen of the excrements.

From the preceding experiments it appears that the chickens, having assimilated 342·3216 grammes of organic matters, derived from the barley, their weight increased by 12.4363 grammes. The whole of the remainder was employed for the maintenance of respiration and disengaged by that function. I regret not having been able directly to determine the quantity of carbonic acid and water which was produced by the birds during the experiment. Determinations of this kind would have yielded still more certain data.

The experiments communicated above show that two chickens, weighing 1398·380 grammes, consumed each day, 66·464 grammes of barley, 15·073 grammes of sand, and 1·053 grammes of chalk, which quantity expressed in per cents., relative to the weight of each chicken, answers to 4·758 barley, 1·078 sand, and 0·075 chalk, in the whole to 5·906.

The excrements daily rejected weighed 32·724 grammes, which, reduced to per cents. relative to the weight of each fowl, answers to 1·097 organic matter, and 1·243 inorganic matter, in the whole to 3·340.

The chicken, therefore, on being fed with barley, rejected with the excrement more than one half of the matters consumed; namely, one third of the organic substances, and about the whole amount of inorganic matter.

These experiments form the first part of a series of experiments, which I am about to continue, with the view of ascertaining which constituent parts of one and the same nourishing substance are fixed in the bodies of various vertebrata, and which are the products of decomposition, which, under the influence of digestion, are produced from them.

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ON THE LIQUEFACTION OF GASES.*

LETTER FROM PROFESSOR FARADAY TO M. DUMAS.

I THANK you for the interest which you take in my experiments. Although they have occupied me for six months, they may be very briefly detailed. You remember the experiments of M. Aimé, (*Annales de Chimie et de Physique*, 3rd serie, t. viii. p. 357) who disengaged various gases at the bottom of the sea, thus being submitted to a high pressure. The results obtained could not be observed in the state of compression, and they were obtained, moreover, at the ordinary temperature. You will also remember the experiments of M. Cagniard Latour on ether, by which he proved that, at a certain temperature, liquids are converted into gases.

Now if this point of liquefaction is, as it appears to be, the lowest possible with the lightest and most volatile bodies, existing as gas, there is little or no hope of liquefying substances like hydrogen, oxygen, or nitrogen at any pressure, so long as they are maintained at ordinary temperatures, for their point of liquefaction is probably below the ordinary temperature, or even a considerably diminished temperature.

You have, therefore, the key to my mode of experimenting.

I endeavored, at first, to obtain a very low temperature, and for that purpose I employed M. Thilorier's bath of solid carbonic acid and ether; but, in using it, I placed it under the receiver of the pneumatic machine. By maintaining a constant vacuum, I lowered the temperature to such a degree, that the carbonic acid of the bath was not more volatile than water at the temperature of 86° F., for the barometer of the pneumatic machine was at 28·2 inches, the external barometer being at 29·4.

This arrangement being completed, I adjusted together, by means of cocks and stop-cocks, small glass and copper tubes, in such a manner that, with the aid of two pumps, I could force and compress different gases to a pressure of 40 atmospheres, and at the same time submit them to the intense cold obtained under the pneumatic machine, and to examine the effects. As I expected the cold produced several results which pressure alone would never have been able to give, and principally in the solidification of bodies ordinarily in the gaseous state. I lay before you these different results.

You will remember the gases which have already been condensed, and you have, I believe, added arseniuretted hydrogen; it is probable, besides, that M. Aimé condensed olefiant gas and fluosilicic acid. The following are my last results:—

Olefiant gas condenses into a fine, clear, colorless, transparent liquid, but it does not solidify; it dissolves the resinous bituminous oily bodies.

Pure hydriodic acid may be obtained in

* *Annales de Chimie et de Phisique*, Jan. 1845.

either the solid or liquid state. Solid hydriodic acid is very clear, colorless, and transparent, generally presenting a few fissures, which traverse the mass; it greatly resembles ice.

Hydrobromic acid may also be obtained either as a limpid and colorless liquor, or as a clear and transparent solid body.

These two acids require very careful distillation, in close vessels and under high pressure, to be obtained pure and colorless.

Fluosilicic acid has been condensed to the liquid state, but it is necessary to operate at the lowest temperature. It is extremely liquid, and as mobile as hot ether. It then produces a pressure of about 9 atmospheres, and does not give any sign of solidification. It is transparent and colorless.

Fluoboric acid and phosphuretted hydrogen presented some results of condensation.

Hydrochloric acid liquifies readily, at least at the pressure of 1 atmosphere, but it does not solidify.

Sulphurous acid congeals immediately, as might be expected.

Sulphuretted hydrogen becomes solid, and then constitutes a white, crystalline, transparent mass, rather resembling congealed nitrate of ammonia, or camphor, than frozen water.

Carbonic acid, when it thus passes from the liquid to the solid state, without being dispersed in the fall of snow, constitutes a very beautiful substance, as transparent as crystal, so that I doubted, for a few minutes, whether the tube which contained it was full in vacuô, and I was even obliged to fuse a portion of it in order to ascertain the presence of a solid body. Solid carbonic acid exerts a pressure of 6 atmospheres, which prove to me how easily liquid carbonic acid should become solid when it is laché in the open air.

Oxide of chlorine is a fine, very friable, orange-red crystaline substance; it does not present any indication of explosive power.

Protoxide of nitrogen is one of the gases which I had condensed before. I have seen in the journals that M. Natteréz has repeated my experiments by employing a pump for compression, and that he obtained a liquid in the open air. I, likewise, condensed it into a liquid by means of my pump; but I have also solidified it by the cold bath. It then constitutes a fine transparent or colorless crystaline body; but, in this state, its vapor is not equivalent to the pressure of 1 atmosphere; and this result agrees with another experiment in which, having opened a vessel containing this liquid, part of it evaporated and cooled the remainder, but did not solidify it. The cold produced by this evaporation is very great, which was proved by putting the tube and its contents into a bath of solid carbonic acid and ether in the air. This bath, which instantly freezes mercury, acted as if it had been a vessel filled with a hot liquid, and immediately caused the protoxide of nitrogen to boil violently. From this I am disposed to employ liquid protoxide of nitrogen for new experiments on hydrogen, oxygen, and nitrogen; for, by placing a bath of this liquid protoxide in a pneumatic machine, and by expelling the air and gas, we may place the bath of protoxide relative to that of carbonic acid in the vacuum, in the same ratio as these two baths observed in the air.

Cyanogen congeals, as Bussy has already observed.

Perfectly pure and dry ammonia may be obtained as a solid, white, crystalline and transparent substance, heavier than liquid ammonia, and having very little odor, on account of the feeble tension of its vapor at this temperature.

Arseniuretted hydrogen and chlorine do not pass from the liquid to the solid state.

Alcohol becomes as thick as cold oil, but does not crystallise any more than caoutchouc, camphine and oil of turpentine; but these bodies become viscid.

Binoxide of nitrogen and oxide of carbon gave no sign of liquefaction at the lowest temperature, and at a pressure of from 30 to 35 atmospheres.

In making these general observations, I have determined many numbers relative to the point of fusion of these various gases, to their tension at different temperatures, &c. The numbers will be given in the memoir which I am preparing on this subject; and in which I hope to have something new to say concerning the state which oxygen, nitrogen, and hydrogen assume in passing to the liquid state. Will this last body be developed in the metallic form as you think? Will nitrogen be a metal or will it retain its place among the non-metallic bodies? This will be decided by experiment.

INVESTIGATIONS CONCERNING THE ACIDS OF THE GASTRIC JUICE.*

BY M. MILSEUS.

The physiologists and chemists who have studied the composition of the gastric juice, have always found it acid, and almost all of them have admitted that the acidity was due to the presence of one or several free acids,

* *Journal de Pharmacie*, January, 1845.

phosphoric, lactic, butyric, acetic, and hydrochloric. M. Blondlot has recently denied the presence of free acids in the gastric juice; he has admitted that the acidity of this fluid is due to biphosphate of lime, whose reaction it is impossible to destroy with saturation by means of carbonate of lime.

The facts on which he relies for proving this will be found in his *Tracte Analophique de la Digestion.*

Two objections may be made to these experiments. Indeed, M. Blondlot does not say that he has taken the precaution, in filtering the gastric juice, of using paper free from ash. Again, when he puts gastric juice in contact with carbonate of lime, he observes no effervescence, but he subtracts the weight of carbonate employed before and after the experiment.

It is easy to prove that the gastric juice contains a free acid, concerning the nature of which I do not intend to express an opinion; the following are the experiments on which I base my assertion. 32 grammes of gastric juice, which M. Blondlot had sent to M. Dumas, were put in contact with some pieces of white marble weighing 7 gr.·007; they were left in contact without heat for two days, then they were carefully washed and weighed; their weight did not amount to more than 6 gr.·937; they therefore lost 70 millegrammes. I had made the first experiment by washing them with pure water, drying them to ascertain that no loss of weight had occurred; but small pieces were detached from the lumps of marble which I had used, and although I had taken the precaution of taking an account of them, this experiment did not satisfy me.

73 grammes of the same juice were put in contact with 3 grammes of Iceland spar, and left for 24 hours; after some time, the crystals of spar were covered with small bubbles of gas; when these were displaced by agitation, new bubbles were formed. After the experiment, the surface of the crystals was corroded, and they weighed 3 gr.·354; they had therefore lost 0 gr.·108.

M. Bernard was kind enough to place at my disposal a dog with a stomachal fistula.

68 grammes of very impure gastric juice, full of lumps of bread, dissolved, in twelve hours, 0 gr.·071 of spar.

64 grammes of gastric juice, collected after feeding with boiled beef, dissolved 0 gr.·166 of spar.

60 grammes of chymous paste, collected after a dog had been made to eat mutton fat, and a piece of cartilage, dissolved 0 gr.·057 of spar.

These experiments, therefore, confirm the presence of a free acid in the gastric juice. M. Bernard, moreover, in his thesis, availing himself of the analysis of M. Blondlot, says, positively, that the juice dissolves the metals with effervescence. I have only confirmed his experiments with the balance. M. Dumas had remarked, moreover, that artificial digestions cannot be made, or only very difficultly, with gastric juice which has been filtered through chalk, and that a trace of hydrochloric acid, added to the juice, immediately restored its properties.

ON A MODE OF WRITING ON GLASS.*

BY M. SIMONIN.

I NOW communicate, in few words, an easy mode of engraving on glass, divisions, letters, and characters, not susceptible of being obliterated, of which chemists, pharmaciens, &c., frequently have need in localities in which special industry cannot come to their aid. Engraver's varnish is laid with a soft pencil on the tubes, flasks, &c.; when it is dry, we trace or write with the point, removing the vamish; and on these places is spread a thin layer of a soft paste, made at the moment of use, with powdered fluate of lime and concentrated sulphuric acid. After some hours' contact, it is washed, and the glass is sufficiently rongé and creusé. To mark lines, tickets, &c., in an indelible manner, this paste may be concrete with a leaf of fine laminated lead, in order to render the action more energetic.

PROCESS FOR REDUCING CHLORIDE OF SILVER AND OBTAINING METAL.†

M. OERCHSIE, Controleur Demonnies, proposes the employment of galvanic action for effecting the reduction of the chloride of silver, which is obtained in the humid assay of alloys.

After having carefully washed the chloride of silver, until it contains no traces of acid, it is dried to the consistence of a thick pap, and it is mixed in this state with a saturated solution of chloride of sodium. This humid mass is placed in a porous vessel, or in the bottom of a flower-pot, the hole of which is stopped. In a glazed vessel with a flat bottom, of a different form, are put two blocks of wood of a centimetre and a half in

* *Journal de Chimie Médicale*, January, 1845.

† *Journal de Chimie Médicale*, Jan., 1845.

breadth, and on the latter a plate of amalgamated zinc, presenting a surface equal to that of a porous vessel. On this plate of zinc, two full glass tubes are placed, to receive the vessel containing the chloride of silver. Into the latter is introduced a thin plate of silver or platinum, which is put in communication by means of a small strip of one or other of these metals, with the plate of zinc deposited in the lower vessel; the zinc plate is steeped in a mixture of 20 parts of water and 1 part of sulphuric acid.

From the arrangement results a galvanic action which reduces the chloride of silver from the centre to the circumference. From time to time, when the effervescence on the zinc abates, a little sulphuric acid is added, to replace that which has combined with that metal. It is certain that all the chloride of silver is reduced when the liquor has no longer a milky appearance on being stirred. The decomposition being completed, the saline solution is decanted from the silver; the latter is washed, and after having been dried, it is fused in a crucible with a little potassa.

According to the author, the reduction of chloride of silver by this process does not cost one fourth of the expense required for the reduction by the dry way at present employed. The silver reduced from the chloride by this process, is in so fine a powder that it may be employed for covering, with a hair pencil, objects moulded in wax to be copied by the galvano-plastre process.

[One of the most simple processes consists in taking well-washed chloride of silver, putting it in contact with a zinc plate, in a porcelain capsule, in heating it, if it be desired to accelerate the operation, and in adding a few drops of sulphuric acid; in these two cases, the silver of the chloride is reduced in a very short time. We perform this operation at our lectures, and the process, which is due to Arfwedsor, has already completely succeeded with us.]

MEMOIR ON DIGITALIS PURPUREA.

BY M. HOMOLLE.

The vegetable kingdom furnishes to therapeutics few plants which have been made the subject of such important works as the digitalis purpurea, although, notwithstanding its very powerful and singular properties, it was only in 1721, on the report of Murray, that it was admitted into the *London Pharmacopœia*, and took its place in a treatise on materia medica only in 1788. There is, therefore, some ground for surprise that we are still ignorant as to the nature of its active principle, and the fruitlessness of the means hitherto employed for obtaining it should lead us to regard it as differing in its chemical character from the proximate products obtained from other vegetables.

We will avoid everything relative to the natural history of digitalis, as well as endeavors made to study its physiological and therapeutical action, and we will enter at once into the examination of works relating to its chemical composition.

Destouches and Bidault de Villiers devoted themselves to separating the products soluble in water and alcohol, and to proving the presence of salts of lime in the residue.

Planavia treated the aqueous extract by ether, distilled with water, and thus separated the chlorophylle, then redissolved in ether the aqueous portion evaporated and mixed with oxide of lead; he thus obtained a gluey resin.

Leroyer, of Geneva, prepared an extract with ether, redissolved it in water, treated it by oxide of lead, evaporated after filtration and dissolved again in ether, gave as a last result a product which he regarded as alkaline, and in which he thought he recognised very distinct poisonous properties.

Rein and Hasse, of Stockholm, proved the presence of gum and superoxalate of potassa in digitalis; they attribute its properties to a soft resin, in great part carried down by the green fecula which is precipitated from the juice of the fresh plant.

Welding, whose *modus operandi* we have not been able to find, observed in digitalis a volatile oil, containing a flocculent concrete matter, a fixed fatty matter, a product which he calls digitaline, gallic acid, a red coloring matter, soluble in water, gluten, albumen, chlorophylle, sugar, and mucilage. Digitaline, according to this chemist, enjoys alkaline properties, and is soluble in alcohol and ether; but M. Soubeiran observes that the experiments of M. Welding are far from conclusive.

Dulong of Astafort dissolves the aqueous extract of digitalis in alcohol, precipitates the potassa by tartaric acid, and the excess of the latter by acetate of lead, and the excess of lead by sulphuretted hydrogen, evaporates and treats the residue by acetic ether, which dissolves the distillation which evaporation gives under the form of an orange yellow bitter soft and *filante* mass, deliquescent, but capable of becoming hard and brittle by desiccation. This matter is soluble in water and alcohol, precipitable by subacetate of lead and nut-galls, sparingly soluble in ether.

In 1837, M. A. Henry, Apothecary to the military hospital of Phalsburg, addressed to

journal of the *Société des Sciences, Physiques, et Chimiques de France*, a memoir in which he points out the following process for obtaining the active principles of digitalis in a great state of concentration.

The filtered juice of the plant is treated by boiling carbonate of magnesia until it has lost all acid re-action, then after filtration, evaporated to an extract, which is again taken up by boiling alcohol at 33°. Only one fourth of the extract is dissolved; the remainder insoluble in alcohol and ether, of a slightly saline and by no means bitter taste, constitutes a black mass in which reagents indicate lime and gallic and hydrochloric acid. The alcoholic liquors are distilled, then evaporated to dryness, and the residue is treated by boiling ether, which removes a little green resin. Thus exhausted, the extract is dissolved in distilled water, with an excess of subacetate of lead. It is freed from the excess of lead by sulphurretted hydrogen of the lime by oxalic acid, and by evaporation the filtered liquor gives a dry extract, which is redissolved in alcohol at 36°, and submitted to ebullition with calcined magnesia. The filtered liquor is placed on plates in an oven until completely dried under the form of reddish brown transparent scales, of a slightly sweet taste at first, and afterwards very bitter, half a grain of which administered every three hours lowered the pulse from 150 to 43. Moreover, the juice of digitalis furnished by distillation a limpid liquid of a strong, penetrating, herbaceous odour, and of a slightly acid aromatic herbaceous taste, with traces of volatile oil. The free acid of the plant is gallic acid, to which the bitter principle is to be attributed. The green fecula contains starch; reagents indicates in the juice gallates and hyrochlorates of lime, traces of iron, &c.

M. Ruevenne, principal apothecary to the Hôpital de la Charité, at Paris, in reporting on this work in the No. of the *Journal des Connaissances Médicales Pratiques*, for Dec., 1837, makes the following remarks:—"This process gives all the bitter principal of digitalis; it is the best means hitherto proposed for obtaining it in a state of great concentration, but this product cannot merit the name of digitaline, since it must retain sugar, extractive matter and acetate of magnesia

M. Ruevenne having, on the occasion of this work, repeated the various processes of chemists engaged with this plant, all these processes furnished him as a last result an analagous product, under the form of a reddish yellow extract, of the consistence of honey, and very bitter, more or less soluble in ether.

Some experiments made to determine the real solubility of the bitter principle of digitalis in ether, led him to recognise that this solubility, always feeble, varied according to the degree of concentration of the ether, which dissolves so much the more as it is less pure or mixed with a larger proportion of alcohol.

The *Repertoire de Chemie*, for February, 1838, contains the experiments of Trommsdorff, of Erfurth, on digitalis; the following is a summary of them:—"The juice of the green plant, coagulated by heat, is evaporated at 190° F., of the consistence of syrup, then mixed, after its cooling, with alcohol at 80° centes, until no more precipitate is produced. The alcoholic liquor furnishes, by evaporation, a very bitter and deliquescent extract. This is redissolved in ether, and the ethereal solution, submitted to distillation, deposits chloraphylle, which is separated. The yellowish liquid remaining mixed with pure water, deposits, *during evaporation*, brown yellow oleaginous drops, almost insipid, becoming hard and friable by cooling: then, filtered and evaporated to dryness, furnishes a yellow, transparent bitter mass, with an acid reaction, soluble in water, alcohol and ether, attracting moisture from the air. After evaporation *in vacuô*, this substance presents no trace of crystallisation. Its aqueous solution abundantly precipitates by subacetate of lead and tincture of galls. This last precipitate is immediately dissolved by the addition of alcohol; tincture of iodine, ammonia, electies, proto and persulphate of iron, and nitrate of silver, produce in it no change. Caustic potassa colors it of a deep yellow. In fine, Trommsdorff classes this substance among acid extractive matters.

Finally, in the report made in 1840, by M. Mialbe, in the name of a prize commission on digitalis, I see that the author of one of the two memoirs presented, sent with it a sample of a yellowish white, amorphous substance, soluble in water, alcohol, and ether, and possessing, according to the author, a poisonous action, in the dose of from 15 to 20 centigrammes, but which the commission could not admit as the active principle of digitalis, and concerning the preparation of which it is silent.

In summing up the notices acquired by the works which we have just quoted, we find the following points established:—

1. It is in the bitter matter that the active principle of digitalis resides.

2. This bitter matter is soluble in alcohol; it is sparingly soluble in ether, according to most experimenters; it readily dissolves in water, with the aid of substances with which it is obstinately combined and mixed.

Moreover, it has not yet been possible to isolate it, and all chemists who have investigated it have obtained only a deliquescent extractive matter, more or less colored and always of complex composition; their opinions also are quite contradictory as to its chemical nature and as to the class of bodies to which it belongs.

Finally, analysis has demonstrated, in digitalis, the presence of an acid, partly free and partly combined (probably) with the bitter principle, and resembling gallic acid. Analysis has discovered in it a volatile oil, a fixed fatty matter, a red coloring extractiform matter, chlorophylle, albumen, starch, sugar and gum, plus salts of potassa, and lime.

With regard to the methods of treatment employed, most of them appeared to us *pitcher*, more or less numerous, and complicated.

The data furnished by the works on digitalis, which we quoted at the commencement of this memoir, lead us to lay down the following bases for the work we have undertaken:—

1. To avoid the employment of heat:

2. To treat the dry plant, coarsely powdered, by the method of displacement:

3. To act directly on the liquors thus obtained by way of elimination.

It is useless to speak of all the working in the dark, through which we had to pass, and to enumerate all *mécomptes* which, especially from 1838 to 1840, arrested the progress which we wished to follow. The following we acquired about the month of June, 1840.

The maceratum of digitalis obtained by displacement, was immediately and directly freed from extractive and coloring matters by subacetate of lead, the excess of the latter removed by subcarbonate of soda, which likewise precipates the greater part of the salts of lime. Afterwards treated successively by oxalate of ammonia and ammoniacal phosphate of soda, to remove the remainder of the salts of lime and magnesia (these latter, especially, whose presence, not noticed by previous experimenters, was one of the obstacles contributing to retard our progress), the liquor was precipitated by tannin, which carries away the greater part of the bitter principles. It remained to extract the latter from the precipitate formed by means of tannin; we thought quite naturally of the subacetate of lead; setting out from this hypothesis, that the bitter principle might be an alkaloïd, and proposing thus to obtain, by double decomposition, an insoluble tannate of lead, and a subsalt of digitaline, which might be removed by alcohol.

But here, also, new obstacles presented themselves: difficulties inherent to the employment of subacetate of lead, forced us to pass successively in review, the employment of various bodies susceptible of forming an insoluble compound with the tannin, and of disengaging the bitter principle from its combination. Finally, oxide of lead (litharge in fine powder), mixed in certain proportion with still humid precipittiate of tannin, enabled us to obtain a product which it was easy to bring to the state of purity.

The following is this process: a kilogramme of dried leaves of digitalis, coarsely powdered and moistened, is placed in a displacement apparatus, in order to be treated by water. The liquors obtained and mixed are immediately precipated by a slight excess of subacetate of lead and thrown on a filter. They pass through limpid and almost completely decolored, retaining all their bitterness, and presenting a slightly acid reaction. Carbonate of soda in solution is added to it until no more precipitate is formed. Filtered *de novo*, the liquid is freed from the lime which it yet retains by oxalate of ammonia, then from magnesian salts by ammoniophosphate of soda.

The filtered liquors present a very evident alkaline reaction, have a light-brown yellow tint, and are excessively bitter; a solution of tannin in slight excess is poured in, and the precipitate formed is collected on a filter, and essayed between folds of blotting paper, in order to be mixed while yet moist with $\frac{1}{4}$ of its weight of powdered oxide of lead. The soft paste which results from it is laid on a filter to drain, pressed between blotting paper, and finally placed in an oven to complete the desiccation. It is then powdered, and exhausted by concentrated alcohol.

The alcohol solution obtained sufficiently evaporated at a gentle heat, leaves, as a residue, under the form of a yellowish granular mass, with a small quantity of mother-liquor, the bitter principle still retaining traces of oil, salts, and extractive substances.

This mass is washed with a little distilled water, which removes the deliquescent substances carried down. It is allowed to drain, and redissolved in boiling alcohol, a sufficient quantity of charcoal washed with hydrochloric acid being added, it is boiled and thrown on a filter. The liquor passes through almost colorless; left in spontaneous evaporation in an oven, the matter is deposited partly on the sides of the capsule, under the form of thin, light, semi-transparent layers, and partly at the bottom of the vessel, under the form of agglomerated, granular, whitish flocks.

The perfectly dried product is pulverised

and treated by rectified ether. It is left in contact for 24 hours, then boiled and filtered. This ethereal solution, abandoned to spontaneous evaporation, leaves a slight residue; a white crystaline layer, formed of a certain proportion of the bitter principle, of a trace of green oleo-resinous matter, of a matter possessing the odor of digitalis, and of a substance crystalised in beautiful needles, white, inodorous, of an âpre taste, with a little acridness, insoluble in water and acohol, fusible at a temperature which we have not determined, but which we believe not to exceed 302° F., and constituting, on cooling, a yellow, crystaline, radiated mass. The small quantity which we have hitherto been able to isolate, has not permitted us to determine its other properties. We propose to pursue its study as soon as we shall have been able to procure a sufficient quantity.

An operation made on 250 grammes of carefully prepared aqueous extract of digitalis, which we had successively redissolved in alcohol and in water, did not enable us to obtain the bitter principle solid and completely isolated. Hence we came to the conclusion that the employment of heat necessary for preparing the extract altered this body while yet combined with the bodies which accompany it in the plant.

Another operation made in July, 1842, on two quarts of fresh juice of digitalis, gave us a fine product, but the very small proportion of the latter appeared to us to dissipate all idea of employing this mode of preparation, independently of the immense inconvenience of being able to practise this process only at a certain period of the year. The fermentation caused in the liquors by the presence of a small quantity of sugar in the plant, however low the temperature, or however long the operation was continued, —a fermentation, whose result was always the precipitation of a small proportion of altered bitter matter—caused us, at first, to add to the water used for exhausting the digitalis, $\frac{1}{10}$ of its bulk of alcohol; we hoped, also, by this means, to avoid dissolving a portion of the salts of lime and magnesia. We were obliged to renounce this addition, which rendered the process more expensive, without improving the results. It is sufficient, in general, to carry on every operation rapidly, and to avoid a higher temperature than from 50 to 52°.

We now pass on to the study of the physical and chemical properties of the principle which we isolated, leaving it the name of digitaline, which previous chemists gave it by anticipation.

Digitaline is white, inodorous, difficultly crystallisable, and occurring most frequently under the form of pappy porous masses in small scales. It possesses so intense a bitterness, that one centigramme is sufficient for communicating a distinct bitterness to half a gallon of water $\frac{1}{100000}$. However, the taste of solid digitaline is slow in developing itself, on account of its sparing solubility in water.

It causes violent sneezing, when pulverised, or incautiously stirred, even in small quantities.

Digitaline in solution in water, or alcohol, has no action on reddened or blue litmus paper; it is, therefore, a neutral substance.

A sample of this substance, purified with much care by the employment of solutions and alternate washings, without the medium of charcoal, fearing that the latter might introduce foreign matters into it, served for proving the following properties.

Exposed to the action of the heat of an oil-bath, in a tube, it was only at 356° F., that it began to be slightly colored; at 392° F. it became brown, and towards 400° F. it began to soften into a kind of pap, which swelled up, appearing to acquire a lighter tint, owing to the interposition of gaseous bubbles. The temperature, having been carried to 428° F., the matter diminished in volume, resuming the brown tint which they acquired at 392° F. Tasted after this experiment, it lost a great part of its primitive bitterness, which was replaced by a rough astringent taste.

Another portion, heated in the air, on a sheet of platinum, at first softened into a yellowish pap, which slightly swelled up; then the matter took fire and burnt vividly, but with a rather dull and smoky flame.

Nothing remains visible after this combustion on the sheet of platinum, only the plate being washed with a little distilled water, the latter became alkaline. The same phenomenon was observed in a certain degree, in burning in the same manner a little perfectly crystallised morphia.

If the digitaline is not in a state of sufficient purity it burns, forming a very high porous musbroom, which disappears on the discontinuance of the calcination.

A portion burnt alone in a glass tube spreads acid vapors. If it be burnt with a piece of potassa, the vapors become alkaline, but as we found the same thing in operating comparatively on a fine sample of salicine, we thought that there here occurred a phenomenon similar to that noticed by Faraday, Reiset, and Gerhardt, and that the nitrogen, here disengaged under the form of ammonia, could not arise from digitaline.

We wish to prove the presence or absence

of nitrogen by M. Lassaigne's process (see THE CHEMIST). Consequently, we operated comparatively conforming ourselves to the directions laid down by the author, on digitaline, salicine, and morphia. In the first two cases, we obtained in the last place, only a feeble greenish yellow tint, which we attributed to the persalt of iron, and with morphia a slightly blueish emerald green tint.

Although these last results were not so decisive as we had hoped, owing doubtless to our not having correctly adapted the *modus faciendi* of the author, for, with morphia, a nitrogenous substance, we ought to have obtained a beautiful deep blue tint. Still, since the same results were uniformly obtained in several experiments, we regarded them as confirmative of our first conclusion, namely, that digitaline is not a nitrogenous product.

ACTION OF WATER.

20 centigrammes of the same digitaline in powder were put in contact for 42 hours with 200 grammes of distilled water, at the temperature of 54° F., and occasionally stirred. At the end of this time the greater part of the power was deposited at the bottom of the vessel. A portion of the filtered liquid evaporated in the open air, left as a residue a white pulverulent layer, whose proportion calculated per 100 grammes of water, was about 53 milligrammes, or rather more than $\frac{1}{2000}$.

The rest of the mixture of water and digitaline was boiled for three minutes, filtered on a boiling sand bath; the liquid did not become turbid by cooling. Evaporated, it furnished 119 milligrammes of residue per 100 grammes of water, or rather more than $\frac{1}{1000}$.

During the evaporation of the two solutions, a portion of the matter separated under the form of white flocks, and there were formed on the sides of the vessel some extractive yellowish zones or striæ, which we were led to regard as arising from a commencement of alteration of the digitaline, by the heat employed in the evaporation.

ACTION OF ALCOHOL.

Alcohol is the best solvent of digitaline. It dissolves it in great proportion when cold, and in still greater when hot. The boiling solution does not, however, deposit anything on cooling. Alcohol appears to be so much the better for dissolving digitaline, as it is more concentrated; however, weak alcohol likewise dissolves a very large proportion of it, and it is even sufficient to add to water a small quantity of this liquid to increase perceptibly its solvent power.

When digitaline has been purified by ether, it appears to dissolve with a little less facility in alcohol. The alcoholic solution of digitaline, abandoned to spontaneous evaporation, deposits the latter, partly in the pulverulent, and partly in the crystalline state; often, also, towards the end of the evaporation, the liquid assumes the form of a kind of hydrated mass, which, after complete desiccation, is presented in mamillaniis crusts.

ACTION OF ETHER.

The digitaline which was used in the two following experiments had been previously treated by cold ether. 20 centigrammes were put in contact for 24 hours with about 30 grammes of perfectly neutral ether, washed with water, rectified over chloride of calcium, and at the density of 0·726. The latter furnished a residue, whose proportion was 80 milligrammes to 100 grammes of ether. It was under the form of a white layer, which, viewed with the microscope, appeared to be slightly crystaline.

Ether, of the density of 0·748, furnished in the same circumstances 347 milligrammes of residue to 100 grammes of ether.

The digitaline used in these experiments, not having been previously exhausted by boiling alcohol, the cyphus obtained as representing the proportion of digitaline dissolved in this vehicle, are not absolutely precise, and the only thing to be learned from this experiment is, that concentrated ether dissolves so much the less of this bitter principle, as its density is lower or its degree higher.

ACTION OF ACIDS.

We have not been able to form any combination of digitaline with the acids. This result was anticipated from the time that we ascertained the neutrality of this principle.

Put in contact with concentrated sulphuric acid, digitaline instantly turned black, and soon formed a solution, which appeared blackish-brown when examined in a thin layer on the sides of the vessel; on the following days, this color passed successively to reddish-brown, smoked amethyst, pure amethyst, and to beautiful crimson. If during this interval a portion of the liquid be poured into a small quantity of water, a limpid solution of a fine green results.

Digitaline put in contact with concentrated and colorless hydrochloric acid, promptly dissolves in it, communicating to the liquid a yellow tint, which becomes in a few moments of a fine emerald green, which becomes deep until it is dark-green. After about an hour, the liquor is turbid, and the matter at first dissolved, is very soon precipitated

under the form of green flocks floating in a greenish-yellow liquid. At the end of two days, the flocks passed to blackish-green.

With phosphoric acid digitaline does not dissolve, and acquires only, at the end of two or three days, a slightly greenish tint.

In pure concentrated nitric acid digitaline immediately dissolves, with a disengagement of red vapors, communicating a beautiful orange-yellow tint. On the following days the liquid passes into golden-yellow, and remains so.

In these different reactions of digitaline with the acids, it is understood that the latter is more or less immediately destroyed.

Digitaline dissolves in acetic acid at 10° without coloring it, and without altering it at least, so promptly as in the concentrated mineral acids.

The diluted mineral acids do not appear to us perceptibly a dissolved digitaline.

ACTION OF POTASSA.

A little potassa put into an aqueous solution of digitaline, but slowly causes its bitter taste to disappear; but if the mixture be dried, the bitter taste disappears, and gives place to an astringent taste.

The singular property of the alkalis of destroying the bitter taste of certain bodies, had already been noticed by M. Berichardat, as regards cynisin and lactucine; but we should add, that digitaline, submitted to the action of liquid ammonia for more than 10 days, did not appear to be altered at the end of that time.

SALINE SOLUTIONS.

We have not found any salt which can be precipitated by the aqueous solution of digitaline; thus, with

Iodoretted iodide of potassium.	No action.
Bichloride of mercury.....	,,
Proto-nitrate of mercury containing a little dento-nitrate..	,,
Acetate and subacetate of lead	,,
Nitrate of silver	,,
Neutral perchloride of iron ..	,,
Chloride of gold	,,
,, of platinum	,,
Acetate of copper	,,

A solution of tannin renders the liquid opaque, white in the mass, and only milky if examined in a drop; the precipitate begins to be formed only at the end of 24 hours.

The solution of digitaline in boiling water presents exactly the same properties, without precipitating the salts above enumerated.

Among the different properties of digitaline, which we have just passed in review, there is one which appears to us especially characteristic, and which belongs, to our knowledge, to no other substances; it is that of forming a solution of a fine emerald green color in concentrated hydrochloric acid. This reaction is so much the more valuable as it is sufficient to put a portion of this matter in a tube with two or three drops of hydrochloric acid, to see the green color developed in a few moments, and we think that this character should form the criterion for detecting digitaline in analytical or medico-legal investigations; afterwards come the action of sulphuric acid and that of acetic acid.

The transformation of digitaline into a sharp astringent principle, under the influence of heat on one hand, and on the other hand, of potassa, joined to the slight alteration which we have also observed during the evaporation of the aqueous solution, regarded in a practical point of view, sufficiently indicate that the employment of heat could only be very unfavourable to the pharmaceutical preparations of which digitalis is the base, and that the addition of alkaline salts to them must be carefully avoided.

We have seen pure digitaline scarcely soluble in water; it must not, however, be concluded that this liquid is improper for dissolving the active principle of the plant. Indeed, it occurs in digitalis united with saline and extractive matters, which favor its solubility in this circumstance.

Digitaline, which has not been purified by ether, retaining, as we have said, a green matter, an odorous substance, and a crystallisable principle, recognisable by its peculiar odor, which resembles that of digitalis.

It dissolves only imperfectly in hydrochloric acid, so that the liquor remains turbid, presenting the same intense green color.

Finally, it leaves some light flocks on being dissolved in acetic acid.

If we thus notice the properties of digitaline in this state, it is because it might happen (supposing that other concurrent circumstances had not given a mode of extraction preferable to ours) that the practise of selling digitaline in commerce under the form of white mamillaniis odorous masses, might be continued without having it treated by ether, in the same way as we daily see tannin employed under the form of pale yellow odorous bractæ, although according to the author of its discovery this acid in its pure state is white and inodorous.

We could have desired to give the elementary analysis of digitaline, but besides this analysis not being required by the programme, we must own that we have not had

time to prepare pure products in sufficiently large quantities. It would indeed have been necessary to act on considerable masses of digitalis, the portion of the bitter principle, which is extracted, being always extremely small; a circumstance which we at first attributed to the imperfection of our means, and which caused us long to despair, but which was afterwards explained to us when we ascertained the powerful energy of this substance on the animal economy. We thought, moreover, that if the prize commission of the *Société de Pharmacie* would be kind enough to charge itself with this delicate point of the history of digitaline. Science must gain by it, and that in case it should not think proper so to occupy itself, we should still be in a situation to return to it.

[*The second part of this memoir, treating of experiments on the physiological and therapeutical action of digitaline, will be given in our next.*]

OBSERVATIONS ON THE TARTRATE OF POTASH AND CHROME.

BY W. P. WHEELER, JUN.

In the winter of the year 1830-31, the brother of Dr. Edward Turner discovered the double salt of oxalate of potash and chrome. He prepared it by adding oxalic acid to a solution of the bichromate of potash until effervescence ceased, and then evaporating the liquid.

Subsequently Dr. Gregory formed it by a better process, viz.: by pulverising 190 parts of bichromate of potash 157·5 of oxalic acid, and 517·5 of binoxalate of potash, then pouring hot water over the mixture, and after effervescence had ceased, by evaporating the liquid to the consistence of a thin syrup. In both these processes the effervescence is caused by the decomposition of a portion of the oxalic acid. This salt crystallises in thin long prisms, which by reflected light appear black, by transmitted light blue, and green when reduced to powder. The color of its solution is red and green at the same time. According to the researches of Dr. Gregory, it is composed of 3 equivalents of oxalic acid, 2 of potash, 1 of sesquioxide of chromium, and 6 of water. In the prosecution of 2 or 3 experiments on the nature of the above salt, I suspected that it might be possible to form a similar compound with tartaric acid. This suspicion was strengthened by the known tendency of tartaric acid to form double salts. I took, then, 1 part of bichromate of potash, 2 of bitartrate of potash, and 2 of tartaric acid, and then poured hot water over the mixture, when carbonic acid gas was evolved, and a solution obtained of the same appearance as the solution of the above oxalate.

This process is attended with one inconvenience resulting from the comparative insolubility of the bitartarate of potash. I now procure this salt by adding tartaric acid to a hot solution of bichromate of potash until effervescence ceases, and the color of the solution changes from the orange red of the bichromate to the deep red and green tints of the tartarate of potash and chrome.

Upon evaporating the solution gently down to dryness, a deep black shining mass is obtained, which when reduced to a fine powder, is of a dark green color. When digested in water it dissolves, and a solution is obtained which is green and red at the same time. This salt is not so soluble as the oxalate of potash and chrome. In other respects they are very similar to one another. It is with difficulty obtained in crystals.

Feb. 18, 1845.

ON TWO NEW METALS—PELOPIUM AND NIOBIUM—DISCOVERED IN THE TANTALITES OF BAVARIA.*

EXTRACT OF A LETTER FROM H. ROSE.

I have found in the tantalite of Bavaria two new metals; but, in the memoir which I send you, only one of these metals is treated of. The oxide of the other metal, to which, in the meantime, I give the name of oxide of pelopium, greatly resembles tantalic acid: although I am well persuaded that it differs from it, I will undertake a series of experiments to prove their dissimilarity in a precise manner.

For more than four years I have been occupied in the investigation of the tantalites of different localities, and the tantalic acid obtained from them. An observation made by my brother led me to undertake these investigations. I found that the tantalites of Badenmais, in Bavaria, and the tantalites of North America, have the same crystalline form as wolfram.

I found that tantalites of the same locality, or of different localities, of the same crystalline form, and the same chemical composition, differ from each other in specific gravity.

The analysis of these different tantalites was executed by Berzelius's process, by fusing the ore reduced to a very fine pow-

* *Journal de Pharmacie*, January, 1845.

der, with bisulphate of potassa in a platinum crucible.

To acquire an accurate idea of the composition of the tantalites of Bavaria, and of North America, it was necessary to submit the tantalic acid obtained to very accurate investigations. If the quantity of oxygen of the tantalic acid, obtained from these localities, be compared with the quantity of oxygen of the bases of the protoxide of iron and protoxide of manganese, it is remarked, that the relation which exists between them is not simple. This relation is, on the contrary, simple in the tantalites of Finland; it is as 3 : 1.

The author supposes that the tantalic acid obtained from the tantalites of Finland, alone, is a pure acid, not containing other substances mixed with it: its characters have been described by Berzelius; and it alone must maintain the name of tantalic acid. The acid obtained from the tantalite of Bavaria, is composed of two acids, one of which greatly resembles the tantalic acid extracted from the tantalites of Finland, and which will form the subject of an early memoir; the other also resembles tantalic acid, but differs from it in many essential points. It is the oxide of a metal which differs from the known metals. I call it niobium; and its acid niobic acid, from the name of Niobe, daughter of Tantalus, a name which calls to mind the resemblance of the two metals and their two oxides.

Tantalic and niobic acids are two metallic acids which, in their properties, most resemble titanic acid and binoxide of tin. All four have probably the same atomic composition. Both, when calcined in the state of hydrate, present the phenomenon of ignition. Both, before and after calcination, are white. Tantalic acid, when heated, is colored of a light yellow; niobic acid, on the contrary, assumes a very deep yellow color. Both, after cooling, become colorless. Tantalic acid presents, after calcination, a white powder, without any lustre; niobic acid, on the contrary, presents pieces of great lustre—a lustre equal to that of titanic acid, precipitated by ammonia and calcined, with this difference, that titanic acid has a brownish color, whilst niobic acid remains colorless.

Tantalic and niobic acids readily combine with the alkalis; fused with the alkaline carbonates, they expel the carbonic acid; niobic acid, however, forms a more fusible compound.

These combinations are soluble in water, and in an excess of a solution of a caustic potassa, or carbonate of potassa; but difficultly soluble in an excess of caustic soda, or carbonate of soda. Niobate of soda is much more insoluble, or almost insoluble, in an excess of a solution of soda.

The acids precipitate tantalic and niobic acids from these solutions. Sulphuric acid, with the aid of heat, precipitates both acids completely; but, without heat, it precipitates the tantalic acid only partially, and the niobic completely. Hydrochloric acid, without heat, produces only a turbidness in a solution of tantalate of soda; an excess of acid even causes this turbidness to disappear: with heat, it precipitates the tantalic acid, but incompletely. In a solution of niobate of soda, it produces, without heat, very considerable turbidness, without precipitating all the niobic acid; but, with the aid of heat, it completely precipitates the latter.

Oxalic acid produces no reaction in the alkaline solutions; but acetic acid produces precipitates in them.

Hydrochlorate of ammonia produces precipitates.

If an infusion of galls be poured into a solution of tantalate of soda, acidulated by hydrochloric or sulphuric acid, a light yellow precipitate is produced. In a solution of niobate of soda, this reagent produces a deep orange yellow precipitate, somewhat resembling the precipitate formed, in the same circumstances, in a solution of titanic acid.

Both precipitates dissolve in the caustic alkalis.

Tincture of galls is the best reagent for detecting small quantities of tantalic or niobic acid in acid solutions. But it is to be remarked that the presence of oxalic acid or any other non-volatile organic acid, prevents the formation of a precipitate by means of galls.

Yellow ferrocyanuret of potassium produces, in a solution of tantalate of soda, acidulated with a few drops of sulphuric acid, a flocculent yellow precipitate, sparingly soluble in a great excess of hydrochloric acid; in a solution of niobate of soda, it produces a very deep red precipitate, which in color resembles the precipitate caused by galls in the same solution.

Red ferrocyanuret of potassium produces, in a solution of tantalate of soda, a flocculent white precipitate; in a solution of niobate of soda, a very deep yellow precipitate.

If a sheet of zinc be immersed in an acidulated solution of tantalate of soda, nothing is manifested; after some time a white deposit is formed; this is tantalic acid, which is precipitated when the excess of acid added has dissolved the oxide of zinc formed. In a solution of niobate of soda,

acidulated with a little sulphuric or hydrochloric acid, zinc produces a blue precipitate. In time this precipitate becomes brown. Perchloride of tantalium, prepared by means of tantalic acid, charcoal, and chlorine, is yellow, very fusible, and very volatile.

Perchloride of niobium, prepared in the same manner, is colorless, infusible, and very sparingly volatile.

If any ammoniacal gas be passed over perchloride of tantalium, this gas is absorbed, but the absorption is not rapid, and the perchloride is heated less than the liquid and volatile metallic chlorides in the same circumstances, it is not that the perchloride has not a great affinity, but the new combination formed surrounds the solid perchloride of tantalium, and prevents the reaction from being propagated.

If this new combination be heated, metallic tantalium is obtained, and hydrochlorate of ammonia is disengaged at the same time. By washing with water, it is freed from hydrochlorate of ammonia. Heated in the air, it is converted into tantalic acid, presenting the phenomenon of ignition. The reduction of tantalium requires a much greater heat than that of titanium under the same circumstances. The tantalium obtained is presented under the form of black crusts; water has no action on it.

Perchloride of niobium, exposed to the action of dry ammonia, turns yellow, and becomes powerfully heated, because, being infusible, it presents a greater surface to the action of the ammonia. The new compound when heated immediately blackens, disengaging hydrochlorate of ammonia. The reduction is made at a much lower temperature than that of tantalium. The reduced metal is presented under the form of a black powder. Washed with water, in order to free it from hydrochlorate of ammonia, the washing water passes off clear, so long as the metal is contaminated with hydrochlorate of ammonia. When the washing is nearly finished, the water is turbid. This inconvenience may be avoided by adding a few drops of alcohol to the washing water. Heated in the air, it burns with ignition, and is converted into white niobic acid. Nitric acid and aqua-regia have no action on it; but it is attacked with a disengagement of acid vapors, by a mixture of nitric and hydrofluoric acids. Tantalium acts in the same manner with these acids.

If the same atomic composition be admitted for tantalic and niobic acids, the atomic weight of niobium is higher than that of tantalium.

ON THE VOLATILE OIL OF THE ALLIARIA OFFICINALIS.*

BY THEODOR WERTHEIM.

The plant known by the name of Alliaria of Deland (Erysimum alliaria L.) is remarkable for the garlic odor of its leaves; to this property it owes its latin name, as well as the german term *Knoblauchkraut*, (garlic plant). It is perhaps less known, that the root of this plant, especially in the spring, previous to the full development of the vegetation of the leaves, most decidedly reminds us in flavor and odor of the root of the radish. With reference to this latter, it was evidently proved by experiments lately instituted in our laboratory by Hubatka,† that the volatile oil which it contains is identical with the volatile oil of black mustard seed. It appeared probable, therefore, that oil of mustard may be contained in the root of the alliaria officinalis, though perhaps to a less amount.

To ascertain the merits of the above suggestions, I undertook the investigation of this plant in the laboratory of Prof. Rettenbacher. It being a most common parasitical plant of our gardens, it could easily be collected. 25 pounds of the fresh root were carefully cut, and distilled in a glass retort with water. From this quantity, about 4 grammes of a volatile oil were obtained, which as regards its odor and other properties could not be distinguished from oil of mustard. On treating it with concentrated liquid ammonia, the whole quantity of the oil gradually disappeared, and after this liquid had been evaporated, crystals were produced, fully possessed of the exterior character, and the action of the ammoniaretted oil of mustard. Exposed to a heat of 166° F., they melt, and form a liquid as limpid as water. On becoming solid, the mass presents a concentrically radiated texture. On analysis the following result was obtained:—

0·2908 grammes of the combination, dried at 212° F., gave 0·4405 grammes carbonic acid and 0·1885 grammes water.

0·2650 grammes, on being calcined with a mixture of lime and caustic soda, to determine the amount of nitrogen, according to the method of Will and Warrentrapp, yielded 0·4403 grammes metallic platinum.

The amount of sulphur was determined by the loss.

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* Liebig's *Annalen der Chemie und Pharmacie*, October, 1844.

† Liebig's *Annalen*, xlvii., p. 153.

This gives—

		In 100 Parts. Calculated.	Found.
8 eqv.	Carbon	606·83 .. 41·48 ..	41·65
8 ,,	Hydrogen	99·84 .. 6·82 ..	7·20
2 ,,	Nitrogen	354·08 .. 24·20 ..	24·00
2 ,,	Sulphur	402·33 .. 27·50 ..	27·15
1 atom Ammoniuret of Oil of Mustard		1463·08 .. 100·00 ..	100·00

The combination examined is, therefore, ammoniuret of oil of mustard, and the result of the analyses fully corroborates the suggestion expressed above, that the volatile oil of the root of alliaria officinalis, be identical with the volatile oil of mustard. It then seemed to me the more interesting to ascertain, whether the odor of the leaves, as might have been anticipated, did really originate from a portion of oil of garlic. With this view the leaves and stalks of the alliaria officinalis were, previous to the period of the blossoming of the plant, carefully separated from the roots, and in greater quantities subjected to distillation; but I did not succeed in obtaining any sensible quantity of oil of garlic; the water, however, obtained on this occasion, possessed the characteristic odor and taste of garlic in a still higher degree than the plant itself.

Though the result of this experiment is not calculated positively to confirm the subject in question, it seems still less qualified to negative it. On the other hand, however, on comparing the formula of oil of mustard with that of garlic, a more intimate relationship between these two bodies becomes probable, by which their supposed coexistence, in various parts of one and the same plant, may be satisfactorily accounted for.

The formula of oil of garlic is, namely:

Allyl ($C^6 H^5$) + S = sulphuret of allyl.

The empirical formula of oil of mustard is:

$C^8 H^5 S^2 N$.

The latter grouped in a different manner may be expressed as follows:—

Allyl ($C^6 H^5$) + sulphocyanogen ($C^2 NS^2$) = sulphocyanide of allyl.

As an additional proof of the correctness of this view of the composition of oil of mustard, we adduce the circumstance that in the seeds and blossoms of the cruciferæ hydrosulphocyanic acid was frequently met with, either free or in a state of combination. If we, therefore, consider the oil of garlic and that of mustard, as combinations of one and the same radical, or perhaps at least of two isomeric radicals, their simultaneous appearance in various parts of the same plant may easily be derived from one and the same source.

Originally the oxide ($C^6 H^5$) + O might have existed, to be afterwards converted into sulphocyanide of allyl, and in the leaves and blossoms into sulphuret of allyl. This may be the proper place to mention a remark, which I have had frequent opportunities of making during my labor on oil of garlic. At a certain distance, namely, from vessels in which oil of garlic was kept, or from the apparatus in which it was distilled, I distinctly perceived an odor of radish, and the same circumstance was severally noticed by others. I was unable, until now, to account for this in any other manner, than by supposing that the oil of garlic in the allium sativum, is attended by traces of oil of mustard, which in the products of the distillation, by its penetrating odor, may be perceived at a greater distance than the oil of garlic. Perhaps in the course of my works on the allyl series, I shall arrive at results on which, relative to this point, more certain remarks may be based. —nt—

GREAT QUANTITY OF URIC ACID FOUND IN HUMAN URINE.

BY DR. MAX PETTENKOFFER.

In the month of January, 1844, a girl, aged 13, laboring under St. Vitus's dance, was brought into the Julius Hospital, of Würzburg, in the wards of Counsellor de Marcus.

To ascertain the composition of urine in this disease, this liquid was sent to the clinico-chemical laboratory of Prof. Scherer, who was so kind as to charge me with its examination.

The urine was limpid, of a urine-yellow, and, when recent, of a feeble acid reaction, soon, however, becoming alkaline, depositing crystals of phosphate of ammonia and magnesia. Treated in a water-bath it continued to be of soft consistence.

How was I surprised when, after having poured concentrated nitric acid on the evaporated alcoholic extract, with the intention of determining the amount of urea, I noticed that brown-yellowish needles made their appearance instead of the usual crystaline plates of nitrate of urea.

Under the microscope the needles were found to be six-sided prisms, in some places intermingled with plates of the known form of nitrate of urea.

These needles, when washed with cold water, pressed between blotting paper, and dried, were fused when placed on a moderately heated platinum foil, and became volatilised, without leaving any residue behind.

The fumes gave out the well-known odor of benzoïn.

There was no doubt that this urine must have contained hippuric acid in considerable quantity, combined with potassa or soda, which was separated by nitric acid; and by an excess of this body in a state of concentration was converted into benzoïc acid. When the alcoholic extract of this urine was evaporated, mixed with hydrochloric acid, and allowed to stand for some time, four-sided prismatic needles of hippuric acid were deposited.

1,000 parts of this urine gave 40·668 of solid residue, of which 31·251 were soluble in alcohol. The remaining 9·417, which were insoluble in alcohol, consisted of uric, sulphuric, and phosphoric combinations, with mucous and extractive matters, which were soluble in alcohol, whereas the alcoholic extract contained the hippurates, besides urea, the extractive matters, and the combinations of chlorine.

There being no method known by which we can quantitatively determine, the extractive matters, even only approximatively, I was obliged to confine myself to the determination of the solid residue, and the quantitative analysis of the ashes of the urine.

In 1,000 parts of the urine I found 40·668 solid constituent parts, which on being incinerated gave 10·599 fixed salts. In the urine of some of the following days, I found in 1,000 parts 49·825 of solid residue, and 12·985 ashes. We see that the relative quantities remained here entirely proportionate. In the first as well as in the second case the quantity of the ashes is to that of the solid residue as 1 to 3·8. These ashes consisted of salts, of which 1·866 parts were insoluble in water, and 11·119 parts soluble in water. The insoluble ones consisted of carbonates of lime and magnesia 1·153, and earthy phosphates 0·713.

Of the salts soluble in water were—
3·996 carbonate of soda,
6·181 chlorides of sodium and potassium,
0·128 phosphate of soda,
0·814 sulphate of potassa.

On considering the quantitative proportions of the ashes of this urine, we find that it is composed in a manner similar to those of the urine of graminivorous animals. The most is the preponderance of the carbonic combinations and the small quantities of phosphates. For 100 parts of these ashes 30 per cent of carbonate of soda are calculated, whereas the ashes of common human urine contains 5 to 6 per cent of alkaline carbonates. If we calculate by the alkaline carbonate found the hippuric acid (supposing the carbonic earths to be the bases of the uric acid), we obtain for 1,000 parts urine 12·886 anhydrous hippuric acid, or in 100 parts of solid residue 25·8 per cent, a quantity, probably the greatest ever noticed in human urine.

I took the advantage of the occasion to obtain greater quantities of hippuric acid. The urine evaporated at a gentle heat was extracted by weak alcohol; this extract was again brought to a dry state, treated with cold concentrated hydrochloric acid, and allowed to repose, when hippuric acid gradualy separated in short prisms, in such a manner that the crystallisation presented a granular appearance. 'I learned on this occasion, that, to avoid a great loss, it is indispensably necessary to allow the spirituous extract covered with hydrochloric acid, to stand for several days in a flat capsule at a temperature less than 100° F., because hippuric acid is very soluble in the free hydrochloric acid, which we may obviate, to a great extent, by the slow evaporation of the hydrochloric acid. Whenever I heated somewhat higher the alcoholic extract with hydrochloric acid, to accelerate the evaporation, decomposition took place. I obtained a dark brown magma, from which then very little hippuric acid could be extracted. At the same time a peculiarly penetrating odor was perceived, the same as I noticed when treating cow's urine in a similar manner. The impure hippuric acid was purified after Liebig's method, by boiling with bleaching lime, chloride of lime, and repeated crystallisation.

Through twelve consecutive days I could extract hippuric acid from the urine excreted by our patient in the morning, the greatest part of this representing rather long crystals is in the possession of the pathologico-chemical cabinet of Professor Scherer in Würzburg.

From what I have stated, it may perhaps be supposed that the said nervous affection had some influence on the constitution of the urine; but I must still mention that the patient partook merely of apples, bread and water. Whenever she was induced to taste meat, she vomited it out again; even broth was not well digested. Though I saw the solution of the question in the food, yet the very great quantity of hippuric acid in human urine, and the quantitative proportion of the ashes was still a very strange and singular fact, because until then hippuric acid had been found merely in the urine of very young children, and according to Lehmann, in the urine of diabetic patients, but invariably in exceedingly small quantities; hence its occurrence was always considered as an anomaly. The treatise on the constitution of urine,

lately published by Liebig,* puts this case in full light, he having proved to evidence, that hippuric acid is found in the urine of every every man who partakes of a mixed diet, and on an average nearly to the same amount as that of uric acid, a fact hitherto overlooked, and that the quantitative relations of the salts in urine, in the main, are likewise proportionate to the salts in the food partaken. The above case seems highly to corroborate these statements, since, as far as I am aware, no instance is related in which the influence of the food on the constitution of the urine, was so striking, and became so palpable. By further ulterior observation, I learned that this enormous quantity of hippuric acid disappeared again from the urine of the girl in the same manner as it came. By the most circumspect psychical treatment and diet prescribed by Counsellor de Marcus, the nervous affection disappeared (it seemed to have been called forth by harsh treatment), the patient gradually relished our usual food, and by that the urine became of the same constitution as that of other human beings.

—nt—

PROVISIONAL NOTICE OF A NEW NITROGENOUS BODY FOUND IN URINE.†

BY DR. PETTENKOFFER.

When fresh human urine is cautiously evaporated, in a manner as not to boil, the free acid saturated by corbonate of soda, the residue extracted by alcohol, this extract treated with a concentrated alcoholic solution of chloride of zinc, an amorphous brown precipitate is at first thrown down, which contains zinc; after having been allowed, however, to stand for some hours, small granular and rather hard crystals are deposited on the sides of the vessel, which gradually augment to such an extent as to represent complete crusts. If the whole is collected on a filter, and the residue boiled with a larger quantity of water, the precipitate, which is at first amorphous, remains undissolved, whereas the crystals become gradually completely dissolved. The solution on being evaporated leaves behind a crystalline yellow residue, which presents a great many physical properties of lactate of zinc. With the microscope very fine four-sided prisms with an oblique terminal surface are distinguished. The crystals are most difficultly soluble in water, insoluble in strong alcohol and ether. In its watery solution, we may recognise chlorine, zinc, and an organic substance which is very rich in nitrogen. By repeated boiling with strong alcohol, or washing with cold water, all the salts—which are mostly metallic chlorides—may be subtracted from the crystals. If the crystals are redissolved in water and heated with hydrate of baryta, the oxide of zinc is precipitated, which carries off the greatest portion of the still adhering coloring substances. After the precipitate is thrown down, a current of carbonic acid gas is passed through the liquid, to remove the excess of baryta, carbonate of baryta is thrown down, and carbonate of zinc is formed. In the filtered solution is now contained chloride of barium and the organic substance; it is dried in a water-bath. This residue is dissolved in alcohol, and after having added sulphuric acid to separate the baryta, it is filtered. The solution, in which sulphuric and hydrochloric acids are now contained, is boiled with oxide of lead, which combines with the sulphuric and hydrochloric acids. Completely to remove the chloride of lead, a little absolute alcohol is added and filtered. If the filtered mass by its reaction indicates still an amount of lead, which is to be attributed to the acetic acid which the alcohol might possible have contained; this is to be precipitated by sulphuretted hydrogen. On evaporating in a water-bath, a white crystalline mass is obtained of a neutral reaction, of a taste not very characteristic (at first somewhat bitter, afterwards becoming most slightly saline, and prickling in the pharynx), and is easily soluble in water and alcohol. Being heated on a sheet of platinum, it melts and becomes brown, is carbonised, exhaling a most unpleasant urinous odor, and vapors of ammonia, burning off entirely, though with some difficulty. If to an alcoholic solution of this body chloride of platinum be added, no precipitate is thrown down, whereas chloride of zinc produces a copious white precipitate. This precipitate, on being collected, dissolved in water, and evaporated, reproduces the crystals of the zinc compound, as they are crystallised from the urine.

This body is endowed with the remarkable property of entering with chloride of zinc into an insoluble combination. I found in 0·170 grammes of the pure combination of zinc, 0·038 oxide of zinc, and 0·027 chlorine, a proportion which exactly corresponds to the chloride of zinc.

The organic substance, on being several times crystallised, gave in two combustions with chromate of lead, the following composition, expressed in per cents:

* Liebig's *Annalen*, L. p. 161.

† Liebig's *Annalen der Chemie und Pharmacie*, Oct., 1844.

	I.	II.	III.
Carbon.....	39·37	39·28	39·3
Hydrogen....	6·79	7·39	7·0

The combustion of the pure substance with oxide of copper, gave as a mean of five tubes, the ratio in volumes, nitrogen and carbonic acid, as ..... 1 : 28·6

A second combustion of the combination of zinc, gave the ratio in volumes, as..... 1 : 2·53

Mean from these.. 1 : 2·68

Which proportion, according to the quantity of carbon found, corresponds to 34·41 nitrogen.

A combustion with soda-ash, according to the method of Will and Warrentrapp, yielded 33·63 per cent. of nitrogen, the mean per centage of the two of nitrogen is calculated to be 34·02.

The empirical formula deduced from these numbers is: $C^9 N^3 H^8 O^3$.

As a mean of the analysis was found—		Calculated.
Carbon	39·3	39·2
Hydrogen	7·0	6·4
Nitrogen	34·0	34·7
Oxygen	19·7	19·7

I intend to confirm this formula on another occasion, and to recommence on a larger scale, this labour on the manner in which this highly-important body occurs on, its compounds, &c. I now obtain large quantities of it. Human urine emitted in the morning may perhaps contain 0·5 per cent. of this body.

Perhaps I may succeed in finding more of it in the urine of certain classes of animals. The present formula is distinguished from uramil, discovered by Liebig and Wöhler, by its amount of hydrogen and oxygen,

I commenced my investigations on this subject in the clinico-chemical laboratory of Professor Scherer in Würzburg, and continued them in Giessen, where I intend to recommence them next year.

—nt—

ON IDRYL AND IDRIALIN.*

BY C. BOEDECKER.

Some years ago experiments were made in Idria with the view of extracting mercury, by distilling the mercury ores in close vessels, without access of air, in a furnace of a peculiar construction. In these experiments, which were interrupted by the death of the person who devised the method of operating, a black soft mass was obtained, intermingled with globules of mercury, and was called in Idria *stupp*. A portion of it was sent to me by my friend Alex. Löwe of Vienna, under the impression that it contains idrialin, I charged Mr. Boedecker, one of the most distinguished of my pupils, with the extraction of this substance; he found in it no idrialin, but discovered a new body, the composition of which stands in a simple relation to idrialin, and which undoubtedly originated from the idrialin of the mercury ores. In the following article Mr. Boedecker states the details of his experiments.—[Wöhler.]

The *stupp* was repeatedly boiled with alcohol until it was completely extracted; the yellow extracted mass, filtered while still boiling hot, on being allowed to cool, deposited a small quantity of delicate light yellow leaflets of which we intend to treat in the sequel. The filtered extracts were united, filtered again, and the greatest portion of their alcohol distilled off, when an oily brownish-black mass was deposited on the bottom of the vessel. This mass was soluble in ether and alcohol, from the solutions of which no satisfactory separation could be effected by evaporation; but boiling acetic acid dissolved a body, which on the cooling of the gold-yellow solution, filtered when still hot, was deposited as light yellow extremely fine needles, which essentially differed from the body. The above mass which at first on cooling solidified into a brittle crystalline body, on repeated boiling with acetic acid remained as a similfluid substance.

The substance obtained each time after repeated boiling with acetic acid, was washed first with cold acetic acid, then with water, and dried, and repeatedly crystallised from alcohol. By dissolving in the smallest possible quantity of alcohol, and separating the portions which were first deposited from the solution on cooling, and by frequently repeating this mode of treatment, an additional small quantity of the said leaflets was obtained.

The pure body thus obtained represents a very loose crystalline mass, the particles of which appear to be of the form of needles, on slow evaporation of the alcoholic solution, exhibit papillary groups, forming on the sides dendritic figures; under the microscope heaps of rhombic leaflets are distinguished. In this loose crystalline state, it is almost colorless, of a yellowish-green lustre; crushed or triturated it is colorless. At 186½° F., it melts into a limpid, light yellow, oily liquid, and solidifies at 174°F., into a concentrically radiating, opaque, almost colorless mass.

* Liebig's *Annalen der Chemie und Pharmacie*, Oct., 1844.

Exposed to a still greater heat, it easily and completely sublimes in the form of an extremely fine and loose colorless powder of delicate leaflets, which are most beautifully variegated. On the tongue it excites a sense of slight burning, without any particular taste; its very feeble odor reminds one of that of soot.

In alcohol, ether, acetic acid, and oil of turpentine, when cold, it is sparingly soluble; but very soluble when heated, in such a manner that its solutions saturated at a boiling temperature, on cooling become solid; acetic acid alone, however, is unable to dissolve so much of it. A very trifling quantity of it dissolved in the above liquid, produces a fine variegated bluish color, resembling that of an acid solution of sulphate of chinina. Concentrated sulphuric acid, at the common temperature becomes of a gold yellow color, heated to 212° F., it dissolves it largely, presenting a deep green-yellow liquid, which on being mixed with water remains limpid. Only at a higher temperature sulphurous acid is disengaged. These solutions exhibit likewise a considerable variegation. This behaviour with reference to sulphuric acid, its low melting point, and particularly its composition, prove this body to be essentially different from idrialin. Conformably to what appears from the analyses, this body may be considered as the radical of idrialin, hence I propose to name it *idryl*.

The analysis of idryl were made with oxide of copper, in the following manner:—it was first melted in a sheet of platinum, bent in the form of a little capsule, and together with that introduced into the combustion tube, oxygen being employed to effect the combustion.

The result of the analysis was the following:—

	Idryl.		Carbonic Acid.	Water.
I.	0·3130	grms. gave	1·0840....	0·1570
II.	0·2825	..	0.9785....	0·1365
III.	0·2005	..	0·6875....	0·1020
IV.	0·2625	..	0.9000....	0·1355

For each analysis a substance of different kind of crystallisation was employed. In III. and IV. the oxide of copper was filled in a state rather too cold, and in both, on breaking off the point drawn out, unfortunately a little loss took place.

According to the above numbers, the composition expressed in per centage is as follows:—

	I.	II.	III.	IV.
Carbon ..	94·562	94·575	93·625	93·615
Hydrogen..	5·565	5·353	5·516	5·727

It certainly appears from this, that idryl is composed of carbon and hydrogen, in the identical proportion assigned by Dumas to idrialin, and by Laurent to chrysen, namely, $\underline{C^3}\ H$, which in 100 parts gives—

Carbon	94·75
Hydrogen	5·25

It, however, widely differs from chrysen. This latter body is of a yellow color, melts at 446° F., is insoluble in alcohol, by concentrated sulphuric acid; it is rendered reddish-brown when cold, and dark green when heated. It is readily distinguished from idrialin; a very small quantity of it is sufficient to impart to hot concentrated sulphuric acid a fine, deep, blue color. On being boiled with alcohol or acetic acid, it is sparingly dissolved; its melting point is certainly above 3120° F., wherefore it does not melt in boiling oil of turpentine; and idrialin itself cannot be sublimed in a current of carbonic acid, without undergoing decomposition; a small portion of it only is not decomposed.

The occurrence of idrialin in the ores of Idria, the probable mutual relation between the formation of idryl with the occurrence of idrialin, and the circumstance that one analysis only of idrialin is known, namely, that of Dumas, induced me to undertake a fresh analysis of this body, and the more so, as Dumas, himself, having had but a small quantity at his disposal, seems to be uncertain of the correctness of his analysis.*

With this intention a portion of idrialite (idriolin ore) was sublimed in a current of carbonic acid, to extract the idrialin; a portion of this, in which, examined by a lens, no mercury could be discovered, and which remained perfectly colorless, was employed in analysis I.; on combustion, however, a trace of mercury was most distinctly recognised in the tube of chloride of calcium; for analysis II., therefore, a portion was dissolved in aceton and recrystallised; another portion was sublimed for a mixture of alcohol and oil of turpentine, the sublimate freed, by boiling alcohol, from any traces of oil of turpentine, and employed in III.; for experiment IV. the idrialin was obtained by boiling the idrialite with oil of turpentine, recrystallising the portion separating, or cooling, from a mixture of alcohol and a little oil of turpentine, with an addition of animal charcoal cool, this being likewise colorless, representing a mass of a fine lustre of mother of pearl.

The result of the analysis performed according to the matter above indicated was as follows:—

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* Vide *Traité*, iv., p. 651.

	Idrialin.	Carbonic acid.	Water.
I.	0·2090 grams	0·7030 ..	0·1010
II.	0·1600 ..	0·5370 ..	0·0760
III.	0·2075 ..	0·7000 ..	0·0990
IV.	0·2325 ..	0·7810 ..	0·1105

Reduced to per cent. we obtain—

	I.	II.	III.	IV.
Carbon..	91·842	91·640	92·111	91·722
Hydrogen	5·362	5·270	5·293	5·273
Oxygen	2·796	3·096	2·596	3·005

From this it appears that idrialin is not a mere carburet of hydrogen, but that it likewise contains oxygen. On multiplying the formula $C^{3}\overline{H}$ by 14, the oxygen amounts exactly to one atom for one atom of the body $C^{42}\overline{H}^{14}$, and its composition would be calculated as follows:—

C^{42}	=	3155·040 ..	91·990
H^{14}	=	174·713 ..	5·094
$\overline{O}$	=	100·000 ..	2·916
		3429·753	100·000

The oxygen amounting in four analyses to 2·8 per cent. could not be derived from a substance which was not burned, because at the end of the combustion, during the passage of the pure oxygen, the combustion tube, throughout its whole length, was invariably kept in a state of strong ignition; the oxide of copper reduced to the metallic state during the process of combustion, was at the end of the operation again completely oxidated, and no accident took place by which a loss of carbonic acid or water might have been caused.

Idrialin, therefore, contains carbon and hydrogen, in the same relative proportions as they are contained in idryl; and it is right to consider idryl as the radical of idrialin, the composition of the former is to expressed by $C^{42}\overline{H}^{14}$.

A few experiments instituted with the intention of producing idryl from idrialin, by the action of sulphuric acid, gave no satisfactory result. It is to be regretted that, owing to want of substance, we could neither determine the density of its vapor nor further examine its behavior with chlorine and sulphuric acid. It combines with both, forming, with sulphuric acid, a compound acid, which forms soluble salts with baryta and oxide of lead. Perhaps it is identical with the idrialin sulphuric acid noticed by Schrötter.

The body obtained in fire laminæ, and designated above, presents a behavior different from that of idryl. It is far less soluble in alcohol and acetic acid, and is not affected by concentrated sulphuric acid when cold; when heated, however, it dissolves in the latter acid with a brown-red color; its melting point is found to be above 212° F., yet it is volatile before it is seen to fuse, subliming in fine completely colorless laminæ. Its small quantity admitted of only one analysis being made:—0·140 grammes gave 0·4802 carbonic acid, and 0·715 water. This amounts in 100 parts to:—

Carbon	93·654
Hydrogen	5·666
	99·320

—nt—

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

A COMPOSITION OF MATTER FOR RENDERING WATER-PROOF ARTICLES MANUFACTURED OF LEATHER, SUCH AS BOOTS, SHOES, &c.*

PATENT GRANTED TO WM. J. ROOME.

THIS compound consists of "32 parts mutton or beef tallow, either or both, 32 parts linseed oil, 24 parts yellow bees'-wax, 6 parts neats' foot oil, 4 parts lamp-black, and 2 parts litharge, or red lead. The materials are to be placed in a pot of sufficient size, and reduce the materials to a liquid mass, by the application of heat: when all is melted, to keep stirring the composition until it becomes nearly cold, or while in the liquid state to *fill* it into tin or earthen vessels, for use."

* *Journal of the Franklin Institute.*

MODE OF PREPARING A NEW PIGMENT.*

PATENT GRANTED TO HENRY BESSEMER.

THE new paint, which constitutes the chief part of this invention, is made by mixing metallic powders (known as bronze powder) with gum-resins, oil, and turpentine, in such proportions as to form a fluid, capable of being used in the same way as oil paint.

The following is the mode of using the paint:—Into a copper or other vessel placed over a brisk fire, 8 pounds of gum-copal are thrown, and the heat is so regulated that the gum will fuse in about twenty minutes. An addition is then made of 2½ gallons of drying linseed oil, heated to nearly the boiling

* *Journal of the Franklin Institute.*

point; the oil being introduced in small quantities, and the mixture well stirred up. The mixture is now boiled for about two hours (skimming off any impurities that may arise); after which it is allowed to cool to 150° F., and 25 gallons of turpentine, heated to 150° F., are slowly introduced, and thoroughly incorporated with the mixture by stirring; 1 gallon of slaked lime, or other alkaline matter, is then added, and the mixture is then allowed to rest for three days. When this time has expired, the fluid portion of the mixture is drawn off from the lime, which will have subsided, and is then mixed with the metallic powder, in the proportion of five parts, by weight, of fluid, to four parts of powder. The metallic powders preferred by the patentee, are those manufactured by the process for which he obtained a patent on the 15th of June, 1843.

That part of this invention which is "applicable to the preparing and treating of oils, turpentine, varnishes, and gold size, when employed to fix metallic particles and metal leaf, or as a means of protecting the same," consists in neutralising the acids that are combined with those materials, by the use of lime or other suitable alkaline matter.

The acids in gold size, and such varnishes as are employed to fix metallic powders and metal leaf in japanning, paper-staining, painting, &c., are neutralised in the following manner:—One pound of slaked lime is ground, by means of a mullar, with as much of the gold size or varnish as will reduce it to the consistence of ordinary oil paint, and this mixture is added to the gold size or varnish, in suitable proportions for neutralising the acids contained therein. In treating oils, turpentine, or such varnishes as may be used to protect the metallic powders or metal leaf, the lime, or other alkaline matter, which is added to them (for the purpose of neutralising the acid), must be subsequently separated by filtration; but if any of the oils or varnishes should not be sufficiently fluid to pass through a filter, they are made so, by the addition of spirits of turpentine, which is afterwards separated from them by evaporation.

The patentee claims, as his invention, Firstly—A pigment or paint, consisting of bright metallic particles, combined with purified gums, oils, or spirits, in such manner, and in such proportions, as to form a painting fluid. Secondly—The method hereinbefore described, of preparing a fluid for the purpose of mixing with metallic particles of powder (known as bronze powder), and forming thereby a pigment of paint. Thirdly—The combination of lime, or other suitable alkaline matter, with such gold size or varnish as may be used in fixing metallic powders and metal leaf. Fourthly—The use of lime, or other suitable alkaline matter, for the purpose of neutralising the acid contained in such turpentine, oil, or varnish, as may be used as a covering or protection to metallic powders and metal leaf, and the means, hereinbefore described, of effecting the same.

ON THE FORMATION OR SECRETION OF CARBON BY ANIMALS.*

BY ROBERT RIGG, ESQ., F.R.S.

HAVING given a former article on this subject by Mr. Rigg, we lay our excellent contemporary under contribution for the following:—

"In a former communication," says the author, "I described how the experiment might be made which proves that carbon is an animal formation or secretion; and also directed attention to inferences which may be drawn from this principle of animal economy.

"That animals secrete carbon is an inference which may be drawn from our knowledge of the weight of carbon in the food, and in the air respired by man.

"The weight of carbon in the carbonic acid imparted to the atmosphere by an adult, in twenty-four hours, is, according to

	Grains.
Menzie's experiments.....	6,713
Lavoisier's	6,020
Davy's	5,950
Allen and Pepy's	5,169
Mean.....	5,963

"If we compare the weight of carbon thus imparted to the atmosphere by respiration alone, independently of perspiration, with the weight of the carbon contained in the average quantity of food consumed by an adult, we find that the quantity of carbon given off is greater than that supplied to the body by food. This difference is more apparent in the case of persons who subsist upon scanty diet, such as the inmates of workhouses, prisons, &c.

"By increasing the number, and varying our experiments, we find a solution, both of this less and greater difference, between the weight of carbon respired, and that in the food consumed. Animals appear to have the power of secreting carbon out of other bodies, and this power is called into action in an inverse proportion to the food supplied. Diminution in the quantity of food

* From the *Lancet*, Feb. 22, 1845.

is followed by a diminution in the quantity of carbon in the air respired. The difference between the weight of carbon in the air respired, and that in the food, is greater where the animals subsist upon a scanty, than upon a plentiful supply of food. In this paper I shall confine my observations simply to the secretion of carbon.

"I have, from time to time, subjected a great many articles of food to ultimate analysis. The following table shows the weight of carbon contained in that allowed for each adult per week in the

CITY OF LONDON UNION.

	Grains of Carbon.
Bread, 112 ounces, containing	12,516
Meat, 21 ditto.	3,949
Vegetables, 36 ditto	1,494
Suet pudding, 16 ditto	1,824
Cheese, 14 ditto.....	2,450
Beer, 11 pints.....	4,170
Milk porridge, 10½ ditto	4,440
Soup, 4½ ditto	823
Per week.....	31,666
Per diem.....	4,524

BRENTFORD UNION.

Bread, 84 ounces	9,912
Meat, 15 ditto.....	2,820
Pudding, 14 ditto	1,596
Potatoes, 24 ditto	996
Cheese, 8 ditto	1,400
Gruel, 15 pints	3,465
Soup, 4½ ditto	823
Per week.....	21,012
Per diem.....	3,002

UXBRIDGE UNION.

Bread, 99 ounces	11,682
Meat, 5 ditto	940
Bacon, 5 ditto	1,310
Potatoes, 24 ditto	996
Suet pudding, 28 ditto	3,192
Gruel, 10½ pints.....	2,887
Broth, 3 ditto..	540
Soup, with meat, 4½ ditto.....	1,440
Per week.....	22,987
Per diem.....	3,284

PENITENTIARY, MILLBANK.

Bread, 184 ounces.....	21,712
Meat, 20 ditto	3,760
Potatoes, 80 ditto	3,320
Onions, 16 ditto.....	512
Cheese, 4 ditto	700
Gruel, 11 ditto	3,025
Soup, 4½ ditto.....	573
Per week.....	33,602
Per diem.....	4,800

HOUSE OF CORRECTION, CLERKENWELL.

Bread, 136 ounces.....	16,048
Meat, 18 ditto.....	2,384
Gruel, 11½ pints.....	3,162
Soup, 3 ditto	540
Per week.....	22,134
Per diem.....	3,162

On comparing their daily results with the weight of carbon in the respired air, agreeable to the mean of all the experimenters (5,963), and to the lowest (Allen and Pepys, 5,169), we have—

	Carbon in the food.		Deficiency of mean result.		Carbon per cent. lowest result.
City of London Union	4,524	..	24·2	..	12·5
Brentford ditto	3,002	..	49·7	..	42·0
Uxbridge ditto	3,284	..	45·3	..	36·5
Millbank Penitentiary	4,800	..	19·9	..	7·2
House of Correction, Clerkenwell	3,162	..	47·2	..	38·9

In all these, there is an excess in the proportion of carbon imparted to the atmosphere according to the lowest estimated result of from 7·2 to 42· per cent., and according to the mean estimated result of 19·9 to 49·7 per cent.; and, in the case of persons who subsist principally upon vegetable diet, or who live chiefly upon tea, coffee, and other dilute food, the difference between the weight of the carbon in the food consumed, and that in the above-mentioned estimated results, is still greater. And if we compare the weight of carbon in the food of a man who lives well and works hard (which increases the weight of carbon respired), with that which he gives off by respiration, we find the weight of the former less than the estimated weight of the latter. I make this comparison in the case of my gardener, aged thirty-six years, weight twelve stone, eleven pounds; height five feet, nine inches. His average dietary for many weeks, in the spring season, from his account, was

	Grains of Carbon.
Bread, 6 pounds, containing.....	11,340
Meat, without bone, 4½ ditto.....	15,750
Potatoes, and other vegetables, 6 ditto.....	3,570
Beer, 7 pints	2,590
Butter, 4 ounces.....	1,150
Sugar, 4 ditto.....	700
Cheese, 4 ditto	700
Coffee, 4 ditto	385
Tea, on Sunday morning, say.....	60
Per week.....	36,245
Per diem.....	5,178

"In this example we have a person of more than ordinary size, of thirty-six years of age, in good health, working from morning till night, and increasing in weight upon food which contains less carbon than he gives by respiration, independently of that which passes off by perspiration and by other means.

"These data support the opinion that carbon is secreted by the animal system, and the opinion is confirmed by direct experiment upon other animals. It may be sufficient here to give one experiment, which is easily made, and satisfactory in its results. It was made with a young mouse, of very active habits, which weighed 181 grains. The mouse was placed in a clean wire trap, four and a half inches in diameter, where it remained for twenty-eight days, the period of this experiment, excepting when it was taken out for the purpose of being weighed. It was supplied with food during this period, consisting of

	Grains of Carbon.
1620 grains of bread, containing....	43·8
120 ditto cheese	59·4
140 ditto peas.....	57·1
Total.....	554·5

"This animal was placed under shallow glass shades, containing 210 and 400 cubic inches, atmospheric communication being cut off by mercury. It remained under the smaller from one hour to an hour and a half; and under the latter, from two hours to two hours and a half, each time, when portions of the air were removed, transferred to suitable apparatus, previously filled with mercury, and tested for carbonic acid by liquor potassæ.

"On every day, except Sundays, the mouse was kept under one or the other of these shades from six to sixteen hours each day. After the second day, its habits did not appear to be affected by being placed under the glass shades. By taking the carbon imparted to the atmosphere during these hours as the basis of our calculation, the mouse, during the twenty-eight days, gave off 742 grains of carbon, and increased in weight, during the same period, from 181 to 208 grains.

"The mouse was killed by placing it in a small tube standing over mercury, after which it was exposed for two days to a temperature varying from 212 to 230, when its weight was 56 grains; it was then reduced to a fine powder, which, on being subjected to analysis with oxide of copper, gave the following results:—

Carbon	45·62	..	25·55
Hydrogen	7·04	..	3·94
Oxyen	19·38	..	10·85
Nitrogen	13.46	..	7·54
Ashes.....	14·50	..	8·12
	100·		56·

"Another mouse, of the same brood, which weighed 192 grains, was, on the first day of the experiment, killed and dried, when its weight was fifty-one grains, powdered and analysed in the manner above described. It was constituted of—

Carbon	45·62	..	23·27
Hydrogen	6·82	..	3·48
Oxygen	19·39	..	9·89
Nitrogen	13·55	..	6·91
Ashes.....	14·60	..	7·45
	100·		51·

"If we regard these two animals as similarly constituted at the commencement of this experiment, we have, as a recapitulation—

		Grains.
Carbon in the mouse on the 28th day.....	25·55	2·28
" " 1st day..	23·27	
Carbon in the air respired	741·	186·5
" food	554·5	
Increase.....		188·78

"These, together with similar results obtained by experimenting upon other animals and birds, dispose me to infer that carbon is formed or secreted by animals out of other bodies.

"On a more minute examination of these experiments, we find, that when animals secrete carbon, hydrogen, and oxygen, disappear. This fact leads us to a knowledge

of one source of animal heat; for, imperfect as our acquaintance with the specific heat of bodies may be, it is sufficient to warrant us in saying, that if hydrogen and oxygen disappear, and if this chemical change has for its result carbon, heat will be generated. Our experiments teach us, that proportionate to the exertion of any animal, within certain limits, is carbon secreted. Hence, as every one is aware, warmth is a consequence resulting from exercise of the body.

"One or two questions arise on reviewing this subject—Are nitrogen and inorganic bodies also animal secretions?

"The experiment which proves that animals secrete inorganic matter out of other bodies, is simple and conclusive; but experiments upon the secretion of nitrogen by animals are attended with difficulties not easily overcome. This much is susceptible of experimental demonstration:—animals either secrete nitrogen, or appropriate atmospheric nitrogen to the purposes of life."

HOW TO PREVENT MILDEW.*

In the Geo-Agricultural Report of Middlesex, published in your paper of the 25th of January, you speak of the deterioration of the crops of wheat in some parts of that county from the prevalence of mildew. It appears to me that this arises from the great excess of ammoniacal salts, owing to the heavy manuring caused by the proximity of the metropolis, and it also appears to me that the remedy is very certain and very economical.

About the end of October you published a letter of mine on the growth of wheat, on the same land, in successive years. I refer to it because what enabled me to remedy such an evil will, in all probability, be serviceable to others, if they can be induced to try it. A repetition of some of the facts connected with the experiment will enable me to point out what I think will be found beneficial, and I trust that some one will be induced to try, and publish the results.

In the first year of my experiment (1842), I manured a portion of the field with guano, and another with nitrate of soda; the straw and wheat were very like what you describe the Middlesex crops to be; the blade was discolored and brown, and on looking closely at it, it seemed as if the cellular tissue had been ruptured from some cause or other; the straw was poor, and a bad color, and the sample very indifferent. The year after, I varied the experiment, by mixing silicate of soda with one portion of nitrate of soda, and sulphate of magnesia with another; and still further to vary the experiment, I covered a part of each lot with guano (the salts were sowed the first week in April, the guano a fortnight after); this entirely prevented mildew, and the straw was as bright and good (particularly in that manured with the silicate of soda) as I ever saw, and very near 2½ tons of it to the statute acre (there were 392 stones of 14 lbs). In 1844 I mixed the sulphate of magnesia, silicate of soda, nitrate and common salt together, in addition to a sprinkling of quick lime, and I had not only good straw, but an abundant crop of wheat (50 bushels to the acre); the straw was shorter than in 1843; but I attributed this to the great drought of the season. The quantity of silicate of soda used was 60 lbs.; of sulphate of magnesia 90 lbs. per statute acre.

I do not know whether the use of quicklime is frequent in Middlesex, nor do I know whether the price of it is such as to enable the agriculturists to use it. I have been told that the chalk does not make as good lime for either agricultural or building purposes, as our mountain lime-stone does; but is it ever tried for the former? Is not the practice of chalking resorted to instead? If it is, let me strongly recommend some of the Middlesex farmers to apply some to their wheat now, in portions varying from one to three tons per acre, and see if the abundance and good quality of the crop does not amply repay them. This is on the supposition that the chalk contains little or no magnesia; and I would also recommend it to be used in conjunction with the salts above mentioned; or rather, I would apply the lime now, and the salts in moist weather, any time between the middle and the end of March. Many people laugh at these homœopathic doses of salts applied to land; and ask what can be the good of sprinkling half a cwt. of anything on an acre of ground. The example of common salt in our domestic economy, appears to me to offer a good illustration. On the average, mankind probably do not each eat more than the twentieth part of an ounce of salt per day; but that little is indispensable to their health. If they say, where a little does good, much will do more good, the absurdity is at once apparent with regard to salt in cookery; but it does not seem to be so manifest in agriculture, when I have seen 5, 6, and even 15 cwt. of salt recommended to be applied to an acre. I must admit that the last-mentioned quantity was proposed by a dealer in salt, who might, perhaps, justify the recommendation with the old proverb, "There is nothing like leather."

* *Agricultural Gazette*, Feb. 13, 1845.

Since the above was written, I have seen your paper of the 1st Feb, in which the writer of the Geo-Agricultural Report recommends the use of lime and chalk to the farmers who use much of the London manure. I am glad to find my ideas strengthened by the opinion of a writer who evidently understands what he writes about, but for reasons previously given, I would always use it in conjunction with soluble silica and magnesia, which I consider the most important gifts which science has bestowed on agriculture.

ON THE ADVANTAGE OF REDUCING THE FOOD OF CATTLE BEFORE GIVING IT.*

BY A. GYDE.

As I have just concluded the experiments you wished, I hasten to forward you the results, which are as follow:—Two horses in good health, in daily work, and as nearly as possible equal in size and age, were selected for the experiment. They were each allowed 5lbs. of oats (42 lbs. per bushel), and a sufficiency of good hay, of which they consumed about 17 lbs. per diem each horse. The only difference in the feeding consisted in one horse having the oats thoroughly crushed, and the other being allowed the oats uncrushed. On the fourth day of the above mode of feeding, the solid excrements of each horse were examined. 100 parts of the dung from the horse fed on crushed oats were found to be deprived of all the nutritious matter contained in the food, and to consist of woody fibre, mixed with the animal secretions and some salts; while 100 parts of the dung from the horse fed on uncrushed oats were found to contain 1·4 per cent. of nutritive matter, consisting of starch and gluten, which had not been acted on by the stomach, mixed with the ordinary constituents of the solid excrements of the animal—this arising from the inability of the horse to perform perfect mastication, and must vary with circumstances, such as age and rapidity of feeding. The same horses were then fed with cut and uncut food, consisting of hay cut into chaff, and hay uncut. At the expiration of the third day, the excrements were examined, but no chemical difference in their composition was detected; the food in both instances was found to be equally exhausted of its nutritive matter. The shorter period occupied by the horse in filling its stomach, and consequently greater amount of rest obtained, and the means of mixing food and preventing waste by cutting it into chaff, requires no observation from me, but will be material points in this mode of feeding.

ON THE REAL VALUE OF GUANO.*

BY J. B. LAWES, ESQ.

I CANNOT let the remarks of your correspondent, in a late number of the *Gazette*, pass, without making a few observations upon them, particularly as he has drawn his conclusions contrary to the experience of agriculturists, and as the calculations from which he founds his arguments are, I believe, by no means accurate. Mr. Davis states that 4 cwt. of guano contain only 40lb. of ash, 30lb. of which are nothing more than a carbonate of lime, silex, alumina, and other mineral and metallic matter, which abound in most soils; and that this 40lb. cannot supply 608lb. of ash taken by a crop of corn, together with 4,000lb. or 5,000lb. of organic matter; and that a dressing of dung contains 700lb. or 800lb. of ash. Having collected a considerable number of analyses of guano, such as are imported into London, I can state, with tolerable certainty, that its composition is as follows:

	Peruvian.	Ichaboe.
Water	12 to 15 ..	25 to 30
Organic matter	50 ,, 60 ..	25 ,, 30
Containing 12 to 15 parts of ammonia.		5 to 9 pts. of am.
Phosphate of lime	15 ,, 25 ..	25 ,, 30
Fixed salts	,, ..	8 ,, 10

Dung contains in a Ton.

Water	1,568
Organic matter	567
Containing of ammonia	13
Salts, containing 6 to 7 per cent. of phosphate of lime ..	92
	2,240

* *Agricultural Gazette*, February 15, 1845. † *Ibid.*

The great difficulty of getting a fair sample of dung, makes the analysis of it rather doubtful—the water here is estimated at 70 per cent.; on referring to the average of a number of analyses by Boussingault, I find that he makes 79 per cent. as the quantity of water. The quantity of water in dung decreases when the straw is in very large proportion, compared with the excrement of the cattle; but for the same reason, also, the quantity of ammonia and phosphates in the dry manure decreases, which renders the quantity of these two ingredients about equal in the dry dung. Considering 15 tons dung per acre an average dressing, and 4 cwt. of guano, they will each contain—

GUANO.	lb.
Phosphate of lime	112
Ammonia	54
Organic matter	171
Potash, soda	30
DUNG.	do.
Phosphate of lime ..	100
Ammonia	195
Organic matter	8,505
Potash, soda, and silica ..	1,338

On comparing the guano with the dung it will be perceived that the phosphate of lime is about equal in both, the ammonia in the dung is about four times greater, while in organic matter and alkalies, such as potash, soda, silica, the quantity contained in guano may be left out of the calculation. How is it, then, that guano and other substances are capable of being substituted with equal efficiency for 20 or 30 times the weight of dry dung? It is this:—that some plants are capable of deriving the whole of their organic matter from the atmosphere, when supplied with phosphate of lime, while others require in addition to this an ammoniacal substance. Guano may be therefore considered the type of a perfect manure, and while it remains at its present price it is not surprising that farmers are reducing their stock when the loss arising from fatting cattle is considered, oil cake being 10*l.* 10s. per ton, and beef 6d. per lb. It will be now worth while to inquire whether land is capable of being kept in an average state of fertility by the application of inorganic matter and ammonia, allowing the atmosphere and soil to supply the great bulk of vegetable and mineral matter. In many parts of England a dressing of bone-dust, which consists of phosphate of lime with a highly azotised substance, when placed upon a meadow causes it to yield an increased quantity of hay for many years. I find that I can mow a crop of hay weighing 1¼ tons per acre annually from a field which will produce half-a-ton without manure, either by the use of guano, nitrate of soda or potash, or sulphate of ammonia; and this has been continued for eight years, without decrease of produce. In practice the price of these substances should regulate the choice; the low price of guano at present makes it preferable; were guano to advance to 15s. per cwt. sulphate of ammonia would compete with it successfully.

The two great writers on Agricultural Chemistry, Liebig and Boussingault, have adopted different views with respect to the value of the various elements of manure, the former placing the whole of the value on the inorganic portion, the latter on the azotised portion. The truth, I believe, lies between the two. Some experiments, which I hope soon to place before the public, prove that upon an exhausted soil it is possible to grow turnips in large quantities with manure consisting only of mineral matter, but when wheat is tried with the same manure the produce is very small, and the addition of an ammoniacal salt causes a very marked increase. In growing a crop of turnips with inorganic matter only, the quantity required to produce a given weight of organic matter may be reduced by the use of a substance containing ammonia, and it may be reduced again by the employment of a substance containing ammonia and carbon, hydrogen and oxygen. If this is strictly correct, it would appear that plants consume or return to the soil a quantity of phosphate of lime when condensing their carbon and nitrogen from the air; that when ammonia is added, less phosphate is required; and when a substance containing all the elements of vegetation is employed, a much smaller quantity of phosphate is required.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

ON ZINC IN A MEDICO-LEGAL POINT OF VIEW.*

BY DR. STEUDNER.

Zinc is daily employed in a great variety of cases in the arts, and it is well, in time, to ask whether, from medico-forensic considerations, its use should not be rather restricted.

The author first says, that in a technical point of view, excepting in the pumps of springs, in vessels for crystallisation, and in the forms of sugar refineries, this metal not coming in any direct contact with the human body, is to be considered as not injurious to health; in all other respects its use is to be interdicted. Of all metals, except the alkalies, and earthy metals, zinc (oxide of zinc?) is most easily soluble; it is not only decomposed by water, diluted acids, aqueous solutions of alkalies, but even neutral salts—for instance, common salt—may dissolve zinc, and, therefore, owing to the facility with which it is dissolved, and to the poisonous effects which it produces on the organisation, it should never be employed for the construction of vessels in which meat or drink is to be prepared or kept. The fact that a small quantity of zinc may protect great surfaces of other metals, as tin, copper, steel, from becoming oxidised and dissolved, and in consequence of which it was proposed to employ these metals for making culinary vessels, rendering them innocuous by soldering on a strip of zinc, does not do away with the difficulty; it is true, that in that case meat, even when acid will not dissolve copper, it will act on the zinc instead, and thus nothing is gained, it is a mere exchange of the kind of poison. Zinc vessels may be employed in order to obtain a greater quantity of cream from milk, as is constantly done in the south of Germany.† The innocency of the zinc in that case is to be derived from the circumstance, that it is fixed by the casein, which, as have been proved by experiments, like albumen, decomposes the acid organic salts; and, for this reason, the author believes that dishes which contain albumen or casein are the only ones, which, if the excess of acid be not too great, and the boiling not continued too long, may with impunity be prepared in zinc vessels.* That the zinc of bathing tubs is dissolved by mineral waters rich in carbonic acid, admits of no doubt; hence, the use of zinc tubs should only be conceded in the case of sulphurous waters, and strictly prohibited in others, as the bad consequences might be of a serious nature. The same may be said with reference to alloys, in which zinc enters, employed for domestic purposes, except, perhaps, mortars; for though they are less liable to be attacked by acids, yet these latter bodies may dissolve enough to become injurious. The cases which until now were chiefly considered by medical jurists, are the covering of houses with zinc, the pumps of wells, and the vessels in sugar refineries. With reference to the first, it appears, from experiments made by Boutigny, that water flowing down from the roofing of houses made of zinc, was impregnated with that metal, and for this reason the covering of houses with this metal should be prohibited in places where rain water is collected to serve as drink for man or animals. The use of zinc should likewise be prohibited in the manufacture of pumps for wells, and of vessels used in sugar refineries, as the deleterious effects which may be induced in the latter cases, by the employment of zinc have been fully proved in the *Medic Correspondenzblatt*, of the Würtemberg Medical Association, and in Koch's *Nachtrage zu den Preuss Medizinælgesetzen*, page 116, &c.

* Caspar's *Wochenschrift*, No. 33, 1844.

† The author accounts for this fact in the following manner; the casein which renders the separation of the fat from the milk difficult is fixed by the lactate of zinc by which the amount of cavein in the milk is diminished.

* On this occasion the author directs attention to the poisoning by sulphate of zinc; having found in his experiments that sulphate of zinc is completely decomposed by carbonate of magnesia and albumen, he believes that these substances may be able speedily to remove all the symptoms produced by the zinc poison.

† *Lancet.*

ALCOHOLIC DRINKS.†

The January number of a monthly periodical, entitled *National Temperance Advocate*, contains the names and addresses of 120

highly-respectable medical men (many of them well-known in the profession) attached to the following "Certificate," which (says a correspondent) is likely to be much more abundantly signed, but that hitherto "so great has been found the difficulty of procuring so many minds *to come to one precise conclusion*, and to approve of *style*, *language*, and *phrase*, to suit the case, that this progress has not been attained without great personal labor, much time spent in visits and conversation with individuals in various places, and a very large correspondence through every part of the kingdom:—

"CERTIFICATE.

"We are of opinion that there is no principle of strength or nourishment for the human frame in alcohol, or generally in drinks of which it forms a part, such as ardent spirits, fermented wines, cider, ale, beer, porter, and others; that any trifling portion of nourishment contained in the last three is greatly exceeded by that in barley-water, porridge, or gruel, made from an equal quantity of grain: that alcoholic beverages generate ultimate weakness instead of strength; that alcohol never entirely assimilates with the corporeal system; that intoxicating fluids are in no wise necessary to persons in ordinary health, nor are they required for any particular constitution; that the daily or habitual use of any portion of them (much more what has been generally, but erroneously, thought a moderate portion) is prejudicial to health; that the excitement or cordial feeling they create is mere stimulation, which departs in a short time, and is unproductive of any element of real strength; and that, contrary to ordinary opinion, the health and average comfort of the nation would be greatly promoted by their entire disuse as beverages."

THE JUICE OF LEMONS IN SCURVY.*

BY DR. PUPKE.

THE patient was a prisoner in the town of Gnesen (Prussia), and affected in a high degree with scurvy. The bark, ratantiva, mineral acids, and alum, were employed without effect; but he was restored to health by large doses of lemon-juice, and by the external application of creosote, diluted by a decoction of sage-salvin.

—nt—

* *Medizinische Zeitung*, No. 18, 1844.

POISONING INDUCED BY CARBONATE OF LEAD.*

BY DR. SCHUBERT.

A MAN-SERVANT, of a robust habit of body, about twenty years and some months, having been afflicted with a heartburn, and burning in the region of the stomach, by mistake, partook of from 5 to 6 drachms of kremserweisz (carbonate of lead), instead of chalk, after which he suffered for several hours from burning pains in the stomach, and repeated violent vomiting. Twenty-four hours after this occurrence, when the author visited him, he felt violent pains in his body, it being very tender and inflated; his face was red and tumid; the eyes sparkling; the tongue and mouth were red and dry; violent thirst; the bowels constipated; when the vomiting ceased, the patient took sal. amon. cum. tinct. opii. simpl, and afterwards emuls. oleos. c. tab. amar., and gradually recovered.

—nt—

WHAT BECOMES OF THE COCULUS INDICUS.†

THERE is annually imported into England a very large quantity (a good many years ago, and before the Beer Act was passed, which increased the importation, it amounted to many tons weight) of the fruit or berries of the plant *Cocculus Indicus*, (properly the *Menispermum cocculus*,) a narcotic, acrid poison, which is not used by the apothecary, neither, I believe, in manufactures, and is virtuously disowned by the brewers, large and small. What becomes, then, of this large annual importation of a vegetable poison? Nobody will acknowledge its employment, unless, it is true, the druggist, who does sell it occasionally, in pennyworths, to the idle and mischievous, for the purpose of poisoning fishes. Is not this circumstance, alone, of its being so universally disowned, suspicious? Experiment proves, that the Cocculus Indicus berries produce effects on the sensorium, exactly analogous to those of the variety of strong ales, porter, and beer, in proportion to their degree of potency, rapidly inducing head symptoms, giddiness, intoxication, ringing in the ears, dull stupor, followed by severe headache, oppressive nausea, and (if pushed to excess on the lower animals) convulsions and death. The coincidence is remarkable.

* Casper's *Wochenschrift*, No. 36, 1844.

† *The Beer Houses of England*, by a Medical Practioner.

MERCURIAL OINTMENT.*

I saw in your Paper of the 25th ult. a recommendation to one of your agricultural correspondents, relating to the use of strong mercurial ointment for the eradication of lice from cattle. I do not deny that in the hands of a skilful and intelligent operator the mercurial ointment is a valuable remedial agent for the above purpose, but if placed in the hands of an ignorant and careless cowherd, or farm servant, it is a very dangerous one; for, in the course of my practice I have seen very dangerous symptoms of salivation brought on by its careless and immoderate use.

I have lately been called upon professionally to attend upon many bad cases of vermin on the skin of cattle, and I have invariably found the following ointment or liniment to be effectual in their destruction, without the dangerous results that are sometimes seen from the use of mercurial ointment:—

Take of the prepared fat of geese, 4 oz.; train oil, 4 oz.; sulphur vivum, 2 oz; mix well. The affected animal must be taken into the house, if out, be well cleaned with a strong brush, and a little of the ointment applied, and well rubbed in wherever the lice can be found on the skin; cleanliness must be rigorously attended to, along with good keep and pure water.

I have occasionally used the following decoction of Tobacco:—

Take of fine-cut Tobacco, 8 ounces; boiling water, 8 quarts. Let the decoction stand until cold; then strain for use.

I will relate a case in point, to show my mode of using it. About two months ago I was professionally called upon to attend a young heifer, the property of one of the most extensive dairy farming firms in the vicinity of Manchester. She was of the improved short-horn breed, about two years old, in calf, and very fat. She always had been in a good and well-sheltered pasture, and I could not discover by what means she had caught the infection. Upon examination of the beast, I found her to be literally swarming with lice, which I believe to be the Pediculus capitis, but, not being a professed naturalist, I cannot say if I am right.

The above was the worst case of the kind that I ever saw or heard of, for these plaguing parasites kept the poor, ill-fated beast in continual torment, and, as Mr. Youatt has it, appeared to be actually devouring her before her time. I ordered the beast to be brought out of the field, and placed in a comfortable and clean loose box. She was then well brushed all over with a whalebone brush, after which she was sponged lightly over with the above-mentioned Tobacco-water; plenty of clean straw was then thrown under her, she was supplied with food, and left for the night. The next day there were myriads of dead lice lying on the hide of the beast—indeed, the hair was completely covered with them. I ordered the brush to be applied again, and to be lightly sponged over as before, and in about two days after she was completely cleared of them, and is now as clean as ever.

In using the decoction of Tobacco great care is requisite; for, like the mercurial ointment, I have often seen evil consequences to be the result of its immoderate use, and it ought never to be used, unless under the immediate superintendance of the veterinary surgeon or owner.

Proper care is requisite in keeping the beast well sheltered and clean, after using it, and being supplied with good food, pure water, &c. But, as an innocent remedial agent for the destruction of these parasitical animalculæ, an agent that is safe in the hands of the commonest and most ignorant farm servant or labourer, and one that I never knew to fail if properly applied, I would recommend the use of the first-named remedy.

* *Agricultural Gazette*, Feb. 22, 1845.

EFFECTS OF MILK DIET.*

At the Meeting of the Medical Society, of London, on the 23rd of Dec. 1844, a communication was read from Mr. W. J. Ward, of Brighton, having reference to a late discussion by the Fellows of the Society, respecting the influence of milk as an article of diet. Three cases were detailed, in which the writer was of an opinion that the use of milk diet had been of the most essential service. In the first case, the patient, a female, was suffering from a sloughing bubo of the groin, of a frightful character. Porter, wine, bark, and opium, internally, and all the local remedies usually resorted to in these cases, were tried without effect; the patient was rapidly sinking, but under the use of milk diet, and the application of a stale beer-grounds poultice to the affected part, she rapidly recovered. In the second case, that also of a female, all the symtoms of phthisis pulmonalis were materially mitigated by the use of milk diet, and sanguine hopes were even entertained of a recovery, when the patient fell a victim to having indulged immoderately in the eating of "pumpkin pie." The third case was that of a young lady, aged

* *Lancet*.

seventeen. She stated, that during the last fifteen months she had not retained any food on her stomach for more than three or four hours consecutively. There was very little emaciation, and he should have doubted the fact, if it had not been corroborated by her friends; there was also no apparent motive for simultation. Small alvine evacuations about once a week; menstruation irregular; there was slight pain on pressure at the scrobiculus cordis. He directed small blisters, about the size of a shilling, to be applied about once a week round the neighbourhood of the stomach, and a quarter of a grain of opium, with half a grain of blue pill, to be taken every night.

It was four o'clock in the afternoon when he saw her; she had just rejected her dinner. He directed that she should have nothing but the pill that night, and to be begin the next morning with half a pint of milk, to be taken thrice a day only; at first, dry bread, *ad libitum*, to be eaten with each portion of milk; afterwards bread and butter. He should have preferred asses' milk if it could have been procured. He met the physician who attended her, and at whose suggestion he saw the patient, about three months afterwards, and he told him that she had not rejected her food since the first taking of the milk.

It was probable that the administration of milk in the above cases was useful, as furnishing a food easy of digestion; the smallness of quantity being advantageous, according to a pithy saying of the late Mr. Abernethy, "by putting no more into the oven than it could bake."

Practitioners, according to his (Mr. Ward's) observation, often failed in the cure of chronic diseases, in consequence of the food and medicine being in quantity incompatible with the digestive powers of the stomach—an error sometimes attributable to the practitioner, much more frequently to the patient. One of the mistakes also that we committed was that of hunting after new remedies, whereas the proper application of those already in use was of much more importance in the cure of disease.

The President regarded the communication as valuable in offering practical points. He had seen milk diet of service in cases of hysterical vomiting.

Some discussion took place respecting the meaning of the word "hysterical."

Several members spoke of the advantages of milk diet in certain cases, and laid great stress on the regularity, as to time, with which the milk was administered. Cases, however, did present themselves, in which, probably from peculiarity of constitution, milk as an article of diet did not agree.

ON NARCOTIC MEDICINES.*

BY DR. PERCY PICKFORD, OF HEIDELBERG.

THIS memoir may be summed up in the following propositions based on a great number of experiments:—

1. A narcoti cacts only when it has passed into the circulation.

2. The narccotic passes into the circulation by the veins, and not by the lymphatics.

3. The narcotie being no longer to be found in the blood, or in any other part of the body, it must be admitted that it has a special action on the nervous mass with which it identifies itself, and modifies the functions of the nervous system. The blood is, therefore, only the vehicle of the poison.

4. The symptoms produced by the narcotic on the nervous system differ essentially, according as the poison has arrived at the nervous centre, or at a nerve only.

5. Death, which so rapidly follows the swallowing of a narcotic in a large dose, is due to a modification of the nervous centres; but small doses contribute to death by acting on the nerves, especially on those of the heart.

6. The action of the narcotic is so much the more prompt, as the poison has been conveyed into a part of the circulating system nearer to a nervous centre.

OBSERVATIONS ON THE COMPOSITION OF A NEW SPECIES OF CALCULUS, FOUND IN THE KIDNEYS AND URETERS OF A BITCH.†

BY M. LASSAIGNE.

THE author of this memoir, published in the *Journal de Chimie Médicale*, for 1828, a detail of the analyses of various urinary concretions occurring in the dog. He showed that many of these were analogous in composition to those found in the human bladder; and inferred from thence a manifest conformity as regards the different functions of man and certain of the lower animals of similar organisation. The remarkable feature in the present research is, that the calculus consisted of two organic principles, one belonging exclusively to the products of the renal or urinary secretion, the other only met with heretofore as a

* From *Archer für Physiologische Heilkunde*. Published by Drs. Roser and Wunderlich.

† *Bulletin de l'Académie de Médicine*, Dec. 1842.

secretion of the liver. He has, therefore, very properly called it *urino-biliary* calculus. This fact, which seems unique in the history of concretions, merits the attention both of the medical practitioner and of the physiologist.

The animal which formed the subject of this observation was a bitch of large size, a sort of bull-mastiff, which died in the hospital of the school at Alfort, from dropsy.

The *post-mortem* inspection was made by M. Prudhomme, who discovered in the kidneys, the ureter, and the bladder, a considerable number of small calculi, very irregular in shape, of a beautiful grass-green color. Among other changes, it was remarked that the renal substance presented a faint yellowish tint, the liver contained in its texture little whitish granulations of phosphate of lime, and small well-defined cysts, of the bigness of a pea, filled with a soft matter of a greyish-green, formed of half-coagulated albumen, mixed with a little of the green principle of the bile, phosphate, and carbonate of lime.

A portion of these calculi was carefully dried and pulverised; the powder emitted a slight urinous smell, and had a marked greenish yellow hue. In this comminuted state even boiling alcohol did not seem to act sensibly as a solvent, although the liquid assumed a pale green color. Sulphuric ether afforded the same result. In order to determine the nature of the substance held in solution by these liquids, they were evaporated after being mixed. The residue after evaporation was a minute quantity of resinous like matter, of a fine green, insoluble in water, and which, by contact with dilute nitric acid, immediately changed color; it became bluish, next violet, and lastly bright rose, by prolonged action at the ordinary temperature; this last color finally changed to orange yellow. All these effects of successive coloration induced by this acid, denote positively that the small quantity of green matter taken up by the boiling alcohol was the green substance of the bile, or *cholechroïne*.

The feeble action exercised by alcohol on these calculi led to the substitution of a solution of caustic potash; this liquid, even in the cold, dissolved almost in totality the mass, and disengaged at the same time a strong odor of ammonia. The alkaline solution had a fine greenish yellow hue; saturation with acids occasioned a copious flocculent precipitate, of a greenish white, which, left to itself, was found reduced after the lapse of some hours to a greenish crystalline powder, composed of small flattened needles arranged in crosse. Microscopic examination having led to the belief that this was uric acid, the fact was verified by treating it along with nitric acid at a gentle heat, and evaporating to dryness; whereupon the characteristic carmine tint of muroxide was obtained.

The uric acid, which the potash menstruum had dissolved, was combined in these calculi with ammonia, as shown by its disengagement above. It may be noticed that this acid, which, in a state of purity, is white, was here evidently combined with a certain quantity of the green coloring matter, which neither *water* nor boiling *alcohol* was able to remove. After several attempts, however, it was completely separated by re-dissolving in a minimum of the potash solution, and adding to the liquor eight or nine times its volume of alcohol, at a temperature of 190° Fahr. This caused directly an abundant white flocculent precipitate of super-urate of potash, there being held in solution the green coloring matter combined with potash. By means of filtration and saturation with weak acetid acid, this matter was entirely withdrawn from the uric acid.

The above analysis, carefully repeated on a given portion of these calculi, yielded the following results, abstractedly of the order in which the principles are combined: uric acid, 62·5; green matter of the bile, 12·1; ammonia, 25·4; traces of phospate of lime and carbonate of soda, in 100 parts; or, taking the order in which the principles ought to be distributed, urate of ammonia, 87·9; green matter of the bile, 12·1; phosphate of lime a trace, in 100 parts.

These calculi, then, are not only remarkable for having uric acid as a constituent, a thing of rare occurrence in the canine race; but for having it associated with a principle peculiar to the hepatic secretion. This mixed composition would seem to favor the idea that in certain pathological states the urinary organs and the liver are so closely dependent upon each other, that the functions of the one might sometimes easily supply those of the other, as indeed certain physiological and pathognomonic phenomena tend to establish.

SINGULAR CASE OF RESUSCITATION OF A FROZEN BODY.*

BY DR. DEUBER.

THE person in question was a female beggar, 42 years of age, addicted to the use of spirituous drinks, and covered with vermin. One evening in February, 1844, the ther-

* *Medizinische Zeitung*, No. 21, 1844.

mometer being 18° F., she was found asphyctic, and having been refused admittance into the neighboring houses, was conveyed to a horse stable, where the author found her. The whole body was cold, stiff, and motionless, in such a manner that, with some force, the body might have been dragged round by one arm only without becoming bent. The usual means for restoring life not being applicable in this case, the author ordered the back, the nates, and the extremities, to be flagellated with birch-rods. After ten or fifteen minutes a slight redness appeared, which, this treatment being continued for some time, became more vivid; after half-an-hour one arm, which was raised, fell suddenly down, as rapidly as if it had been struck by a flash of lightning, respiration became visible, and after ten minutes she uttered some sounds. Next morning she resumed her occupation of begging.

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ON THE EFFECT OF THE OIL OF JUNIPER BERRIES ON THE ANIMAL ORGANISM.*

BY DR. SIMON.

(An Abstract of a Dissertation: *De olei Juniperi æthereivi, experimentis adjectis.* Berolini, 1844.)

The results of the experiments made by the author are the following:—1. The oil of juniper-berries employed in large doses (one ounce), is poisonous to the same extent as oil of turpentine. 2. It is absorbed in the animal body (odor in the abdominal cavity). 3. It is secreted by the perspiration of the lungs, and partly with the urine (odor of the exhalations of the urine). 4. It produces no inflammation of the stomach and intestines; but it occasions only a sometimes bloody congestion in the vessels of the small intestines, and is invariably productive of of an augmented secession of the epithelium cells and extravasation of blood in the stomach. 5. It acts strongly on the kidneys, these being found abounding with blood; and, in one case, Bellini's capillary canals were visible in the urine as cylinders. 6. It sometimes acts on the lower intestines; but not always in the same manner as oil of turpentine. The oil of juniper-berries is, therefore, analogous in action with that of turpentine, with the difference, however, that large doses of the latter invariably arrive in the lower intestines, causing diarrhœa and augmented secession of the epithelium, whereas, with juniper-oil, these effects were, in one instance, only noticed by the author.

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* *Medizinische Zeitung*, 19, 1844.

VALERIANATE OF ZINC.*

This seems to be the antispasmodic *à la mode*. It was first prepared by Prince L. J. Bonaparte. It is obtained by saturating valerianic acid with very pure and fresh precipitated oxide of zinc, in a crystalline form, as brilliant pearly scales, very bright and of dazzling whiteness. It is neutral, more soluble in hot than in cold water; soluble also in alcohol, ether, and oils.

According to M. Devay, the valerianate of zinc has been employed successfully in cases of facial and intercostal neuralgia, in megrim, and other affections of a *purely* nervous character. It has also been tried in epilepsy. The dose is from one and a half to four grains in the day. M. Cerulli, of Parma, is stated to have cured three cases of neuralgia (supra and infra-orbital) by administering this salt at the period of the attack, in the dose of a grain and a half daily, divided into two pills. By continuing the medicine in the above quantity a complete cure was effected in one patient after 30 days; in another after 40 days; and in the last after 50 days had elapsed.

COCHINEAL A SPECIFIC IN HOOPING COUGH.†

BY DR. BENNWITZ.

Last winter when, besides measles, the hooping cough was epidemic in Berlin, the author had frequent opportunities of convincing himself of the curative effect of cochineal in the latter disease. This remedy not only considerably mitigated the violence of the disease, and lengthened the intervals between the fits, but complete cure was effected, on the average, in a fortnight. Mostly, if circumstances permitted it, the author prescribed an emetic at the commencement of the disease, and afterwards the cochineal in the usual manner:—

℞.—Cochenillae grana quatuor.
Salis tartari grana octo.
Aquæ fervidæ unciam unam et semis.
Syrupi simplicis unciam unam.

M.S.—The whole to be consumed in 48 hours.

The cochineal was by no means so effective against the cough when attended by measles, as occurred in a case of hooping cough of some time standing which was complicated with measles; afterwards, its use was productive of a speedy recovery.

—nt—

* *Annuaire de Thérapeutique*, 1845, and *Medical Gazette*.

† Casper's *Wochenschrift*, 37, 1844.

IV. REVIEWS AND NOTICES OF BOOKS, &c.

THE CHEMISTRY OF VEGETABLE AND ANIMAL PHYSIOLOGY. By Dr. G. J. MULDER, Professor of Chemistry in the University of Utrecht. Translated from the Dutch, by Dr. FROMBERG. With an Introduction and Notes, by J. F. W. JOHNSTON, F.R.S.S., P., L. and E.—W. Blackwood and Sons. Part I.

We hail with great satisfaction this translation into our language, of a work which has for some time held a deservedly high reputation on the continent, and in this country, among those to whom it has exclusively been made accessible by its translation into the German, than which, we are informed by Professor Johnston, this translation is more correct, and than which it is rendered more acceptable and valuable by numerous additions and corrections transmitted to Dr. Fromberg by Professor Mülder, and by many notes contributed by Professor Johnston. All circumstances considered, therefore, we feel fully justified in recommending this work to our readers.

The present part, which contains the matter of two of the Dutch parts, treats in the first chapter:—I. Of the chemical forces: 1. Determinate value of the force which produces combination; apparent quiescence of the forces during combination. 2. Chemical action is action at immeasurable distances. Polarity of the molecules. 3. The influence of circumstances upon chemical forces. 4. Catalysis; Molecules in motion. Of organic forces:—1. Connection between organic and molecular forces. 2. The development of a germ. 3. Equivocal generation. 4. Transference of vital forces.

Chapter II. is devoted to inorganic, organic, and organised bodies—plants and animals.

Chapter III. to the atmosphere in its connexion with organic nature.

Chapter IV. to water, considered in its connexion with organic nature.

And Chapter V. to the relation of the soil to organic nature.

Each of these divisions in the arrangement is treated in a masterly manner, calculated to render the acquisition of knowledge easy to a mind of moderate capacity.

The subject of this work is, perhaps, considering its bearing on the nature and laws of animated matter, the highest to which the mind can be directed. We can only find space for the following extract on

INORGANIC, ORGANIC, AND ORGANISED BODIES, PLANTS, AND ANIMALS.

"All bodies divide themselves naturally into two principal classes, namely, those possessed of peculiar organs, by means of which certain acts are performed, and those which are destitute of such organs. The former are called *organised*, the latter *unorganised*. Those substances, which are either themselves organised, or which are derived from such as are so, are called *organic* substances; those which contribute to the composition of unorganised bodies, are called *inorganic* substances.

"It is not difficult to indicate the organs of an organised being, if it have them either in great number, or very much developed. Such organs, in the classes of the so-called higher animals, are the eye, the stomach, the liver. It needs no lengthened demonstration to show what is meant by such organs as these. There is no room for doubt, indeed, as to the application of the term, in even the least developed condition of organised beings; for, in general, an organ signifies and instrument by which a certain act is performed: it may be either a very complicated or a very simple one. A mere cell, therefore, is such an organ; the only function necessary to constitute it such, being the capability of producing cells similar to itself.

"This power of forming new substances is peculiar to a certain class of natural products. Such as are endowed with the requisite instruments are called organised. Each organ has its peculiar office assigned to it. But all the organs are, besides, united into one whole: they act in combination to maintain the whole, to give it a substantive existence, to make it perform certain actions—in a word, they form by their union what we call living beings. Living beings, therefore, are organised. Organised beings and substances either have lived, or are still living. In every thing that either has not lived or does not live, we miss those organs, which are destined either to produce new substances, or to perform certain actions; and, from the absence of those organs, their actions also are wanting in such as are unorganised.

"This is the case with all minerals. Their composition, however regular as regards their form and the proportions of their elements, is such that, without the intervention of forces acting from without, the mass persists in the state in which it has once been placed. The whole is composed, in a regular manner, of parts, which are quiescent till their forces are excited by external causes. But all plants and animals

have an exactly opposite character. Every thing belonging to them is in a state of continual motion, till causes come into operations which convert that motion into rest. This motion is accompanied by a constant change in the substance of their parts, which continues even when the mutual harmony of these parts is disturbed—the substantive existence of the individual destroyed. The animal and vegetable worlds are made up of an endless multitude of structures, which are in a state of almost uninterrupted activity;—an activity, the first indication of which is a change in their constituents, while its further manifestations are characterised by a great variety of phenomena, all of which we include under the general name, *phenomena of life*. The regular recurrence of these phenomena we call *health*; their disturbance, *sickness*; their entire suspension, *death*; their harmonious co-operation for a common end, that is, primarily, for the maintenance of the whole, we designate *life*.

"The minutest portions of an organised being consist, for the most part, of very small organs, which unite in vast numbers to constitute a substantive whole, which we call an instrument or organ. Such is the liver or the spleen. Thus, as an organised individual consists of organs, so a larger organ consists of smaller ones; and as the actions of an individual result from the combined actions of all its organs, so again the actions of any larger organ are nothing but the combined actions of the smaller organs, by which the larger is made to appear as a substantive whole. So also in most organs, the functions of the whole is made up of all the function of all its parts combined. Thus the simplest form of a lung is an air-cell, covered with a vascular net of arteries and veins; the smallest form of a submaxillary gland, is a small grain, into which, on one side, arterial blood enters, while the secreted saliva flows from it on another. In the same manner, the simplest form of a plant is a cell, because; if numbers of these are taken and placed together, in various orders, they produce a plant.

"The size of the organs, then, by no means determines whether natural products are either organised at all, or more or less organised. The very smallest of them are still organs—manifesting themselves by certain actions, and originating phenomena, not wholly, but partly dependent on external conditions. Any substance, however, which possesses a greater variety of these organs—each of which, by themselves, or with their respective groups, perform several actions—is properly said to be *more* organised. There exists in nature an astonishing variety of such substances. Some are of exceeding simplicity, as in the lower orders of plants. Others are of a very complicated structure, possessing a variety of smaller organs, united either singly or in groups; and thus acting in a variety of ways. These latter abound in the higher classes of animals.

"It need scarcely be remarked, that each organ, or each of its constituent organs, differs in substance from such as act or manifest themselves differently from them. And this difference appears not only in their substance, but also in their form—the component parts of each being arranged in conformity with the functions they are intended to perform in different classes of individuals. Hence an infinite diversity of products. They can present a new arrangement to the substances supplied to the individual from without; and hence their products are innumerable. The substances thus produced by the organs are called *organic*. They are called organised, if they are either organs themselves, or if they contain organs; but generally organic if they are the products of organs. Organised substances, therefore, are organic substances; but organic are not always organised.

"Organic unorganised substances do not at all differ from inorganic, as to the nature of the substances themselves. But they materially differ as to the arrangement of their constituents. The elements of the organic kingdom are to be met with in substances which are not organic; but wherever we find them, they are the same, and exhibit the same properties. If, however, these elements are placed in certain circumstances, they form combinations dependent on these circumstances. This tendency to combination, which exists in the elements, and which is to be looked for *within*, and not *without*—being dependent on nothing but the elementary forces, concealed, as it were, in the molecules of carbon, hydrogen, nitrogen, and oxygen—this tendency being excited to a determinate degree under certain conditions, produces new substances. This general law, of which chemistry gives innumerable examples, is obvious in organic nature. Slight differences of temperature are quite sufficient to produce, in the chemical laboratory, entirely different combinations from the same elements. In common chemical experiments, numerous minute circumstances influence the nature of the product which results from the combination of two or more elements.

"Looking at the subject in this general point of view, new combinations, entirely different from each other, must be produced

by different organs from the same materials, supplied from without. And if we add to the endless multitude of different organs in plants and animals the diversity of materials afforded from without, then we shall understand how that endless series of new combinations must necessarily arise, which is formed in plants and animals, out of two, three, or four elements, and which chemistry makes known to us. In a word, every change of materials in the organic kingdom, as in the inorganic, is the result of the primary forces of the elements, and cannot be ascribed to any other source.

"By the nature of those products, therefore, the nature of the actions of the said organs is more or less determined. But there are several other effects of these peculiar instruments, which go far beyond a simple change of materials. A great many living creatures are endowed with the power of changing their position at will, and possess for this purpose peculiar organs, in which the change of materials is limited to their own support exclusively—such are the muscles. They have a complete system of organs, by which they are enabled to absorb substances from without, to retain them for some time, to expend them gradually, and to employ them in the formation of new organic substances—such are the organs of digestion. They have peculiar instruments, the organs of sensation, by which certain impressions from the external world may be conveyed to a spiritual being, endowed with a power of observation, will, and even consciousness;—a being which chooses according to fixed rules, forms judgments, and draws conclusions; remembers former situations in which it has been placed; imagines new situations as if they really existed;—a being which is itself conscious, why, what, and for what purpose it exists;—a being which can investigate more or less, and admire the Cause of the universe; which recognises in itself a faint image of the Divinity, who governs both worlds and atoms;—a being which may acquire moral worth, and has a clear idea of the distinction between good and evil;—a being which is conscious that it has been created for immortality.

"How surpassingly beautiful is the visible world! At one extreme of organic nature spiritual man is placed, dwelling for a season in a godlike house, formed by a master-hand, suited to his moral elevation, making him familiar with all nature, and with many of its beauties:—at the other extreme, the minute fungus, scarcely visible, as simple in composition as it is humble in appearance—as ill-furnished with organs, as the human body is rich in them, and abundantly provided.

"It is easy to distinguish between these extremes in the series of organised beings—their difference, indeed, being so obvious that we can scarcely perceive any conformity. But, whether from the one extreme we descend, or from the other ascend, it is exceedingly difficult to draw the line of demarcation between animals and plants. The result of all the endeavors to determine this line of separation has been, that the structures of the individuals, which constitute the transition class between animals and plants, and which partake in an equal degree of the nature of both, do not enable us to reduce them to either class. They may with equal propriety be called either plants or animals.

"There exists a mutual approach between the materials of which animals and plants consist, in certain determinate conditions—an approach which is really remarkable. Miguel has described an epizootical fungus, the Isaria cycadæ; and Virey has made us acquainted with a minute moss upon the silkworm. He has lately recorded further observations, by which the Tinea lupinosa of Guy de Chauliac is reduced to a kind of mucor of the genus Mycodermata. The sporules of these kinds of plants find a fertile soil in the *scales* with which the hairy part of the head of children, &c., is covered: they penetrate into the epidermis, and in this way sustain and feed themselves. To these plants also belong the bran-like moulds, mucors, &c., which have been placed in the order of the Coniamycetes, of Nees von Esenbeck. A great many dartrous pimples belong to the class of the fungus-like mosses. The warts of the human body are classed with the gynosporanges (?) by Meynier of Ornans. It is difficult to make out if the tubercle of the lungs be really a Cycoperdon, and the cancer a Uredo caries.

"Though there is thus no uncertainty as to the characteristics proper to oaks and lions, each of them respectively producing a complete impression on the mind, so as to afford a standard whereby plants and animals may be examined and determined; nature has, nevertheless, established forms of transition, which do not admit of a fixed classification.

"The attempt will, therefore, be always made in vain, to draw a distinct line of demarcation between animals and plants. Dumas is one of the last who has tried to do so (*Comptes Rendus*, 28th Nov., 1842). He thinks that the following table of distinctive properties establishes a new system and gives a clear idea of the difference, which we have more than once stated:—

PLANTS	ANIMALS
Produce indifferent substances containing nitrogen;	Consume indifferent substances containing nitrogen;
,, fats;	,, fats;
,, several varieties of sugar, starch, and gum.	,, sugar, starch, and gum.
Decompose carbonic acid;	Produce carbonic acid;
,, water;	,, water;
,, ammoniacal salts.	,, ammoniacal salts.
Give off oxygen.	Absorb oxygen.
Absorb warmth;	Produce warmth;
,, electricity.	,, electricity.
Are apparatus of reduction.	Are apparatus of oxidation.
Are immovable.	Change their place.

He is unacquainted with the actual state of science to whom this detail is new, or appears to embody the truth. It is universally known, and will appear from what follows, that not one of these so-called distinctive characteristics will stand the test of examination.

"As we are not able to draw a clear and just line of demarcation between plants and animals, neither can we between unorganised and organised substances. As soon as the functions of the organs have reached a minimum, we approach the limits of those series of substances, the most complicated of which may easily be distinguished from mineral matter—but of which the least complicated opens the way to the knowledge of another series of substances, which, being wholly dependent on circumstances, do not possess the power to represent anything but the usual properties which are inseparable from matter.

"1°. The general characters of organised and living beings are, increase and growth, generation and death. Besides these properties, common to animals and plants, the former are endowed with the power of perceiving external impressions, and of moving themselves at will. Schleiden justly characterises animals as having a tendency in their organism, to develop their life to the utmost degree of individual separation, and thus to conceal, internally, their principal organs; that which is shown externally being, as much as possible, a uniform surface. The plant, on the contrary, bears its chief organs without, increases very much externally, and shows its beauties all on the surface.

"2°. If any substance is deprived of the properties common to animals and plants, then it is either an inorganic or a dead organic substance. The latter are characterised by the absence of the effects of organs, which are themselves present; the former by the total want of any organs, which either can now, or formerly could, perform any action in any circumstance whatsoever.

"From the articulations of the Gallionella ferruginea of Ehrenberg being made up almost wholly of oxide of iron, it appears that the transition from organic to inorganic substances is almost as imperceptible as that from animals to plants.

"3°. *Binary and ternary combinations.*—It has been thought that a distinction might be drawn between organic and inorganic substances, by stating that, among the former, ternary and quaternary combinations occur, but in the latter only binary ones. This limit being quite arbitrary, is, at the same time, entirely unchemical. Every one but partially acquainted with the chemistry of organic nature is aware that a great number of radicals have already been discovered, and that the terms ternary and quaternary, as applied to combination, were of no use after the first radical was indicated. Ether does not consist of C^4H^5O, but of $C^4H^5 + O$. The oxygen can be separated from it, and sulphur, chlorine, bromine, iodine, or cyanogen substituted. Thus we have ethyl (C^4H^5) a binary combination. This unites with oxygen, sulphur, &c., and forms another binary combination. All organic substances are, no doubt, built up in a similar way, but the radicals of many of them are not yet known. A few years ago not one was known. Science is gradually discovering more of them, and encourages the hope that, in process of time, every organic substance will be reduced to its own radical.

"4°. If any distinction is to be made between organised and unorganised chemical substances, it should stand thus: that in the former, compound radicals exist, and in the latter elementary ones. But is this distinction founded upon their belonging to two different provinces of nature, or upon a peculiarity in the nature of the carbon, hydrogen, and nitrogen, of which organised substances consist? The answer is obvious; for the organic kingdom is built up out of the four elements—the elements themselves being wholly independent of organic nature. It is, moreover, doubtful, whether all orga-

nic substances contain compound radicals. On the contrary, it is probable that cellulose, starch, gum, sugar, &c., consist of carbon and water. The transformation of tannic into gallic acid affords strong grounds for adopting this supposition.

"5°. *Juxtaposition.*—Another real difference is assumed as to the way in which organised and unorganised substances are formed. In the latter, a juxtaposition is supposed; in the former, a growth, an increase of solid matter in some of its parts. First, this distinction is not quite accurate; for horns, nails, and hair, which are all produced from protein compounds, are formed by juxtaposition. Of hair, this is proved by Dr. Van Laer beyond dispute.

"But let us consider this juxtaposition in the unorganised kingdom. When burned gypsum, after being mixed with water, changes into a solid mass, we look in vain for any juxtaposition. Does anything else happen in the deposition of the materials of bone, within the cells of the bones? Does not the bony matter of the skull extend itself from a centre in the form of rays? Let us take an example from the vegetable kingdom. How does the formation of wood take place? Is it not a real superposition when layers of woody fibre are deposited against the walls of a cell? But the very same happens also in the ordinary growth of an organ. Matter does not *penetrate* into matter. If the germ of an organ, produced by simple juxtaposition, once exist, then the organ cannot increase in bulk without one particle placing itself *next* to another, and so shifting its place; and this is a real juxtaposition. This is fully confirmed by the way in which cells are produced—a subject with which, in recent times, we have become somewhat better acquainted. There exists, however, a difference between crystals and many organic substances. The cellular and fibrous primary forms which are present in the latter, are wanting in the former; while the symmetrical forms of the crystal are not found in organic nature. The latter is just as averse to symmetrical forms, as inorganic nature is to cells and fibres. We seldom find crystallised compounds in the bodies either of plants or animals; and the substances contained in them, which are susceptible of regular crystallisation, exist there usually in a state of very fine division, or of solution. In regard to theïn, which is so beautifully crystallisable, we do not find a trace of it crystallised either in coffee or in tea.

"If we add to this, that many organic substances, provided they are placed in favorable conditions, assume juxtaposition—for instance, tartaric acid, citric acid, &c., morphine, narcotine, many resins, camphor, &c., which are all organic substances—then the positive distinctions between organic and inorganic nature, grounded on this peculiarity, disappears. We may, however, state, as distinctions between organised and unorgacised substances, that generally the former are in a cellular, the latter in a crystalline state; that the one class performs a function of which the other is not capable.

"After what has been said, we must necessarily conclude that the causes of this diversity are reducible to the forces which primarily exist in the organic molecules, carbon, hydrogen, nitrogen, and oxygen, and in those of all the other elements. The four organic elements have the peculiarities which are indicated in organic nature by their almost innumerable mutual reactions and combinations. To these elements a potency is imparted, is such an expression ought to be retained in science.

"6°. Finally, between plants and animals, which are both characterised by the cellular form of the component organs, there is this real difference, that cellulose ($C^{24}H^{21}O^{21}$) forms the principal part of the cellular mass in plants; while in animals the primary material is gelatine ($C^{13}H^{10}N^{2}O^{5}$), or the matter which produces glue after ebullition; and to this rule no exception has yet been discovered, either among animals or plants."

A TOXICOLOGICAL CHART, exhibiting at one view the Symptoms, Treatment, and modes of detecting the various Poisons, Mineral, Vegetable and Animal; to which are added concise directions for the treatment of suspended animation. By W. Stowe, M.R.C.S.E. Tenth Edition.

Of this greatly improved edition of a chart which has for many years maintained and deserved the highest reputation, we can only say that it should be suspended in every house in the kingdom.

THE PHARMACOPÆIA A DEAD LETTER; or an Exposure of the Systematic and Fradulent Adulteration of the Pharmacopœia, with Suggestions for its Prevention. By William Bastick, Member of the Pharmaceutical Society, and late a Student of the University of Giessen. London: published by the Author, 2, Lower Brook Street, Bond Street.

The adulteration of substances employed for the cure or mitigation of the diseases inci-

dent to humanity, and the substitution, by the wise in their own conceit, of inefficient or inert formula for those whose efficacy has been confirmed by practice, are crimes of equal enormity deserving of an equal degree of punishment, and the fact that both are allowed impunity in the most intelligent nation in the world, cannot but arouse the wonder and anger of all who reflect on the subject for one moment.

In the course of our career as editors of this journal, we have at various periods entered on this melancholy subject in its columns, and have also made private appeals for consideration of it in high quarters. But we fear that her Majesty's ministers are too fully occupied in regulating financial measures to take in hand a matter having no influence either on their tenure of office or on the revenue of the country, and regard only the sanatory welfare of the people. By the exertion of great influence, ministers have been induced to bring forward a bill for the regulation of the medical profession, calculated to benefit the exerters of that influence only, and not the main body of the profession.

It is quite clear that no measure such as is required will secure the attention of the legislature unless imperatively demanded by pressure from without. For this, the public is too apathetic, and nothing but a well organised system of agitation will arouse it.

The open and undaunted exposure of a part of this system by the intelligent author of the pamphlet before us calls for the admiration of his brethren and the public, all of whom we seriously exhort to read it. Facts disgraceful to the parties concerned are published.

As we hope to be enabled shortly to take up our pen in this cause with some effect; we content ourselves with quoting the following passage:—

"Let any man read the Pharmaceutical Transactions for the last three years, and he will there find a multitude of examples of this system of substitution and adulteration. Many of the leading members of the society have continually avowed this practice. Refer to the papers read before the society, under the head of Unguent. Hydrar. Nit., as a proof of what we assert, or to the many articles on the subject of extracts. There has been a never-ending contest and discussion as to who could prepare the best Ung. Hydrar. Nit., not by following the directions of the Pharmacopœia, but by deviating from them. These malpractices were declared, by the respective members, to be the methods which they followed, not by way of an experiment, but for the public consumption."

TO CORRESPONDENTS.

"Amicus" will oblige by sending us his address.

"Mr. Rees" will receive a letter by post.

SUMMARY OF METEOROLOGICAL OBSERVATIONS MADE AT OTLEY, YORKSHIRE, IN JANUARY, 1845.

BY E. THOMPSON.

BAROMETER.

Monthly mean	29·73°
Mean at 9 a.m.	29·60
,, at 3 p.m.	29·67
,, at 9 p.m.	29·93
Maximum on 7th	30·06
Minimum on 20th	28·90
Range	1·16

THERMOMETER.

Monthly mean	35°
Mean of maxima	39
,, minima	32
Maximum on 6th	47
Minimum on 30th	17
Range	30

Rain.—0·16 inches.

WIND.

Days		Days.
N. 0		S. 0
N.E. 0		S.W. 10
E. 1		W. 8
S.E. 0		N.W. 2

WEATHER.

Days.		Days.	
Clear	3	Rain	4
Cloudy	2	Misty	5
Overcast	17	Frost	9
Showers	1	Hoar frost	0

GENERAL REMARKS.

The barometer during the month has been generally low; the thermometer about the average. The first three weeks were characterised by foggy and cloudy weather, with little frost. At the commencement of the fourth week, a storm of wind occurred, which was the precursor of frost and snow.

Febrile and exanthematous diseases have prevailed.

Nota Bene.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of the Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th; Advertisements on or before the 27th.*

THE CHEMIST.

I. CHEMISTRY.

ON CHINOLINE.*

BY C. BROMEIS.†

In considering the analyses of quinine and cinchonine, executed by Liebig, we find that the two alkaloïds in their atomic composition, differ only by one equivalent of oxygen, and must be regarded as various degrees of oxidation of one and the same radical. This view becoming more plausible by the simultaneous appearance of the two alkaloïds in one and the same plant, I undertook a long series of experiments, intending to convert the lower degree of oxidation of cinchonine into the higher of quinine; but these, I regret to say, were wholly without success. When I was engaged in these experiments, Gerhardt communicated his interesting observation on the decomposition of the alkaloïds, and on the preparation of chinoline.‡

Gerhardt treated quinine at a boiling temperature, with a highly concentrated solution of potassa, and obtained thereby a volatile, oily, nitrogenous base, which he called *chinoline*.§ the more detailed properties of which, however, he did not fully describe in his first treatise. He also at first communicated only an incomplete analysis of the platinum salt, and left the composition of chinoline unestablished. This decomposition of the alkaloïds by caustic potassa, seemed to me most appropriate to furnish the clue to the relation between quinine and cinchonine.

With this view I put in a tubulated retort, one ounce of cinchonine (in the same manner as Gerhardt did with quinine), three ounces of solid hydrate of potassa, one quarter of an ounce of water, and maintained them in a state of continued boiling. After the greatest portion of the water was distilled off, a considerable disengagement of hydrogen commenced, the whole mass was covered with much froth, presenting in several places an intense purple color; during the operation this color became more universal and manifest.

Simultaneously with the coloration a volatile, oily body was formed, which, in conjunction with the water distilled over into the recipient, fell there to the bottom. The water distilled off was restored by a few drops every five minutes, or else the retort would have been corroded, ammonia being disengaged, and the whole mass easily becoming brown, and burned.

After the operation was brought to a close, a great excess of caustic potassa with carbonate of potassa remained in the retort.

The body thus obtained coincides in all its properties with the chinoline prepared by Gerhardt from quinine, which Gerhardt entirely corroborated in a more complete treatise afterwards published by himself;* in a similar manner, he likewise succeeded in preparing the same body from strychnine. Gerhardt, at first, communicated a formula of the composition of chinoline, which, as stated already, was found to be unsatisfactory. According to this, it contains two atoms of oxygen; in the latter treatise these are properly omitted. The amount of carbon and hydrogen are likewise changed; hence the two formula differ entirely. He also analysed only the body in its combination with platinum, and not in its pure and hydrated state. This circumstance, and the fact that

* Liebig's *Annalen der Chemie*, vol. x., November 30, 1844.

† The results of this work have already been communicated to the association of German natural philosophers at Mainz. By a change in my position and residence, however, I have hitherto been prevented from publishing it.

‡ Liebig's *Annalen*, vol. xlii., p. 310, and xliv., p. 279, and *Rechèrches sur la Classification Chimique des Subst. Organiq.* p. 42.

§ Berzelius, in his last annual report, to prevent mistakes, altered the name *chinoleïn* into that of *chinolin*; wherefore it is likewise done here.

* *Rechèrches sur la Classification Chimique des Subst. Organiq.* p. 42.

the analytical results obtained by myself, do not completely coincide with those of Gerhardt (especially with reference to the amount of hydrogen) and this being likewise the case with reference to the hydrates of chinoline, it may be useful here again briefly to state the following results.

Chinoline prepared from cinchonine is heavier than water, throughout oily, of an intensely bitter taste, in its odor very slightly resembling prussic acid, is of a strong alkaline reaction, soluble in alcohol, ether, and in all diluted acids.

Chinoline prepared in the said manner, though directly limpid as water, still mostly contains some ammonia and water. To free it from these, it must be subjected to a fresh distillation.

By this treatment the chinoline, at first limpid, becomes turbid, on account of the particles of water which, separating, distil over first, containing the whole amount of ammonia, which possibly might have been mixed with the chinoline. If a great portion of the water be distilled off, the whole mass again becomes limpid; the chinoline then enters likewise into ebullition, when first it passes over in conjunction with a little of the separating water; afterwards, however, it comes off in rather an anhydrous state, and by conducting the heat properly, it leaves no residue.

Notwithstanding its high boiling point, it is so volatile at a common temperature, that, if one drop of it be thrown on paper, a stain is produced similar to that from a fatty substance; but which, after a short time, completely disappears. Cooled down to 68°F., both hydrated chinoline and the anhydrous one remain fluid and limpid.

HYDRATE OF CHINOLINE.

(*a*) If raw chinoline be not subjected to distillation, but freed only from the admixed ammonia, by being shaken with cold water at 32°F., a limpid oil, saturated with water is formed.

Reduced to this state, and treated with oxide of copper, at the elementary analysis, the following numbers were obtained:—

I. 0·2085 grammes gave 0·5340 of carbonic acid and 0·1335 water.

II. 0·2400 grammes gave 0·6130 of carbonic acid and 0·1505 of water.

To these quantities the following correspond:—

		Calculated.		Found.	
				I.	II.
19 equivalents	Carbon	1452·3	70·28	70·80	70·62
11 ,,	Hydrogen	137·3	6·64	7·11	6·97
1 ,,	Nitrogen	177·0	8·57	8·57	8·57*
3 ,,	Oxygen	300·0	14·51	13·52	13·84
		2066·6	100·00	100·00	100·00

(*b*) If chinoline, at 30°F., be saturated with water, and heat then applied to it, at 59°F., water separates, and it becomes turbid; and at 212°F., the water, being entirely volatilised, carries off a small portion of chinoline. If the application of heat be discontinued at the moment when the chinoline becomes completely limpid again, and this latter body be now subjected to analysis, the following result is obtained:—

I. 0·2010 grammes of chinoline which was not distilled, but dried at 412°F., on combustion with oxide of copper, yielded 0·5740 of carbonic acid and 0·113 of water.

II. 0·2835 grammes of the same substance gave 0·8109 of carbonic acid and 0·1610 of water.

III. 0·2000 chinoline prepared from a second portion of cinchonine, distilled over again with aqueous vapor, and afterwards from the water, on combustion with chromate of lead, gave 0·5725 of carbonic acid and 0·1180 of water.

* In this, as well as in both subsequent combinations, the amount of nitrogen could not be determined by the method of Warrentrapp and Will, because the chinoline, even though the tubes employed were three feet long, and heated to whiteness, distilled over into the recipient containing hydrochloric acid in an undecomposed state, by which circumstance the amount of nitrogen appears always to be too high, on account of a difficultly soluble double salt being formed by the combination of chinoline with hydrochloric acid, as is the case with chloride of platinum; a circumstance which in the combustion of difficultly soluble bases, may frequently occur, as several bases on being fused with hydrate of potassa, or soda, furnish, perhaps, chinoline, or a similar body difficutly to be decomposed. In the above analyses, therefore, the amount of nitrogen was calculated from the analyses of the platinum salt.

IV. 0·2290 grammes of the same substance, on being ignited with oxide of copper, gave 0·6570 carbonic acid and 0·1355 of water.

To these results corresponds the following composition :—

		Calculated.		Found.			
				I.	II.	III.	IV.
19 equivalents	Carbon	1452·3	78·87	78·97	79·05	79·15	79·35
9 ,,	Hydrogen	112·3	6·24	6·40	6·31	6·55	6·57
1 ,,	Nitrogen	177·0	9·61	9·61	9·61	9·61	9·61
1 ,,	Oxygen	100·0	5·28	5·02	5·03	4·69	4·47
		1841·6	100·00	100·00	100·00	100·00	100·00

In the operation of drying, it becomes difficult to hit on the point, in which the water is completely removed; and it is likewise almost impossible to prevent a trace of carburetted hydrogen being formed at the preparation of chinoline, owing to a small quantity of water distilling incessantly; these circumstances satisfactorily account for the differences occurring in the above analyses as well as for their deviations from the calculated results.

ANHYDROUS CHINOLINE.

To obtain chinoline absolutely free from water, it must be put into a small retort, allowed to stand for a long time over fused hydrate of potassa or chloride of lime, and is afterwards distilled.

In the anhydrous state it possesses a still more intense taste and odor, becomes by careful distillation limpid like water, afterwards, however, readily acquiring a yellow tint, being rendered of a more dense consistence, and heavier. At 59°F., as a mean of two coinciding weighings, its specific gravity was 1·084.

On combustion with oxide of copper it gave :—

I. 0·2045 grammes 0·6205 of carbonic acid and 0·1100 of water.

II. 0·2730 grammes 0·8285 of carbonic acid, and 0·1445 of water.

Hence the following composition is calculated :

		Calculated		Found	
				I.	II.
19 equivalents	Carbon.....	1452·3	84·00	83·86	83·90
8 ,,	Hydrogen....	99·8	5·76	6·11	5·88
1 ,,	Nitrogen	177·0	10·24	10·03	10·22
		1729·1	100·00	100·00	100·00

From the above numbers, therefore, it most clearly appears that the two first combinations are only hydrates of the third, the anhydrous chinoline. In each the combination C^{19} H^{8} N is contained, in one instance, with three, and, in the other, with one atom of water; their respective compositions may, therefore, be expressed by the following formula :—

Anhydrous chinoline .. $C^{19}H^{8}N$
First hydrate ..... $C^{19}H^{8}N + HO$
Second hydrate ..... $C^{19}H^{8}N + 3HO$.

Gerhardt, it is true, noticed already that on heating chinoline, water is separated; he attributed, however, this phenomenon to the decomposition of the chinoline, which, as appears from the above numbers, is not the case. That this decomposition does not occur, is likewise shown by the formation of chinoline at a high temperature, and by the difficulty met with in decomposing this latter body in tubes heated to whiteness. To this must be added, that the anhydrous chinoline, by merely being mixed with cold water, may be converted into the hydrated combination, and the latter, in its turn, by heating, becomes anhydrous again; and, finally, that both, as will be shown in the sequel, furnish complete identical salts and double salts.

It is also this amount of water which accounts for the formula of Gerhardt; because it seems that Gerhardt analysed at that time hydrated chinoline, and afterwards its anhydrous variety, in which case he found, first, less carbon, more hydrogen, and oxygen.

HYDROCHLORATE OF CHINOLINE.

If dry hydrochloric acid be passed over hydrated or anhydrous chinoline, it is violently absorbed, the chinoline thereby becoming very hot. If, however, it be artificially cooled, the whole mass soon solidifies into a white crystalline salt, which absorbs an additional quantity of hydrochloric acid, becomes red, and liquifies; on cooling, this

combination forms radiated crystals, which, attracting moisture from the air, are apt to fall into a state of deliquescence.

Chinoline seems, therefore, to form two combinations with hydrochloric acid, the one neutral, and the other acid; the latter is, at any rate, of a strong acid reaction. Both combinations, at the common temperature, are highly volatile, owing to which, the hydrochloric acid gas passing over it incessantly carries off small portions of it, wherefore this combination is less suited for determining the atomic weight.

CHLORIDE OF PLATINUM AND CHINOLINE.

If to the aqueous solution of the hydrochloric combination an equally diluted solution of chloride of platinum be added, a dense golden-yellow crystalline precipitate is obtained, which is sparingly soluble in cold water, but readily soluble in it when boiling. On cooling, the chloride of platinum and chinoline thus obtained separates in five clusters of prismatic crystals. By repeated crystallisation the last trace of chloride of platinum and ammonia is easily removed, this latter body being first separated, and is never absent, if raw chinoline, as furnished by the first preparation, be employed in the preparation of the platinum salt.

On igniting the double salt there were left from the uncrystallised precipitate—

I. 0·0655 grammes 0·0183 of platinum = 28·17 per cent.

II. 0·0772 grammes 0·0217 of platinum = 28·11 per cent.

This precipitate having been again produced in larger quantities by the decomposition of the crystalline hydrochloric combination,* then once recrystallised and dried at 212°F., gave—

III. 0·8500 grammes 0·2400 platinum = 28·23 per cent.

IV. 0·6810 grammes 0·1930 platinum = 28·34 per cent.

The salts of the two latter analyses twice recrystallised gave—

V. 0·2013 grammes 0·0850 platinum = 28·21 per cent.

As a mean of the three latter analyses are calculated for chloride of platinum and chinoline, and anhydrous chinoline the following atomic weights:—4333·0 and 1765·7.

On combustion with chromate of lead in the elementary analysis were furnished—

I. 0·3242 grammes of chloride of platinum and chinoline, 0·390 of carbonic acid, and 0·0790 of water.

II. 0·3175 grammes burned with oxide of copper gave 0·3890 of carbonic acid, and 0·0810 of water.

III. 0·2770 grammes gave 0·3385 of carbonic acid, and 0·0665 of water.

To determine the amount of nitrogen by the method of Warrentrapp and Will, gave—

I. 0·4355 grammes chloride of platinum and chinoline, 0·2730 chloride of platinum and ammonia = 3·98 per cent. of nitrogen.

II. 0·3860 grammes gave 0·2560 chloride of platinum and ammonia = 4·21 per cent. of nitrogen.

III. 0·4375 grammes 0·2757 chloride of platinum and ammonia = 4·00 per cent. of nitrogen.

If all the above results be reduced to hundredths, and, for simplicity's sake, those that correspond be combined, then for the composition of this platinum compound the following table is obtained:—

		Calculated.		Found.		
				I.	II.	III.
19 equivalents	Carbon....	1452·3	33·76	33·78	33·89	33·80
9 ,,	Hydrogen..	112·3	2·61	2·71	2·83	2·68
1 ,,	Nitrogen ..	177·0	4·11	3·98	4·21	4·00
3 ,,	Chlorine ..	1328·0	30·86	31·19	30·73	30·71
1 ,,	Platinum..	1233·3	28·66	28·34	28·34	28·81
1 equiv. Chloride of Platin. & Chinoline		= 4302·9	100·00	100·00	100·00	100·00

CHLORIDE OF MERCURY AND CHINOLINE.

If to a somewhat concentrated solution of hydrochlorate of chinoline be added a solution of chloride of mercury, a fine white precipitate of chloride of mercury and chinoline is thrown down, which, on being heated, is very easily dissolved in the supernatant liquid, and, after cooling, separates in fine crystalline scales, of the lustre of mother-of-pearl. This combination is decomposed at a low temperature, becoming thereby at first reddish, and afterwards black. Its solution is of a most disagreeable bitter metallic taste, and

* The hydrochloric combination was prepared from the anhydrous, hence the distilled, chinoline.

possesses, as all other combinations of chinoline do, the characteristic odor of the latter-named body.

On comparing the above-communicated composition of chinoline with quinine and cinchonine, of which the former was prepared, it appears that quinine to be converted into chinoline has to lose only the elements of one atom of carbonic acid and four equivalents of hydrogen; whereas cinchonine requires the addition of one atom of water, by means of which it is likewise resolved into carbonic acid and chinoline, hydrogen being disengaged; this action may be rendered clearer by the following equation:—

Quinine ..... $C^{20}H^{12}NO^2 = CO^2 + C^{19}H^8N + H^4$
Cinchonine ..... $C^{20}H^{12}NO + HO = CO^2 + C^{19}H^8N + H^5$;

whereas Gerhardt in his last treatise communicated as follows:—

Quinine ..... $C^{20}H^{12}NO^2 = CO^2 + C^{19}H^{10}N + H^2$
Cinchonine ..... $C^{20}H^{12}NO + HO = CO^2 + C^{19}H^{10}N + H^8$;

according to which latter the anhydrous chinoline contains two equivalents of water more than I obtained in all my highly-concordant analyses, in three of which the combustions were conducted with the utmost accuracy, and the platinum salt carefully dried; wherefore I may be allowed to hope that I have established in the foregoing the real composition of anhydrous chinoline.

Notwithstanding all attempts to convert cinchonine into quinine have hitherto been fruitless, yet the products of the decomposition of the two alkaloïds by caustic potassa being similar, the formation of chinoline from both, and the great concordance of their constitution, remove all doubts as to their internal relation; and we may indulge the hope, so important in a theoretical as well as in a practical point of view, finally to succeed in converting the lower stage of cinchonine into the higher one of quinine.

—nt—

ON A NEW TEST FOR BILE AND SUGAR.

BY DR. PETTENKOFER.*

In the winter of 1843-4, when under the direction of professor Scherer, in his chemico-clinical laboratory at Würzburg, engaged in decomposing the bile, I noticed a phenomenon which I think may be recommended as a re-agent for cholinic acid and sugar. When ox-bile, to which sugar had been added was precipitated by concentrated sulphuric acid, and this latter body was added until the cholinic acid having become very hot began to be re-dissolved, the liquid assumed a deep violet color, resembling that of hypermanganate of potassa. At first, I was inclined to ascribe this beautiful and striking change of color to a decomposition of the coloring matter of the bile; but I found that this change is likewise effected, and even more perfectly, with bile previously rendered colorless; and with the pure bilin of Berzelius. As is known, cholinic acid, or sugar, treated simply with concentrated sulphuric acid, produces no such phenomenon, and we learn from this interesting instance, to what an extent this change may take another turn, if two organic substances put together are simultaneously decomposed by one and the same agent, yielding a product totally different to that which is obtained when they are acted upon singly. In this point of view, it would have been of the highest importance to me to isolate the new product of decomposition, and to ascertain its constitution; but I regret that my attempts and endeavors to obtain a pure preparation have hitherto been unsuccessful, and to conclude from the change of color, this body is rather a transient phenomenon. I therefore content myself with describing my observation as a means of recognizing—

1. The bile (cholinic acid.)
2. Sugar.

With reference to cholinic acid, it has hitherto been very difficult to detect it, and the proofs given of its presence are generally but equivocal; and when the bile has once left the intestinal tube, in most cases, it can no longer be detected at all. But little more has generally been attempted than to demonstrate the presence of the coloring matter of the bile by concentrated sulphuric acid, and to suppose wherever coloring matter of the bile was found, the principal constituent of the bile, cholinic acid, must likewise be present. That this, however, is not invariably the case appears from the circumstance, that the coloring matter of the bile is evacuated with the feces; whereas, the bile itself is reabsorbed, as in feces of healthy individuals all properties of bile hitherto known have almost always yielded a negative result. To detect bile, I propose the following treatment: — When a liquid is suspected to contain bile, a small quantity of it is to be thrown into a little test-tube, adding, drop by drop, about two-thirds of its volume of sul-

* Liebig's *Annalen der Chemie*, vol. x, Nov. 30, 1844.

phuric acid, by which the temperature is considerably raised. The additions must be made gradually so as not to raise the temperature above 122° F., to avoid carrying too far the alteration produced in the cholinic acid. After this from 2 to 5 drops of a solution of common cane sugar, containing for each part of sugar 4-5ths parts of water, are added, and the whole liquid is shaken. When cholinic acid is present, the violet-red color becomes more or less apparent, according to the quantity of bile contained. When solid or pulpy masses are examined, in order to ascertain whether they contain bile, they may be extracted by alcohol; the extract evaporated until it is reduced to a small volume, and then treated as above.

In this operation, certain precautions must be adopted.

1. The temperature ought not much to exceed the degree stated above, as in the contrary case, this would tend rapidly to destroy the color, if ever it were produced.

2. The quantity of the sugar employed ought not to be great, because in that case the sulphuric solution would exhibit a deep black-brown color, or sulphuric acid might be formed, by which the violet-red color would be disguised or destroyed.

3. For the same reason, the sulphuric acid employed in the reaction must be free from sulphurous acid (the presence of arsenious acid does not interfere with the reaction.)

4. If the liquor contain albumen, to insure success it is advisable to remove it by coagulation, because solutions of albumen, even though in a very concentrated state, only when heated with sugar and sulphuric acid, produce a similar coloration. I never observed a similar fact with mucus, nor with solutions of albumen in a somewhat diluted state, which latter invariably became brown.

5. A very great excess of combinations of chlorine, but to an extent in which it never occurs in the organism, may change the color into a brownish-red.

6. If the solution containing the bile is too diluted, it is advisable to concentrate the liquid, by cautiously evaporating it in a water-bath, then to extract this with alcohol, bringing it back again to small bulk by evaporation, and to expose the cooled liquid to the reaction. This sometimes takes place only after the lapse of several minutes, especially when the sulphuric acid is very gradually added, and, therefore, a lower temperature produced.

In the cases of liquids in which but little bile occurs, as generally in urine, exudations, &c., to render the proofs more positive, I found it necessary to make an alcoholic extract, to evaporate this in a water-bath almost to dryness, and to put it in this half-soft state on a watch-glass. To the entirely refrigerated mass to be tested, a very little solution of sugar is added (most appropriately by a glass-rod), in such a manner that the whole may become but moderately hot —which effect is favored by the great surface of the watch-glass—and it is allowed to repose for some minutes. When only a trace of bile is present a coloration will take place. If, in addition to this, other colored substances be present, then to be enabled better to observe the experiment, the liquid may, after the reaction has occurred, be poured in a little test-glass to view it the moment the rays of light pass through, which, however, the intensity of the violet-red color being considerable, will seldom become necessary. A few preliminary experiments will suffice to render these relations more clear to any chemist, and to convey the necessary facility for executing it.

Concentrated hydrochloric acid, treated with bile and sugar, shows the same red coloring; but this is far less beautiful and intense, than when treated with sulphuric acid.

In this reaction, instead of cane-sugar, grape-sugar, fecula—in short all substances, which on being treated with concentrated sulphuric acid are converted into grape-sugar—may be employed. In this manner were treated the bile of man, the fox, the dog, neat cattle, swine, cock, frog, carp; the result being invariably the same. It therefore appears to me corroborated by facts, that the bile of the vertebrata is in its essential constituent parts the combination of cholinic acid with soda, throughout chemically the same, as albumen is always the same.

It was interesting to me to discover, by means of this reaction, the bile excreted with urine in cases of pneumonia. The quantity of cholinic acid, so far as it could be estimated by the more or less intense coloration produced by the said reaction, seemed to keep pace with the hepatisation of the lungs. It becomes manifest that, when the respiration is embarrassed, the bile in the organism undergoes an incomplete change, and it enters into the urine in an only partially decomposed state. If the obstruction in the lungs is of a longer standing, we find the products of the decomposition of cholinic acid, the coloring matter of the bile, in the blood and the urine; by the pneumonia icters is called forth. These cases are of frequent occurrence in medical practice.

The feces of a healthy man, extracted with alcohol in the said manner, exhibited not the slightest reaction, whereas it was completely produced, when a small quantity of

bile was added to the feces, previous to experimenting.

Some observers have already noticed that when the so-called calomel openings are mixed with concentrated mineral acids, their green or green-yellow color is converted into red. I had myself on previous occasions often remarked this phenomenon, the effect being most beautiful, when sulphuric acid was employed. I could not then account for the phenomenon; but I now think I am aware of its cause. The rapid passage of the chyme through the intestinal canal allows, under these circumstances, but little to be absorbed. Hence, in the evacuations, bile, remains of sugar, and of meat and drink partaken, must be found. And when they are treated with concentrated sulphuric acid, the described reaction must take place. In every case of diarrhœa bile is found in the evacuations. The interest of this fact in a pathological and therapeutical point of view is evident. By the administration of purgatives, we are enabled to remove the greatest part of the bile secreted during a certain time, whereas, otherwise, it passes into the blood, giving rise therein to further changes. The alterative property of purgatives may, perhaps, find its explanation in this fact.

As all reagents are of a mutual employment, baryta for sulphuric acid, sulphuric acid for baryta, so are cholinic acid and sugar. If we suspect that a certain liquid, for instance urine, contains sugar, then we gradually add concentrated sulphuric acid to an aqueous solution of ox-bile, until the precipitate of cholinic acid first formed is redissolved, and throw a little of it into the urine under examination, when, if sugar be present, the liquid rapidly assumes a violet color. The quantity of sugar requisite for that effect, being inconsiderable, it will rarely be necessary to concentrate the urine by evaporation.

When blood has to be examined in order to ascertain whether it contains sugar, the albumen, &c., is removed by boiling with alcohol—and the concentrated mass is treated as above.

In this test for sugar, however, to be certain of the result, we must first ascertain the absence of fecula, by iodine, owing to the same reaction being obtained by fecula. If this latter substance be found, the presence of sugar remains doubtful. Such a combination, however, will scarcely ever occur in the testing of blood or urine for sugar. To attain this end, I prefer my reaction to that effected by copper salts with potassa: first, because its effect is more delicate and more speedily produced; and, secondly, because the ammoniacal combinations of the urine cannot interfere with the reaction; the ammonia, on becoming free, retains the oxide of copper, as well as the protoxide of copper in solution, until it is completely driven off by boiling, whereas a reduction of the oxide of copper may easily be caused by other substances.

Pure mannite and bile, treated in the above manner, exhibit no trace of the described reaction. By this means it is easy to demonstrate in manna the presence of grape sugar. —nt—

ON THE ACTION OF FUSED HYDRATE OF POTASSA ON CANE SUGAR, GUM, STARCH, AND MANNITE.*

BY DR. I. GOTTLIEB.

The action of hydrate of potassa on several organic substances, was some time ago investigated by Gay-Lussac, whose attention was chiefly directed towards the formation of oxalic acid, and who in vinic acid showed likewise the production of acetic acid. The discovery of Pelouze, that cane sugar, under certain circumstances, may be resolved into butyric acid, hydrogen, carbonic acid, and water; and the observation of Professor Redtenbacher,† that, on burning greater quantities of sugar, an odor becomes manifest, much resembling that of acroleïne, induced me to examine in his laboratory, by means of fused hydrate of potassa, the products of the decomposition of sugar, and other substances relating to it.

At the first glance it seemed probable, that, besides a simultaneous formation of several other products, butyric or acrylic acid will be produced. The results obtained at first, confirmed me in the latter part of the above supposition, for, on the action of cane sugar, I obtained an acid, the odor of which greatly resembling that of acrylic acid, its combination with oxide of silver having almost wholly the same properties as acrylate of silver. Its composition, however, materially differs from that of acrylic acid, and by communicating in the following the result of its investigation, I may remark, that I mostly employed cane sugar in its preparation, but not without having repeatedly ascertained that starch, gum, and mannite furnish the same products. If a concentrated solution of potassa is evaporated until it solidifies on cooling, and gradually, the application of heat being continued, be brought in contact with about three parts of hydrate of potassa, to one of sugar, the mass becomes brown,

* Liebig's *Annalen der Chemie*, vol. x., 30th November, 1844.

† *Ibid*, vol. xlvii. p. 148.

under a continued disengagement of hydrogen, and the odor of caramel is clearly perceived, which soon disappears, being replaced by a more aromatic odor. The most violent action ceases after some minutes, the mass becomes dense, frothing continually, until the brown coloration disappears, when the whole becomes rather solid, and the operation may be considered as brought to a close. After cooling, the saline mass is of a light yellow color. It is to be dissolved in a little water, and treated with an excess of moderately diluted sulphuric acid, whereupon, disengaging a great amount of carbonic acid, it becomes very hot, which, however, must be as carefully as possible avoided. At the same time a copious precipitate of binoxalate of potassa is thrown down. The liquid filtered off is distilled in a retort. The distilled mass is strongly acid and contains formic acid, acetic acid, and a new acid, which, for reasons to be stated in the sequel, I call metacetonic acid. To remove the formic acid, it is most advisable, to boil the mixture with an excess of oxide of mercury, so long as oxide of mercury is disengaged, whereupon the salts of protoxide and suboxide of mercury formed, are decomposed by sulphuretted hydrogen, and the two acids are saturated by carbonate of soda. I could not separate the acetic acid from the metacetonic acid by any other process, than by the crystallisation of its soda salts, and even this method is liable to several difficulties, and especially as the acetic acid always constitutes the greatest portion of the distilled mass, and it is always necessary to operate with from one to two pounds of cane sugar, to obtain only small quantities of pure salt of soda. Metacetonic acid is so nearly related, in its properties, to acetic acid, that I was unable to find out in their salts any material difference on which an easier method of separation could have been based. Their silver salts possess almost a similar degree of solubility; mixtures of metacetonate and acetate of copper, zinc, baryta, &c., do not crystallise, and in addition to this, both acids have a great tendency to form in combination with the same base, double salts. With the silver salt this occurs even though the acetate be present in great excess. The metacetonate being most difficultly crystallisable, whereas, with the acetate of soda, the contrary is the case, we very soon arrive at the point, in which the greatest portion of the acetate of soda is removed; but it is the more difficult, however, to separate its last portions from the metacetonate, as in the mother liquor, which becoming almost of the consistence of syrup, a manifest crystallisation is never obtained. In this case it answers best to dilute a little, and by a very gradual evaporation, to remove the last traces of the acetate of soda. If, then, after a longer time no crystals of acetate of soda appear, the salt is pure. To ascertain the composition of metacetonic acid I employed the silver salt. To prepare this a solution of metacetonate of soda moderately concentrated is treated with nitrate of silver, so long as a dense white precipitate is thrown down; the whole is boiled, by which a small portion becomes reduced, and is rapidly filtered. On cooling, the metacetonate of silver separates in shining, white, heavy grains, which, under the microscope, appear under the form of small *druses* of short crystalline needles. By the evaporation of the mother liquor an additional small quantity of the salt may be obtained.

The metacetonate of silver is not much influenced by light, and may be exposed to that agent for weeks without any particular change in its color being perceptible. At 212° F., however, it soon becomes black-brown, and is partially decomposed, wherefore it must be dried over sulphuric acid *in vacuo*. Covered by a strong mineral acid, it disengages the odor proper to metacetonic acid, which at the same time resembles that of butyric and acrylic acids. Heated in a porcelain crucible it fuses smoothly, and burns without producing any noise, in which it materially differs from acrylate of silver.

The analysis gave the following results:—

I. 0·2955 grammes left 0·176 grammes of silver, which corresponds to 0·189 oxide of silver.

0·556 grammes, on being burned with oxide of copper, gave 0·3995 grammes of carbonic acid, and 0·1375 grammes of water.

II. 0·290 grammes left 0·172 grammes of silver = 0·1847 oxide of silver.

0·5305 grammes gave 0·3805 of carbonic acid, and 0·135 of water.

This leads to the following formula, and per centage composition:—

		Calculated.		Found.	
				I.	II.
6 equiv.	Carbon	455·12	20·05 ..	19·76	19·72
5 ,,	Hydrogen	62·39	2·74 ..	2·74	2·82
3 ,,	Oxygen	300·00	13·24 ..	13·54	13·77
1 ,,	Oxide of Silver	1451·60	63·97 ..	63·96	63·69
1 atom Metacetonate of Silver		2269·11	100·00	100·00	100·00

The substances of the two analyses are derived from various preparations. From this the formula of the silver salt is developed as follows: $C^6 H^5 O^3 + Ag O$; hence the hydrated acid is $C^6H^5O^3 + Ag$.

In the series of acids, therefore, the hydrates of which contain the carburetted hydrogen, CH, metacetonic, acid occupies a place between butyric and acetic acid. It is soluble in water in all proportions, possessing, as stated above, a peculiar odor, and a somewhat concentrated strong sour taste. The small quantities of it which I had at command did not permit me to study its properties more fully.

I have already mentioned the double salt formed by metacetonate of silver and acetate of silver. This is invariably produced when the original mixture of acetic and metacetonic acids is boiled with oxide of silver and filtered, or when a mixture of acetate and metacetonate of soda is decomposed by nitrate of silver. It separates from the hot solution in fine, shining, dendritic crystals, which resemble neither metacetonate nor acetate of silver.

The filtered salt is light, spongy, and may be dried at 212° F., without undergoing decomposition.

Its analysis gave the following results:—

I. 0·4105 grammes left 0·2544 grammes of silver=0·2728 grammes of oxide of silver.

0·699 grammes gave 0·450 grammes of carbonic acid, and 0·150 grammes of water.

II. 0·5945 grammes left 0·3665 grammes of silver=0·3936 grammes of oxide of silver.

0·688 grammes gave 0·445 grammes of carbonic acid and 0·1485 grammes of water.

This leads to the following formula and per centage composition:—

			Calculated.		Found.	
					I.	II.
5	equivalents	Carbon	379·27	17·39	17·70	17·78
4	,,	Hydrogen	49·91	2·28	2·38	2·39
3	,,	Oxygen	300·00	13·75	13·47	13·62
1	,,	Oxide of silver	1451·60	66·58	66·45	66·21
			2180·18	100·00	100·00	100·00

The formula $C^5 H^4 O^3 + Ag O$ multiplied by two gives—

$C^{10} H^8 O^6 + 2 Ag O = (C^4 H^3 O^3 + Ag O) + (C^6 H^5 O^3 + Ag O)$, from which it follows, that the salt is composed of one atom of acetate of silver, and of one of metacetonate of silver. This is likewise proved by the circumstance, that when the double salt is decomposed by chloride of sodium, and the newly formed soda salt is allowed to crystallise, pure acetate of soda is immediately formed.

I obtained, likewise, on one occasion, the double salt of soda; however, it was not always in my power to produce it. It forms five shining needles, very similar to those of acetate of soda, is very soluble in water, and contains water of crystallisation. The small quantity which I had at command allowed of my determining only the amount of water and soda.

0·3045 of dry salt, on being burned in a platinum crucible, left 0·1825 grammes of carbonate of soda, corresponding to 0·1069 soda. This gives in 100 parts 35·13 per cent of soda. The formula $C^5 H^4 O^3 + NaO$ requires 34·90 of soda.

0·8455 grammes of salt dried in the air lost at 212° F. 0·258 grammes of water, which corresponds to 33·55 per cent. The formula $(C^{10} H^8 O^6 + 2 NaO) + 9$ Ag. requires 31·12 per cent. water. Metacetonate acid readily combines with ether, and the metacetonate of oxide of ethyl is obtained without difficulty, if the metacetonate of silver be boiled with sulphuric acid and absolute alcohol, and then mixed with water, when the ether separates as a light liquid, which possesses an agreeable fruity odor, having, however, no resemblance to that of the butyrate of oxide of ethyl. Its other properties could not be investigated.

On considering the formula of metacetonic acid, it appears that it may be regarded as an oxide of acetone as well as of the metacetone of Frémy, because one atom of acetone $= C^6H^6O^2 + 2O = C^6H^5O^3 + Ag$, that is, hydrated metacetonic acid, or one atom metacetone $= C^6H^5O + 2O$ is equal to $C^6H^5O^3$, that is anhydrous metacetonic acid. The treatment of cane sugar, &c., with potassa is likewise in a great many respects analogous to the process by which Frémy, in heating sugar, gum, and starch with lime, obtained acetone and metacetone. The supposition, therefore, became very palpable, that in the described reaction metacetonic acid owes its origin to a previous formation of acetone and metacetone, which was afterwards more highly oxidised by the water of the hydrate of potassa, and formed metacetonic acid. It has been proved by experiments, instituted by myself on that score, that metacetonic acid originates from metacetone; that acetone, however, being oxi-

dised by hydrate of potassa, is converted into acetic and formic acids.

To obtain this end, I prepared greater quantities of acetone and metacetone, adopting with reference to the first, the method of Zeise, and to the latter that of Frémy. In both cases the operation is carried on with much greater advantage, if, instead of the prescribed proportion of lime and sugar, about one-half of the former be employed, the process advancing more rapidly, and the product obtained being more abundant. The preparation of the metacetone, in particular, may succeed well and easily, if to one portion of sugar only, three parts of lime are employed, and the mixture (most advantageously 10lbs. to 15lbs. at one time) be gently heated in a retort provided with a proper cooling apparatus, which may be done without injuring the retort. As already stated by Frémy, the reaction is very soon communicated to the whole mass, and a brown liquid distils over, which, by repeated rectifying and washing with water, is freed from all impurities. As it is easy, in this manner, to prepare several ounces of metacetone, it may be purified without difficulty.

When metacetone is allowed to fall drop by drop on fused hydrate of potassa, almost the whole of it distils off, without undergoing any other change, and in this manner only traces of a volatile acid are obtained. And it is difficult to produce metacetonic acid by the action of caustic potassa on metacetone; because, if the heat be too great, carbonisation easily occurs, and if the temperature be too low, the metacetone distils over unchanged. In all the experiments which I undertook, I obtained only small quantities of metacetonic acid, but neither acetic nor formic acid. The metacetonic acid, however, is obtained most rapidly and copious, if the metacetone be oxidated with a mixture of sulphuric acid and chromate of potassa. The reaction is at first violent, and must be undertaken in a large phial which is connected with a cooling apparatus. A large quantity of carbonic acid is disengaged, the whole becomes very hot, during which time metacetone invariably distils over in an unchanged state, and, at the close of the operation, it becomes necessary to promote the reaction by applying heat. When the disengagement of gas ceases, the receivers are exchanged, and the acids formed are distilled over. These are the acetic and metacetonic acids, which are saturated with carbonate of soda, and separated by the crystallisation of the soda salts. From the metacetonate of soda thus obtained, I prepared a silver salt, the properties of which perfectly coincide with the metacetonate of silver produced by the action of hydrate of potassa on sugar, gum, &c. Its analysis gave the following results:—

0·2215 grammes left 0·132 of silver = 0·1417 grammes of oxide of silver.

0·7435 grammes, on being burned with oxide of copper, gave 0·5345 grammes of carbonic acid, and 0·188 grammes of water.

From this, the above given formula and per centage composition of the metacetonate of silver is developed:—

6 equivalents	Carbon	455·12	20·05	12·76
5 ,,	Hydrogen	69·39	2·74	2·80
3 ,,	Oxygen	300·00	13·24	13·44
1 ,,	Oxide of Silver	1451·60	63·97	64·00
		2269·11	100·00	100·00

The double salt was prepared with all its properties, and found to be of the same composition as that produced from sugar.

0·360 grammes of substance gave 0·223 grammes of silver = 0·239 of oxide of silver.

0·579 grammes gave 0·3615 grammes of carbonic acid, and 0·1265 of water.

This gives, in 100 parts—

		Calculated.	Found.
5 equivalents	Carbon	379·27	17·39	17·16
4 ,,	Hydrogen	49·91	2·28	2·42
3 ,,	Oxygen	300·00	13·75	13·90
1 ,,	Oxide of Silver	1451·60	66·58	66·52
		2180·18	100·00	100·00

I could not prepare any metacetonic acid from acetone. Dumas* states that by the action of heated caustic potassa on acetone, only carbonic acid and carburetted hydrogen are formed. I cannot confirm this assertion, as in all the experiments in which I passed vapors of acetone upon caustic

* Liebig's *Annalen der Chemie*, vol. xxxv., p. 159.

potassa, after supersaturating it with sulphuric acid and distilling the acid liquor, I invariably obtained a large quantity of acetic and formic acids. Chromic acid, on being treated with acetone, as Dumas found, gave only acetic acid.

It is, therefore, proved that metacetonic acid is really produced, by the oxidation of metacetone; and we may form a clear notion of the action of fused hydrate of potassa on sugar, starch, &c. The sugar, namely, is first resolved into carbonic acid, acetone, and metacetone. The metacetone passes over into metacetonic acid, the acetone, however, into acetic and formic acids, the latter being both partially highly oxidised and produces oxalic acid.

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CHEMICAL INVESTIGATION CONCERNING CERTAIN FUNGI.*

BY DRS. SCHLOSSBERGER AND DOEPPING.

In most works on diet, the fungi occupy a high rank among alimentary matters; and they, therefore, act an important part in therapeutics, as restorative as well as aphrodisiacal remedies. According to a very prevalent opinion, the just stated properties of the eatable fungi are due to the great amount of nitrogen they contain; and, in this latter respect, they are considered as approaching to the animal kingdom. This assertion, however, did not appear to be well-founded on scientific basis, so long as it was not based on direct experiments, plausible as it might have been, on account of several phenomena observed in fungi, especially their extreme tendency to decomposition and putrefaction. After M. Boussingault had, in the determination of nitrogen, established a standard of comparison for the nourishing power of our food, and likewise for the manuring power of organic manures, which is of an easier execution, and yields more positive data than were obtained by former experiments of agriculturists; and after the same chemist had carried the method of comparison through a great series of direct determinations, it seemed to us not without interest to examine, relative to the amount of nitrogen, a number of fungi, of those that are of perennial durations, as well as of those the whole vegetation of which lasts only a few weeks, or only days.

To this end, they were dried at 212° F., and after the quantity of their inorganic constituent parts was determined, they were subjected, according to the process of Will and Warrentrapp, to the determination of nitrogen. This method, however well contrived, labors under the disadvantage that the hydrochloric acid may, especially at the end of the operation, return into the combustion-tube, and thus cause the experiment to miscarry; to obviate which inconvenience, we modified the vessel receiving the acid in the following manner:—to receive the ammoniacal vapors, two phials, as in the apparatus of Fresenius and Will for the determination of carbonic acid, were adapted together, the phial which communicated directly with the combustion-tube was of somewhat larger dimensions; the communication between this phial and the combustion-tube was made by means of a glass tube, bent in a rectangular form, which, after passing through the cork of the phial, was immediately cut off; into the first phial a thin layer of acid was thrown, which scarcely covered the bottom, whereas the main quantity of the acid was contained in the second vessel; the unabsorbed gases could escape from the latter through a long tube, in the middle of which a bulb was blown, to render it impossible for any drops of acid to be carried off. Both phials were closed with thoroughly-washed corks. From this modification of the said apparatus was derived the advantage, that the whole operation could be undertaken with more calmness; towards the close of the operation we could, without concern, allow the liquid of the second phial to rise in the tube, because, even though the whole of its contents were to be evacuated into the first bottle, still, on account of the distance of the liquid from the orifice of the conducting-tube to the combustion-tube, it was impossible that it should remount into the latter. The cleansing of the phials went on with great facility. The sal-ammoniac adhering to the cork was completely washed away by water and alcohol. The sal-ammoniac obtained was throughout determined, in the known manner, by chloride of platinum. Experiments, with the view of comparing them, made with the apparatus as originally contrived by Will, gave quite the same results.

The fungi examined in that way were the following:—*Agaricus deliciosus*, L. (the so-called Reizker or Rothling (orange agaricus).

7·300 grammes of the fresh mushroom after being dried in a water-bath, left 0·961 of solid residue; consequently it consists of

13·1	solid constituent parts
86·9	water
100·00	

* Liebig's *Annalen der Chemie*, vol. x. November 30, 1844.

0·473 of the dried mushroom gave 0·033 ashes (containing iron and manganese) = 6·9 per cent.

0·365 of the dry mushroom gave 0·252 chloride of platinum and ammonia = 4·68 per cent. nitrogen.

Hence, in 100 parts of the fresh mushroom are contained—

0·61 N.
0·90 ashes.

2. *Agaricus arvensis*, Schaeffer, (a variety of champignon).

4·620 fresh mushroom left at 212° F. 0·435; hence, 100 parts of it contained—

9·39 solid substance
90·61 water.

The lamellæ of the inferior surface of the pileus, dried at 212°, gave the following results:—

0·545 left 0·059 of ashes = 10·82 per cent.

0·570 gave 0·582 chloride of platinum and ammonia = 7·26 per cent. N.

100 parts of the fresh lamellæ contain therefore—

0·68 N.
1·01 ashes.

The stalk and the solid parenchyma of the pileus,* somewhat deviated in both respects: 0·634 dried at 212° F. left 0·074 ashes = 11·6 per cent. (these ashes were of a strong alkaline reaction, almost completely soluble in water; and likewise contained manganese.)

0·427 grammes gave 0·495 chloride of platinum and ammonia = 8·3 per cent. N. In 100 parts of the fresh stalk are therefore—

0·77 N.
1·08 ashes.

3. *Agaricus glutinosus*. 6·901 grammes of the fresh mushroom left, at 212° F., 0·434 solid residue; in 100 parts there are therefore—

6·29 solid substances
93·71 water.

0·888 grammes of dry residue gave 0·043 ashes = 4·8 per cent.

0·854 grammes of the dry residue gave 0·5915 of chloride of platinum and ammonia = 4·61 N.

In 100 parts of the fresh mushroom—

0·22 N.
0·30 ashes.

4. *Agaricus russula*, Scop.

6·764 grammes on being dried, furnished 0·599 solid residue; hence, in 100 parts—

8·8 solid substance
91·2 water.

0·902 grammes of the residue, dried at 212° F., left 0·086 of ashes = 9·5 per cent.

0.520 grammes of the residue, dried at 212°F., gave 0·315 chloride of platinum and ammonia = 4·25 per cent.

In 100 parts of the fresh mushroom are therefore—

0·37 N.
0·83 ashes.

5. *Agaricus cantharellus*, L. (*Cantharellus cibarius*, Fr.; Gelber, Pfifferling and Eierschwamm.)

5·713 grammes of the fresh mushroom, at 212° F., gave 0·540 dry residue; hence, in 100 parts—

9·4 solid substance
90·6 water.

1·028 grammes of dry residue gave 0·116 ashes = 11·2 per cent.

0·705 grammes of dry residue, gave 0·318 of chloride of platinum and ammonia = 3·22 per cent. N.

100 parts of the fresh mushroom therefore contain—

0·30 N.
1·05 ashes.

6. *Agaricus muscarius* (*Fliegenschwamm*).

(*a*) The whole substance of the mushroom: 7·632 of fresh substance, dried at 212° F., left 0·721 grammes; in 100 parts therefore are—

9·44 solid substance
90·56 water.

1·340 grammes of the just-mentioned residue left 0·121 ashes = 9·0 per cent.

The incineration in this case as well as in those of most fungi examined by us, was of very difficult execution, and without the addition of a few drops of nitric acid, we could not completely succeed in bringing it about; the ashes were rich in phosphates, and contained a considerable quantity of manganese.

0·455 grammes dry substance gave 0·381 of chloride of platinum and ammonia = 6·34 per cent. N.

Hence, 100 parts of the fresh mushroom contain—

0·598 N.
0·849 ashes.

(*b*) The red cuticle of the pileus gave the same quantity of ashes.

0·575 grammes of its dried substance yielded 0·579 of chloride of platinum and ammonia = 7·02 per cent. N.

7. *Boletus aureus*, Sch. (*Boletus luteus*, L.)

10·732 grammes, dried at 212°, left 0·618 solid residue.

100 parts therefore contain—

5·65 solid substance
94·25 water.

* The extremely beautiful red coloration produced by the action of concentrated sulphuric acid, most conspicuous in its cuticle, is worthy to be noticed.

0·661 grammes of dry residue furnished 0·015 ashes = 6·80 per cent.

0·417 grammes of dry residue furnished 0·310 chloride of platinum and ammonia = 4·7 per cent. N.

In 100 parts of the fresh boletus are therefore—

0·26 N.
0·38 ashes.

The cuticle of this boletus was dried by itself: 0·419 of solid residue gave 0·299 of chloride of platinum and ammonia = 4·38 per cent. N.

8. *Lycoperdon echinatum.*

0·681 grammes of substance, dried at 212° F., gave 0·036 ashes = 5·2 per cent.

0·424 grammes gave 0·399 chloride of platinum and ammonia = 6·16 per cent. N.

9. *Polyporus fomentarius*, dried at 212° F., gave 4·828 grammes, 0·149 ashes, 3·0 per cent. 0·4365 grammes furnished 0·298 chloride of platinum and ammonia, which, (after subtracting the amount of ashes) corresponds to 4·46 per cent. N.

10. *Dædalea quercina.* 4·465 grammes substance dried at 212° F. left 0·142 ashes, = 3·1 per cent. 0·550 grammes gave 0·269 chloride of platinum and ammonia = 3·19 per cent.

The aqueous extract becoming turbid on being exposed to the air was soon decomposed, and disengaged fetid gases, among which sulphuretted hydrogen was clearly recognised.

On reviewing the results of the foregoing investigations, we find that the fungi contain more water than any other vegetable which constitutes an article of our food; many succulent fruits can scarcely compete with it in that respect, though they may scarcely be considered as real nourishing substances. This abundance of water accounts for several striking phenomena of these peculiar vegetable bodies; above all their rapid growth, which is proverbially called shooting forth; after one single rain they sometimes, from the smallest size, after a few hours or a day, become plants of the size of a fist; and hence the tendency to decomposition and becoming rotten is greater in fungi than in any other vegetables, as besides a great quantity of water and a simple structure of their solid constituent parts they contain comparatively a very considerable quantity of proteïc substance. With reference to the latter substance, the fungi are distinguished from all vegetables by their quantity of nitrogen, and in this point, besides by their amount of water, they surpass, by far, our vegetable nourishing substances. Those of the above mentioned fungi, which contain the least amount of nitrogen, approach to vegetable substances the richest in nitrogen, peas and beans, the nitrogen of which, according to Boussingault, amounts to 3—5 per cent. Most fungi dried at 212° F. double or treble the amount of nitrogen found in wheat.

We regret not being in the possession of a sufficient quantity of matter to make more detailed analyses of some rapidly growing fungi; but in all the fungi investigated, on incineration to determine the total amount of the constituent parts of the ashes, a richness in phosphates was distinctly observed, which, therefore, in them is likewise in a direct ratio with the abundance of proteïc substances. According to this, scientifically considered, we may now establish only one view, which hitherto was only based on probability—viz., that fungi contain a *considerable nutrient power* in the strictest sense of the word, and may particularly contribute considerably towards the direct formation of blood. We received in our series some *poisonous* fungi; we did so, first, because, perhaps, with little trouble, we may succeed in converting them into nutrient principles (by a suitable method of preparation, or an addition of spices); and next, on account of their richness in nitrogen, owing to which, where they are found in great quantities, they may become valuable organic manure, and compete with guano, which dried at 112° F., according to Boussingault, contains only 6 per cent. N. But on that point agricultural experiments alone can decide.

With reference to the substratum of the fungi the *fibres*, the *fungin*, described by Braconnot and others, has since been struck out of the list of nutrient vegetable principles, Payen, and, after him, Fromberg, having shown that the fibre of fungi, when properly purified, completely coincides with *cellulose*. Payen found thus—for the fibre of—

Agaricus etulis.	*Boletus ignarius.*
C 44·52	C 43·40
H 6·67	H 6·11
O 48·81	O 50·49

Fromberg obtained from the fibre of *Agaricus albus*, after having boiled it with with water, and extracted with a weak solution of soda, hydrochloric acid and alcohol—

C 45·57
H 6·29
O 48·14;

and after a second treatment with soda, hydrochloric acid, and alcohol, 44 per cent. carbon. In the fungi which we analysed, we had an ample opportunity of corroborating these results. The following instances may be adduced as proofs:—

Polyporus fomentarius, cut into shreds, then extracted, first by cold, and afterwards by hot water, a diluted solution of potassa,

by hydrochloric acid and alchohol, left a substance which, dried at 212° F., gave the following results:—

I. 0·553 grammes, on being burned with chromate of lead, gave 0·917 CO^2 and 0·340 HO.

II. 0·430 grammes gave 0·709 CO^2 and 0·265 HO.

I.	II.
C 45·42	C 45·34
H 6·80	H 6·84
O 47·78	O 47·82
100.00	100·00

After long and continued boiling, we succeeded only in converting, by means of dilute sulphuric acid, the purified fibre into grape sugar.

Dædalea quercina. 0·445 grammes of the raw substance of the pileus dried at 212° F., on combustion with Cu O, gave 0·795 CO^2 and 0·274 HO. On adding the above determined nitrogen, and subtracting the ashes, this gives—

C	50·60
H	7·00
N	3·19
O	39·21

By treatment with various extracting substances the pure cellulous matter was then prepared from the substance of the pileus.

I. 0·517 grammes of this cellulose, on combustion with oxide of copper and chlorate of potassa, gave 0·857 CO^2 and 0·292 HO (no nitrogen).

II. 0·5935 grammes gave, on combustion with chromate of lead, 0·981 CO^2 and 0·342 HO.

I.	II.
C 48·58	C 45·46
H 6·27	H 6·36
O 48·15	O 48·18
100·00	100·00

The light brown, half-mouldy oak-tree wood, on which this fungus was grown, on subtracting the earthy constituent parts, gave the following results:—

I. 0·509 grammes, on combustion with Cu O and chromate of potassa, yielded 0·984 CO^2 and 0·271 HO.

II. 0·530 grammes, on being burned with chromate of lead, yielded 1·018 CO^2 and 0·294 HO.

I.	II.
C 53·16	C 52·82
H 5·91	H 6·16
O 40·93	O 41·02
100·00	100·00

The nitrogen of this wood amounted to about ¼ per cent. Gay-Lussac found in oak-wood, after it was extracted with water and alcohol 52·53 C, and 47·47 H and O, in the same proportion as in water. Meyer found in mouldy oak-wood 53·36 C and 46·44 H and O (*Vide* Liebig's *Agricultur Chemie*, 5th Aufl., p. 437).

It appeared to us remarkable, that in the mouldy wood on which the dædalea was gathered, scarcely any traces of phosphates were found; whereas in the parasitic fungus they were met with in considerable quantities. This case, it seems, is parallel with that observed by Will and Fresenius, in which, in the misletoe which grew on an apple-tree, a great amount of phosphates occurred, whereas, in the tree itself, but little of the said salts could be found.

The raw substance of a *Polyporus destructor* which we found on a half-mouldy poplar-tree, behaved in a peculiar manner. After being dried, it could be reduced to an almost white powder by rubbing it, and contained much less nitrogen than other even perennial fungi, namely, less than 1 per cent.

1·680 grammes substance, dried at 212°F., left 0·072 ashes = 4·2 per cent.

The raw substance, dried at 212°F., gave, on substracting the ashes, the composition of the cellulose, viz.:—

I. 0·408 grammes yielded 0·639 CO^2 and 0·227 HO.

II. 0·407 grammes yielded 0·638 CO^2 and 0·231 HO.

I.	II.
C 43·06	C 43·16
H 6·18	H 6·30
O 50·76	O 50·60
100·00	100·00

After the fungus was exhausted by several extracting substances, a white matter was left as residue.

I. 0·9095 grammes of the same gave 1·453 CO^2 and 0·548 HO.

II. 0·518 grammes of the same gave 0·827 CO^2 and 0·311 HO.

I.	II.
C 43·93	C 43·90
H 6·69	H 6·62
O 49·38	O 49·48
100·00	100·00

The half-mouldy poplar-tree wood on which the polyporus grew, we subjected likewise to an elementary analysis.

0·995 grammes of wood, dried at 212°F., left 0·022 ashes = 2·2 per cent.

0·214 grammes, on being burned with CuO and chlorate of potassa, yielded 0·383 CO^2 and 0·124 HO, besides traces of nitrogen.

Hence —

C	50·15
H	6·43
O	43·42
	100·00

It seems, therefore, that in this case, the cellulose of the poplar-tree wood had passed into the fungus, in a state almost unaltered; whereas, the lignin, which is richer in carbon, was not received by the fungus.

With reference to the amount of fecula of the fungi, we did not succeed in producing a blue coloration in any of the examined specimens, by merely touching them with tincture of iodine; in some, for instance, in the expressed juice of the *Cantharellus cibarius*, we discovered some grains, which turned blue with iodine; but a great many other grains, near the above, in size and form somewhat similar to grains of starch, were noticed, which with iodine became of an intense yellow color; in some few spots the iodine produced green stains, which may perhaps be attributed to a mixture of the blue and yellow grains.

In all the fungi examined by us, besides mannite, we could recognise fermentable sugar. The observation was interesting, that several of the succulent fungi (for instance, agaricus, russula, cantharellus, and emeticus), when kept in a bottle with a narrow aperture, and left for some days, *passed spontaneously into alcoholic fermentation*; offering then a very agreeable smell of new wine not fermented, and from which by fractioned distillation alcohol could evidently be obtained. Genuine cells of yeast appeared, and a strong disengagement of carbonic acid. It seems, therefore, that the proteïc substance of the fungi may easily pass over to that state of metamorphoses in which the sugar (or matter yielding sugar, as inuline) contained in the fungi is brought into the vinous fermentation. The fungi arrived at that state of alcoholic fermentation, even though after eight days' standing in the open air, exhibited an acid odor, but which betrayed no rottenness. Several of the fungi were very rich in vegetable mucus, which, on being evaporated in a water-bath, invariably formed thick membranes; their preparation, however, was very difficult. We likewise instituted some experiments with reference to gases exhaled by fresh fungi, employing on that occasion the following apparatus:—

A large bottle, with a narrow neck, was almost entirely filled with fresh fungi (species of agaric); a tube for the disengagement of gases reached to the bottom of the bottle, which, being bent in a rectangle, was connected with another tube, two feet long, and filled with fragments of pumice-stone (impregnated with concentrated sulphuric acid); by the latter it was intended to retain the ammonia, if any was formed; to the latter tube a bottle was attached filled with baryta water, which in its turn was connected with a tube containing caustic potasse, and a bottle holding some concentrated sulphuric acid; to the latter a tube containing chloride of calcium was adapted, and this connected with the combustion-tube containing oxide of copper; the last was put on the furnace used in elementary analyses. The CuO was first ignited in this tube, and after applying to both its ends tubes containing chloride of calcium, for a quarter of an hour dry air was passed over it by means of an aspirator; after this the apparatus was mounted in the manner described, and before the tube containing the ignited oxide of copper, two tubes were placed, one with chloride of calcium, carefully weighed, and another with fragments of caustic potasse, likewise weighed; the latter communicated with a second chloride of calcium tube, which was finally adapted to a voluminous aspirator. In addition to these a tube entered into the cork of the first bottle (in which the fungi were put), being connected with a glass cylinder containing sulphuric acid, in such a manner that, during the operation of the aspirator, the air conveyed through the apparatus was to pass this liquid; this was done, rather with a view to procure a means for ascertaining the quantities of the passing air than to purify as for this purpose ample provision was made in the apparatus. In the first hour, nay, before the aspirator fully acted, considerably less turbidness and precipitation were noticed in the baryta water; after the aspirator was allowed to run for three hours, and the CuO, during this time, kept in a permanent state of ignition, the two tubes with the chloride of calcium and with the hydrated potassa, which had been weighed and placed before the apparatus, were removed and weighed again; the increase of weight in both was so considerable, (in the tube with the chloride of calcium 100 milligrammes, and in that with the hydrated potassa 300 milligrammes) that a mere incidental increase of weight was out of the question; a large quantity of gas containing carbon and hydrogen must have been produced. It is likewise to be remarked that the sulphuric acid, over which the gases passed, became brownish (from organic matter). At the close of the experiment the fungi exhaled the same odor as when in the fresh state, and only after standing for three

days in the open air the said odor of new and wine unfermented was perceived.

A second experiment, conducted entirely in the same manner, furnished completely similar results. When in some cases, instead of passing the gas over the calcined oxide of copper, we first collected it over water and then, over mercury, brought it in contact with spongy platinum and oxygen, even though we allowed it to stand for several hours, no decrease of volume could be noticed; hence no *free hydrogen* was contained therein, but it was rich in nitrogen. Marcet* found, some time ago, that fungi, on being placed under water and exposed to the rays of the sun, exhale a considerable quantity of hydrogen; this experiment seems so little connected with the physiological relations of fungi which live neither in water nor in the rays of the sun, that even though the hydrogen was really disengaged, no conclusion ought to have been drawn from that fact as to the phenomena of vegetation of the fungi. All that is now fully established is, that fresh fungi (like all green portions of plants) exhale great quantities of carbonic acid, to which, at least in individual species, carburetted hydrogen seems to be added.

The acids proper to fungi, if opportunity be afforded to us, we intend, on an early occasion, to make the subject of a second investigation. —nt—

ON SOME NEW SERIES OF COMBINATIONS OF TUNGSTIC ACID WITH THE ALKALIS.†

BY M. MARGUERITTE.

It is known that tungstic acid, in combining with the alkalis, forms salts in which the oxygen of the acid is triple that of the base, and that the tungstates regarded as neutral are decomposed without the aid of heat by the acids.

The compounds of which I now treat will show that tungstic acid may not only form compounds in different proportions from those of known tungstates, but may also undergo in its reactions and ordinary properties, modifications which present some interest.

If tungsten ore be calcined with an excess of an alkaline carbonate, a fused mass is obtained, which, treated with water, gives a solution representing a neutral tungstate, mixed with the excess of alkaline carbonate employed.

By gradually decomposing this liquor by an acid, hydrochloric acid for example, a white precipitate of hydrated tungstic acid is formed, which, at first, soon disappears by agitation; but, pursuing the decomposition by an excess of acid, the precipitate reappears and does not redissolve. It is the ordinary means of preparing the hydrate of tungstic acid.

But if the acid liquor be filtered and put in contact with a plate of zinc, it assumes a very intense blue color, which announces the presence of a great quantity of tungstic acid in solution.

Now, from what is known of the properties of the acid, there should remain in the liquor only a quantity corresponding to the solubility of this hydrate in water, which, by ebullition or evaporation, is always ultimately deposited. After having evaporated this solution, I obtain, after successive crystallisations of the alkaline chloride, a definite compound which, as is evident, might exist and be crystallised in an acid liquor. With the small quantity of this compound which is produced in this manner, it was easy for me to recognise that it contained tungstic acid and an alkaline base.

In seeking for the causes of the formation of this salt, I thought that I ought to ascertain whether, in the primitive liquor, the excess of alkaline carbonate was necessary.

The solution of a tungstate of average concentration was treated by a few drops of hydrochloric acid; it produced a precipitate which subsisted while cold, and which, when slightly heated, completely disappeared. It therefore became evident, from this experiment, that the hydrate of tungstic acid might be dissolved, in the absence of the excess of carbonate, in the undecomposed neutral tungstate.

Indeed, when the solution of a neutral tungstate is boiled with hydrated tungstic acid, this precipitate rapidly disappears, and in considerable proportions. The liquor having arrived at the point of saturation deposits by cooling a certain quantity of hydrate, which it had dissolved at a higher temperature. These deposits continue to be produced during the evaporation, but finally a compound crystallises under forms which vary according to its composition. This salt may be again dissolved, boiled and crystallised, integrally retaining its properties.

Such is the most simple means of procuring these peculiar tungstates; however, there exists another mode of preparation. This second process consists in boiling a caustic alkali, or without carbonate, with an excess of hydrated tungstic acid. But it is clear

* *Annalen der Chemie*, t. xl., p. 318.

† *Journal de Pharmacie*, March, 1845.

that this is the same as if a neutral tungstate had been used.

The compounds produced in this manner differ in their properties and their composition from the neutral tungstates, the only ones known, except the acid tungstates prepared by fusion, with which Wöhler obtained his very remarkable soda compound.

The ordinary process of estimating tungstic acid, whose accuracy, moreover, has been recognised as insufficient by all chemists, was not applicable with certainty to the analysis of these salts; for in the limit of errors which may be committed in employing it, the centesimal composition of these compounds may cause their formula to vary from one equivalent of acid, or at least leave it undecided. Indeed, a difference of 1 or 2 per cent. constitutes a tungstate of 4 or 5 equivalents of acid.

However, Berzelius, in his report for 1841, gives a precise means of estimating tungstic acid in the neutral tungstates by the nitrate of mercury; but I fear that in thus precipitating the acid tungstates, the excess of nitric acid liberated would react on the precipitate of tungstate of mercury, and thus interfere with the accuracy of this process.

The following is that which I used, and which has given me results of satisfactory concordance. I propose it, therefore, as being of great simplicity and of strict accuracy.

The salt to be analysed is placed in a small platinum capsule, to which are added several times its weight of pure and concentrated sulphuric acid. Employing at first a gentle heat, the temperature is gradually raised to redness. After calcination, the residue is composed of an acid sulphate and of free tungstic acid. It is thrown on a filter, washed with water charged with sal-ammoniac, which possesses the property of preventing tungstic acid from combining with water, and from passing through the filters, which often happens, even when it has been calcined.

When the last washing waters no longer yield a precipitate with chloride of barium, the residue is calcined, in order to drive off the ammoniacal salts, and a few drops of nitric acid are added, to obviate the slight reductions which the tungstic acid may have undergone, and to complete the combustion of the last traces of carbon arising from the filter.

In this manner the water may be estimated by a simple calcination, the tungstic acid directly.

Having endeavored to estimate the alkali by the quantity of sulphuric acid fixed, I proved that tungstic acid, like silicic and boracic acids, decomposed the neutral sulphate at the temperature at which it must be produced for destroying the bisulphate. The tungstic acid then enters into combination with the alkali, forming with it a more or less acid tungstate. It is therefore important not to exceed this limit.

The bisulphate has the advantage of presenting to the parts of the salt which would not be decomposed, sulphuric acid, which, at this high temperature, acts energetically.

It may be ascertained, moreover, that the salt has been entirely decomposed, and that the tungstic acid has not been rehydrated, and consequently dissolved, by treating the first washing water by a sheet of zinc and hydrochloric acid. Not the slightest blue coloration is then produced, and very small quantities of a tungstate, in these circumstances, would be detected by this characteristic.

According to this process of analysis, the following are the results which I have obtained with salts prepared by the means I have indicated:—

I. Bitungstate of soda (in lamellar crystals), decomposable without heat by the acids, $SO\,(WO^3)^2\,4\frac{1}{2}HO$:

II. Tritungstate of ammonia (in octohedral crystals) fuses in its water of crystallisation and under water, like phosphorus, $H^4NO\,(WO^3)^3\,5HO$:

III. Quadritungstate of soda (crystallised in tables from an acid liquor), $SO\,(WO^3)^4\,3HO$:

IV. Pentatungstate of potassa (in prismatic crystals), $PO\,(WO^3)^5\,8HO$:

V. Hexatungstate of ammonia (in defined lamellar crystals), $H^4NO\,(WO^3)^6\,6HO$:

VI. Double bitungstate of ammonia and potassa, $PO\,(WO^3)^2\,H^4NO\,(WO^3)^2\,6HO$.

It is evident from the salts whose composition I have just indicated, that there exist series of tungstates, with 1, 2, 3, 4, 5, and even 6 equivalents of tungstic acid to 1 equivalent of base; and, although I do not present here the complete series for each base, it will be easy, taking the most acid tungstate, and making it dissolve a determinate quantity of alkali, to prepare the intermediate tungstates.

A neutral tungstate dissolving with ebullition a great quantity of hydrate, which it afterwards abandons by cooling, it is possible that by changing the medium a tungstate might be precipitated or crystallised of a degree of acidity higher than those which I have obtained.

These tungstates possess interesting properties—

I. Put in contact with hydrochloric, nitric, and sulphuric acids, they are not decom-

posed without heat, nor even at the limit of boiling.

It is only at the end of a longer or shorter time, without or with ebullition, that they deposit tungstic acid under their influence. This singular stability, in presence of acids which always instantaneously decompose the neutral tungstates without heat, seems to become so much the greater as their degree of acidity is higher. Thus the tungstates of soda of the first and second degree are decomposable without heat by the acids; but that of the fourth degree is perfectly stable.

II. Their solubility is far from diminishing in the ratio of the quantity of tungstic acid they contain, for the quadritungstate is much more soluble than the bitungstate.

III. Treated by an excess of alkali, they again become decomposable by the acids without heat, because they are reduced to the state of neutral tungstates, and resume their properties. Under the influence of a caustic alkali or a carbonate, a white precipitate is formed, which, without heat, is insoluble in the excess of alkali, and which disappears only on being boiled; but it is instantaneously soluble in the solution of a neutral tungstate. This precipitate represents hydrated tungstic acid retaining a certain quantity of alkali which washing never completely removes from it, for it gradually dissolves in the very proportions of the elements of which it is constituted. But the fact to be remarked is the insolubility without heat of the tungstic acid precipitated from an acid tungstate by an excess of alkali, whilst the hydrate, precipitated from a neutral tungstate by an acid is entirely soluble in the same circumstances.

In acting on very dilute liquors, and when too great an excess of alkali has not been put in, the addition of an acid causes this disappearance of the precipitate of hydrate which is formed in the solution of an acid tungstate.

IV. They have a very decided acid reaction on litmus paper, whilst the neutral tungstates appear to have no reaction or a slightly alkaline one.

V. Their taste is of a characteristic bitterness, and much stronger than that of the neutral tungstates.

VI. When calcined, they lose their water of crystallisation and of composition; becoming yellow, and ceasing to be soluble, they contain, without any doubt, free tungstic acid. Heated in an oven to 430°F., they abandon, without becoming yellow and insoluble, a certain quantity of their water of crystallisation, and it is only at a higher temperature that the combination is destroyed and becomes insoluble in losing the last quantities of water which it retains more powerfully.

VII. By double decomposition, they form corresponding insoluble tungstates which, at the very moment of their precipitation, are soluble in these acids for some time without being decomposed, but after a spontaneous desiccation, or desiccation *in vacuo*, or in the medium of their formation, they very soon lose this property.

Their conditions of stability, in presence of acids, seem therefore to be inherent to their state of hydratation.

VIII. They may form double acid salts, by combining with one another in different proportions, and they enjoy the same properties; the double salt of ammonia and potassa for example.

Whatever explanation may be given of these facts, whether the existence of an isomeric modification of tungstic acid soluble in the mineral acids be admitted, or whether water be made to intervene as acting the part of a true base, it is interesting to see that a salt decomposable by the acids becomes stable from the single fact that it takes one more equivalent of tungstic acid, and that it retains, I will even say that it acquires, a greater solubility, when it should lose it; for, with respect to soaps, M. Chevreul made this general observation, that the more acid an insoluble acid salt is, the more insoluble it becomes.

We are induced to admit that water acts the part of a base in these combinations, in the first place, from the tenacity with which a certain quantity is retained at 430°F., the yellow color and insolubility which these salts assume when it has been removed from them by heat, and, in the second place, by the fractional composition for water, which some of these salts seem really to affect; for having operated on a perfectly pure salt which had been crystallised several times, I cannot doubt the accuracy of this analysis; and, although fractions of equivalents of water are not very customary, I must, however, admit them in the composition of bitungstate of soda.

Thus, instead of grouping round the alkali the whole quantity of tungstic acid, the water may be made to intervene in such a manner as to represent tungstates of water and alkali; but then, as regards the double tungstate of ammonia and potassa, it must be admitted, for the same reason, that it is a triple tungstate of two alkalis and water. We can, however, only form hypotheses as to the molecular constitution of these combinations.

Molybdenum, which presents such great

analogy to tungsten, will probably give similar molybdates, and the solubility of molybdic acid in the acids can only be favorable to the formation of these compounds.

In a memoir which I shall shortly have the honor of submitting to the Academy, it will be remarked that tungstic acid, with, relation to the organic acids, presents properties analogous to those which it shows towards the mineral acids: for example, its complete stability in the state of hydrate in oxalic and tartaric acids, the oxalates and bitartrate of potassa; its precipitation by the alkalis, in certain circumstances, and the property which these acids possess of decomposing the tungstates, without precipitating tungstic acid.

This work has been carried on in the laboratory of M. Pelouze, and I am happy to be able here to tender him my acknowledgment of the advice which he has been kind enough to give me.

ON THE LIQUEFACTION AND SOLIDIFICATION OF GASEOUS BODIES.

A LECTURE DELIVERED AT THE ROYAL INSTITUTION,

BY PROFESSOR FARADAY.

BEFORE commencing his lecture, the Professor read an extract from a letter written by Professor Liebig, of Giessen, shortly after his visit to this country, in which the learned writer said, the thing which struck him most in England was the persuasion that only those works that had a practical tendency attracted attention and commanded respect, whilst those which were purely scientific were almost unknown; and yet the latter were the true sources from which the others flowed. In Germany, added Liebig, it was the contrary; but he did not say that that was better—in his opinion the golden medium was the proper course. Mr. Faraday then proceeded with his lecture. The condensation of gases (said he) had been brought before the public some years ago. A gas was one of those substances in an aërial form which remained permanent under the ordinary circumstances of temperature and pressure, whilst vapor was like gas, but which under ordinary circumstances was condensable again into liquid. It was at one time thought that all gases were perfectly elastic fluids, but by his researches he had succeeded in turning into vapor the following nine gases, namely, chlorine, muriatic acid, sulphurous acid, sulphuretted hydrogen, carbonic acid, cuchlorine, nitrous oxide, cyanogen, and ammonia. One of these, namely, carbonic acid, the late celebrated Thillorier, of Paris, had, after many experiments, obtained in a solid state, and Bunsen had subsequently obtained also cyanogen in a similar condition. But although continued attempts had been made to solidify the other seven, and by immersion in deep water a pressure of 200 atmospheres, *i. e.*, of 3,000lbs. to a square inch, had been produced, still they had been unattended with success. He would explain what he believed to be the reason of the failure. If he took a bottle half filled with ether (and this was Latour's experiment), and applied to it heat, the ether would rise in vapor, and so would continue until the vapor was much condensed. At last, the liquor below and the vapor above would be of as nearly the same weight as possible, and the least degree of additional heat would turn the liquor into vapor, or, if taken away, convert the vapor into liquor. Observe what happened. At that temperature of ether no pressure could bring the ether into a liquid state; at a lower temperature it would. He believed, then, the reason why so many had failed in liquefying and solidifying gases was, that although they could procure the immense pressure he had mentioned, they could not obtain a degree of temperature sufficiently low. He would explain in what manner he had succeeded. He had taken as his basis carbonic acid gas in its solid state as produced by Thillorier. A quantity of carbonic acid, in partly a liquid and partly a vapor state, being confined in a tube, the expansion of the vapor forced the liquid through an orifice in the side into a cylindrical brass box, and being acted on by a rapid current of air the liquid carbonic acid was immediately converted into a solid substance like snow. Its temperature in that state was 70° below 0 of Fahrenheit; but though he took that as his basis, it was not low enough for the purpose of his experiments. The temperature must, therefore, be further decreased. It had been demonstrated by Thillorier, that if ether were applied to solid carbonic acid, the temperature could be reduced to even 105° below Fahrenheit; but a lower degree was still required, and that was obtained by exhausting the air. His object, then, was to combine this extreme degree of cold with great pressure in his experiments on gases. The means by which he effected it he thus described:—A quantity of gas in a glass vessel was forced by a condensing pump into a tube inserted in the receiver of an air pump; that part of the tube inserted in the receiver was made of common bottle glass (the strongest kind for experiments, and capable of bearing an enormous pressure) in the shape of a retort, and the bent or lower

part of the tube lying immersed in the cold bath (produced by solid carbonic acid combined with ether, after the air had been exhausted), gas in a liquid, and by an increased degree of pressure, in a solid state, could be obtained. The learned professor illustrated the truth of the principle by producing olefiant gas in a liquid state, and observed that he had succeeded in obtaining in the same condition phosphoric hydrogen, by hydriodic acid, hydrobromic acid, fluoboron and fluosilicon; and in a solid form sulphurous acid, sulphuretted hydrogen, euchlorine, nitrous oxide, hydriodic acid, and hydrobromic acid. He had made carbonic acid the type of the others, but he thought that nitrous oxide would give a power of temperature as far below carbonic acid as that was below common ice. He saw no reason why the same result might not be obtained from oxygen, hydrogen, and nitrogen; and, in fact, he had hoped that evening to have shown oxygen in a liquefied state, but he had failed in his experiments, not because his principle was wrong, but from the porous, and hence imperfect, nature of the vessels used. With respect to hydrogen, he had had indications in the course of his experiments that it would be found to be a metal of a most subtile nature.

ON THE ACTION OF CHARCOAL ON METALLIC SOLUTIONS.*

BY M. CHEVALLIER.

It is known that the discovery of the decoloring properties of vegetatable charcoal is due to Löwitz, that that of animal charcoal was announced by Kehls (*Journal de Physique*, 1793), and better appreciated by Figuier in 1810; finally, that it has been the subject of works of high inportance on the part of MM. Payen, Bussy, and Desfosses, who obtained, in 1822, the former two, the prize, and the third, the medal of encouragement awarded by the Societé de Pharmacie in Paris.

In occupying himself with works on charcoal, M. Payen ascertained that this body possessed the property of removing lime and the salts of lime from the liquids which contain these products when submitted to the action of charcoal.

M. Lassaigne afterwards recognised that charcoal put in contact with iodide of starch and with a solution of iodine, combined with the iodine, and removed it from the liquids, so that no more traces of this body were found in liquids treated by charcoal.

Berzelius has also investigated the action of charcoal, and thus expresses himself on this subject: "It has not yet been examined with the requisite care what are the substances which charcoal separates from their solution in water, and what are those which it does not precipitate; it appears, indeed, from the observations hitherto made, that it acts on substances of organic origin, principally on the coloring and odorous substances, such as wood of *Fernambouc*, cochineal, litmus, indigo dissolved in sulphuric acid, the red color of wine, the brown color which taints the solutions of saltpetre, sugar, and succinic acid, the fetid effluvia of bodies in a state of putrefaction, the empyreumatic oils, that of corn brandy, and of various vegetable oils; but Graham has proved that this property extends to inorganic bodies; he has found, for example, that charcoal precipitates iodine from its solution in iodide of potassium, lime from lime water,* neutral nitrate of lead, and all the metallic subsalts on which he operated, from this solution either in water or in a mixture of that liquid and ammonia, and that the precipitation is effected in so complete a manner, that no more remains in the liquor; on the contrary, arsenious acid and several neutral salts are not precipitated from their aqueous solution; it would be so much the more important to determine what are the bodies, mineral as well as organic, which it is possible to precipitate by this means, as this property of charcoal might be employed in chemical analysis."†

We did not know of Graham's observations when, in 1843, we recognised, in operating on acid wines containing salts of lead, that these wines, when decolored by charcoal did not contain this metal; it is this fact which led us to make the experiments which we here describe.

The experiments we tried were with vegetable charcoal, with washed and with unwashed animal charcoal; these experiments were made in some cases with, and in others without, the aid of heat.

We have acted on water, wine, alcohol, and on acetic acid, and we observed—

I. That vegetable charcoal removes the salts of lead, the acetate and nitrate, contained in all these liquids.‡

* *Comptes Rendus.*

* These facts had been noticed, the first by M. Lassaigne, the other by M. Payen.

† See Graham's *Chemistry*, p. 302.

‡ We are continuing the experiments which we have undertaken on the salts of iron, copper, zinc, mercury, arsenic, anti-

II. That this separation, which took place without heat, was rendered much more active by its aid.

III. That a greater quantity of vegetable charcoal than of animal charcoal is required for removing salts from liquids which contain them.

IV. That five grammes of vegetable charcoal, and five days' contact without heat, are required for removing 50 centigrammes of acetate of lead dissolved in 100 grammes of water.

V. That six days' contact and 10 grammes of vegetable charcoal are requisite for removing 50 centigrammes of nitrate of lead from 100 grammes of distilled water.

VI. That one gramme of unwashed animal charcoal and 48 hours' contact are necessary for separating 1 gramme of acetate of lead dissolved in 100 grammes of water.

VII. That 2 gr. 50 of unwashed animal charcoal and 48 hours' contact are required for removing, without the aid of heat, 50 centigrammes of nitrate of lead from 100 grammes of water.

VIII. That 1 gramme of unwashed charcoal and 24 hours' contact are requisite for removing 50 centigrammes of acetate of lead from 32 grammes of alcohol.

IX. That 1 gramme of charcoal and 24 hours' contact are required for separating, without heat, 50 centigrammes of acetate of lead from 50 grammes of vinegar.

X. That experiments made with nitric and hydrochloric acids have demonstrated that charcoal does not remove from these acids the lead which they contain in solution.

XI. That experiments made with animal charcoal washed and exhausted, of phosphate and carbonate of lime, have demonstrated to us that it requires—

A. 1 gramme of washed charcoal and 24 hours' contact to remove from 100 grammes of water 50 centigrammes of acetate of lead:

B. 2 gr. 50 of washed charcoal and 48 hours' contact for removing from 100 grammes of water 50 centigrammes of nitrate of lead:

C. 1 gramme of washed charcoal and 24 hours' contact for separating from 50 grammes of alcohol 50 centigrammes of acetate of lead:

D. 1 gramme of washed animal charcoal and 24 hours' contact to displace from 50 grammes of vinegar 50 centigrammes of acetate of lead:

E. 2 grammes of washed charcoal and 48 hours' contact to decolor 150 grammes of red wine containing 50 centigrammes of acetate of lead, and to remove from it that salt.

XII. That from experiments made with the aid of heat, it results—

A. That 1 gramme of unwashed animal charcoal and 2 minutes' boiling are required for removing from 100 grammes of water 50 centigrammes of acetate of lead:

B. That 2 gr. 50 of charcoal and 2 minutes' boiling are requisite for removing from 100 grammes of water 50 centigrammes of nitrate of lead:

C. That 1 gramme of unwashed charcoal and 5 minutes' boiling are necessary for removing from 50 grammes of vinegar 50 centigrammes of acetate of lead:

D. That 2 grammes of unwashed charcoal and 5 minutes' boiling are required for decoloring 150 grammes of red wine, and for removing from it 50 centigrammes of acetate of lead.

Experiments made in the same conditions with washed charcoal have proved to us that this body, like unwashed charcoal, removes salts of lead from water, vinegar, and wine, and that only a few minutes' ebullition are required.

If the water in which washed charcoal has been made to react on acetate and nitrate of lead be examined, it is observed that this water frees acetic acid if the acetate, and nitric acid if a nitrate have been used.

Moreover, if we put in contact in a retort —1st, acetate of lead, water, and washed charcoal, and distil, acetic acid is obtained; 2nd, nitrate of lead, washed charcoal and water, and distilled, nitric acid is obtained. There is also found, in the liquor in which the decomposition is operated, and which has been submitted to distillation, free acetic acid in the one case, and free nitric acid in the other.

If we put in contact—1st, water, acetate of lead, and washed and pure charcoal, and if the mixture be left in contact and occasionally stirred, a decomposition is observed: the oxide of lead combines with the charcoal, and free acetic acid is found in the liquor; 2nd, nitrate of lead, water, and pure charcoal, and if left together and occasionally stirred, a decomposition is remarked: the oxide of lead combines with the charcoal, and free nitric acid is formed in the liquor.

Trials of application have been made, and it has been ascertained that the orange-flower water of commerce, which contains salts of lead, in consequence of its being kept in vessels tinned with a mixture of tin and lead, may be freed from these salts by the employment of charcoal; for this purpose, it is put in contact with washed

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mony, &c.; finally, we intend also to examine the action of charcoal on the organic alkalis, &c.

animal charcoal, stirred several times, allowed to settle, and filtered.

M. Naveteur, who, at our request, has made some experiments, has ascertained that, with a few grammes (3 or 4), the salts of lead may be removed from a vessel containing 25 litres of this liquid (the operation was performed at M. Muraour's); the water thus deprived of these salts of lead had not perceptibly lost any of its odor.

We have repeated this operation in our laboratory on some orange-flower water procured from M. Durand, and which contained salts of lead; the lead was removed by the charcoal.

We have also made experiments:—1st, with charcoal prepared by the treatment of flesh by sulphuric acid at 66°; 2nd, with charcoal prepared by the carbonisation of calf's liver in a close vessel. We have ascertained, by these experiments—1st, that the sulphuric charcoal, put in contact, without heat, with water containing acetate of lead, has little or no action, and that the salt of lead remains in solution in the liquid; 2nd, that this charcoal employed with the aid of heat removes a portion of lead; 3rd, that the liver charcoal, either cold, or with the aid of ebullition, partially decomposes the salts of lead, but that the separation is not complete.

From the foregoing, it seems to us to result that vegetable charcoal, unwashed animal charcoal, and animal charcoal washed and separated from the carbonates and phosphates—charcoals which, as is known, form combinations with coloring matters, which combinations are insoluble and precipitate, —are also capable of uniting with the metallic oxides, of separating them from solutions in which these oxides are combined with the acids, and of forming insoluble combinations, liberating the acid.

This property of charcoal of removing the metallic oxides must, in many cases of chemical jurisprudence, be the cause of errors; indeed, in a great number of cases, authors direct the decoloration, by charcoal, of the liquors in which are to be sought the metallic salts which are susceptible of being removed by the charcoal; this indication of the employment of this body exists not only in old works, but in works recently published, and which we have at hand; here we find *the formal prescription for decoloring by this body, liquids in which we have to determine the presence of a salt of lead and other metallic salts.*

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ON THE CHEMICAL COMPOSITION OF GUANO.*

EXTRACT OF A LETTER FROM M. E. MARCHAND, OF FÉCAMP, TO M. BOULLAY.

Being entrusted with the revision, for a local journal, of an article on guano, and on its value as a manure, I examined this curious product myself, and I found, by analysis, that it is composed of—

- Hippurate - -
- Urate - - -
- Phosphate - - } of ammonia.
- Oxalate - - -
- Hydro-chlorate
- Chloride of sodium.
- Carbonate (?) - -
- Oxalate - - - } of lime.
- Phosphate - -
- Ammoniaco-magnesian phosphate.
- Alumina, oxide of iron, and silica.
- Indeterminate organic matter.

The existence of hippuric acid in this product is very remarkable;† for, if guano be regarded, with M. Girardin, as coprolithes arising from antediluvian birds, or, with M. de Humboldt, as excrements and altered remains of birds which have inhabited, and still inhabit, the islands of the coast of Africa, it is no less true that the origin of hippuric acid, in this product, is difficult to be conceived, since none of the analyses of the excrements of birds which have hitherto been made and published have noticed this principle among its constituents.

Struck with these singular results, and supposing, besides, that hippuric acid might also be found in the excrements of birds with which we are more familiar, and that if skilful chemists have not met with it, it is probably owing to their not having looked for it, I am now undertaking a series of analyses of the *excrements of granivorous, piscivorous, and carnivorous birds*, being persuaded that investigations of this kind, properly directed, may give rise to physiological considerations and deductions of high importance.

When my experiments are completed, I will do myself the honor of communicating to you the results.

* *Journal de Pharmacie*, Feb. 1845.

† We have long known it.—Eds. Chemist.

EXPERIMENTS WITH MANURES ON WHEAT IN THE YEAR 1844.—By J. B. Lawes, Esq. Rothamsted, Herts.

QUANTITIES OF MANURE PER ACRE, IN BUSHELS.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Super-phosphate of Lime, 70 lbs. per bush., made with calcined bone, No. 17, being made with common bone	10	..	No Manure.	..	10	5	5	5	9	8	5	5	5	5	5	5	5	5	5	..
Phos. of Magnesia, with Sul. of Lime, 84 lbs. per bush.	..	..		..	..	5	..	..	..	..	..	2½	2½	2½	2	1	1	1	1¼	..
Phos. of Soda, with Sul. of Lime, 65 lbs. per bush.	..	..		..	..	..	5	..	..	..	..	2½	..	..	..	1	1	1	1¼	..
Phos. of Potash, with Sul. of Lime, 75 lbs. per bush.	..	..		..	..	..	..	5	..	..	..	..	2½	..	2	1	1	1	..	..
Sul. of Ammonia, 60 lbs. per bush.	..	..		..	..	..	..	..	1	..	..	..	..	..	..	1	1	1	1½	..
Sil. of Potash, 110 lbs. per bush.	..	..		..	..	..	..	..	..	2	..	..	..	2½	1	1	1	1	1	..
Rape Cake, 60 lbs. per bush.	2½	..		..	..	..	..	..	..	..	5	..	..	..	..	..	..	2½	..	..
Burnt Clay Ashes, to make each quantity equal to 20 bush. per acre	7½	..		..	10	10	10	10	10	10	10	10	10	10	10	10	10	7½	10	..
Farm-yard Dung	..	Tons 14		..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
14 tons of Farm-yard Dung, burnt to ashes	..	..		Cwt. 32	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
Mixture of equal parts of other 19 lots.	..	..		..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	20
Dressed Corn, per acre, in bush.	18	22	16	16	16.8	16.5	16.75	16.25	21.2	16.4	18.8	16.75	17.5	17	17.8	21.25	20.6	22.75	26.25	21
Do. do. in lbs.	1040	1276	923	888	956	964	984	980	1280	1008	1116	1004	1072	1016	1096	1304	1240	1368	1580	1228
Aver. weight of Dressed Corn per bush.	59	59¼	58½	58	58½	60	60½	61½	62½	62	61¾	61½	62½	61¼	62	62½	62¼	62	61¾	58½
Straw and Chaff, per acre in lbs.	1203	1476	1120	1104	1116	1100	1172	1160	1368	1112	1200	1116	1204	1176	1240	1480	1422	1769	1772	1436
Relation of Corn to Straw (Straw being 1000)	864	864	824	804	856	876	840	845	935	906	930	900	890	864	884	881	872	774	892	855
Proportion of Increase in Corn by Manure (No. 3 being 1000)	1120	1382	1000	962	1036	1044	1066	1061	1387	1092	1209	1088	1161	1100	1187	1413	1343	1482	1712	1282
Proportion of Increase in Straw by Manure (No. 3 being 1000)	1074	1318	1000	985	996	982	1046	1036	1221	993	1071	996	1075	1050	1107	1321	1269	1576	1582	1282

Soil.—Heavy loam, well adapted for wheat.

Previous Crops.—Barley, peas, wheat, oats, all without manure, drilled in the autumn, 15 inches between the rows; dung and dung ashes ploughed in, the rest of the manures drilled with the seed, with the exception of sulphate of ammonia, which was sown by hand in the spring. The quantity of land employed for each experiment about two-thirds of an acre. Although the quantity of wheat grown upon No. 3 is given, it may be considered as being considerably higher than what an acre of ground without manure would yield, as it had evidently borrowed from the dung in the adjoining experiment; and as the length of each space was 400 or 500 yards, the increase must be considerable. The absence of all effect from the application of super-phosphate of lime in No. 5 is very remarkable, as this salt has been used upon the wheat crop on the same farm for years, with remarkable success; but when used in conjunction with an ammoniacal salt, as in No. 9, the increase in produce is very striking, and proves that the previous crops had exhausted the soil of its ammonia as well as its phosphates, and that the wheat was incapable of obtaining its necessary supply from the air, although in the ordinary course of cropping sufficient is left in the soil to supply its wants. The effect of sulphate of ammonia in Nos. 16, 17, 18, 19, is equally remarkable; and in No. 19, where a slight increase of the salt is added, the produce of corn is increased some bushels. In Nos. 10 and 14, where the silicate of potash was used in the largest quantities, much of the straw was green and unripe, especially in No. 14. Considering the large proportion of phosphate of potash that exists in wheat, it is remarkable that this salt produces so little effect, although it may be observed that the weight of corn per bushel is higher when this salt has been used. The season that has just passed has been one not well adapted for experimental purposes, although wheat is a crop that suffers less from dry weather than any other. It is intended repeating these experiments (with some slight alterations) this year on the same field, and the results will in some measure show whether the conclusions to which these experiments lead, with regard to wheat, are correct.—*Gardener's Chronicle, and Agricultural Gazette*, March 1, 1845.

EXPERIMENTS WITH MANURE ON GRASS, IN 1841.*

BY J. B. LAWES, ESQ.

SPACE of each experiment, 88 yds. by 13 yds., 2 ft. 4 in.; manure sown by hand, Mar. 4; hay weighed when made, June 17.

Weight of Manure.	Weight of Hay.
1.—56 lb. bone-dust	449 lb.
2.—56 lb. bone-dust, 10 lb. sulphuric acid	415
3.—Ditto ditto 20 lb. ditto	377
4.—No manure	378
5.—56 lb. bone-dust; hydrochloric acid, 25 lb.	449
6.—Ditto ditto; hydrochloric acid, 5 lb.; sulphuric acid, 15 lb.	489
7.—Ditto ditto, 15 lb.; ditto, 15 lb.	483
8.—No manure	424
9.—Bones, 56 lb.; hydrochloric acid, 15 lb.; sulphuric acid, 5 lb.	434

Soil, heavy. *Bone-dust*, when applied to grass, useless.

The difference in these weights may be considered as no more than would arise from errors of weighing, and the results may be considered as unfavorable to the use of bone-dust. Further experiments on this soil have shown that an ammoniacal substance is required to grow large crops of grass.

EXPERIMENTS ON THE POTATOE CROP.*

THE idea of the following experiments was taken from reading Professor Johnston's *Suggestions for Experiments in Practical Agriculture*, pp. 28—30 and 63—67. The results and observations there set down are sufficient to excite the attention of every one having his own interest at heart; and that these experiments have failed to add confirmation to those is only another proof of what Johnston again and again holds out, namely, that what may be highly advantageous in one case may be of no use in another. A principal cause of the non-effect of the saline manures of fluid in the present instance, may perhaps be found in the proximity of the situation to the sea (the great source of saline matter), and in the fact that the land had been manured for more than twenty years almost entirely with fish and sea-weed.

* From *The Scottish Farmer*.

* *Agricultural Gazette*, March 8.

Table of Experiments with different Saline Manures, upon Potatoe Crop, 1843, tried at Myres, near Banff, 1¼ mile from the sea, and about 200 feet above its level. Twelve drills wide, and 121 feet in length, were measured off in the middle of the field, and each drill manured as under, at the rate of 1½ cwt. per imperial acre.

Description of Manure.	Produce per 121 ft.	Produce per imp. acre.	Produce in bolls of 3 cwt.
	lbs.	tns. cwt. lb.	bolls. pecks.
1. Nitrate of soda	109	7 15 80	31 5
2. Sulphate of ammonia	105	7 10 0	30 0
3. Sulphate of soda	113	8 1 48	32 4½
4. Sulphate of magnesia	107	7 12 96	30 9½
5. Nitrate of soda and sulphate of ammonia, equal parts	95	6 15 80	27 2
6. Sulphate of soda and nitrate of soda, equal parts	89	6 7 16	25 7½
7. Nitrate of soda and sulphate of magnesia, equal parts	88	6 5 80	25 2
8. Sulphate of soda and sulphate of ammonia, equal parts	105	7 10 0	30 0
9. Sulphate of magnesia and sulphate of ammonia, equal parts	114	8 2 96	32 9½
10. Sulphate of soda and sulphate of magnesia, equal parts	112	8 0 0	32 0
11. Nitrate of soda and sulphates of soda, magnesia, and ammonia	106	7 11 48	30 4½
12. No saline application	107	7 12 96	30 9½

The land on which the above experiments were tried has a northern exposure; soil light but dry, and containing a fair proportion of vegetable matter—in other words, in good heart; subsoil, a clayish till, overlying greywacke slate. It was after carrying a crop of oats from three-year-old lea, top-dressed with compost of farm-yard dung and earth. In the month of January, 1843, the land got from 30 to 35 loads of farm-yard dung per acre, which was ploughed in, and again cross-ploughed about the middle of April; then drilled about the 1st May, the sets put in, and, without further trouble, covered up. They brairded well; and, after hoeing, the manures mentioned in the table were sown along the top of the drill with the hand. The length of drill taken was 121 feet, equal to the $\frac{1}{180}$th of an imperial acre, the drills being 27 inches wide. The sulphates of soda and magnesia were the common Glauber and Epsom salts of the shops; the sulphate of ammonia was of Aberdeen manufacture, and the nitrate of soda bought from a druggist in Aberdeen. The land had bone-dust for turnips the time before that it was cleaned, and for more than twenty years before that it was manured solely with fish and town-dung. The application of the manures produced no perceptible effect on the potatoe-tops, neither immediate nor remote, nor was the difference of crop, with the exception of Nos. 5, 6, and 7, more than might have been expected from accidental circumstances. The land was as equal throughout as could be got.

MINERAL INGREDIENTS ℔ ACRE OF AVERAGE AGRICULTURAL CROPS.*

BY H. BRIGGS, WAKEFIELD.

	Potash at 4*d.* per lb.	Salt 2*d.* per stone.	Lime 6*d.* per bushel.	Gypsum ½*d.* per lb.	Bones ½*d.* per lb.	Total.
	lbs. *s.d.*	lbs. *d.*	bshl. *d.*	lbs. *s.* *d.*	lbs. *s.* *d.*	*s.* *d.*
Wheat	6 .. 2 0	14 .. 2	1 .. 6	6 .. 0 2	34 .. 1 5	4 3
Barley	14 .. 4 8	17 .. 2½	1 .. 6	12 .. 0 4	48 .. 2 0	7 8½
Oats	42 .. 14 0	7 .. 1	1 .. 6	9 .. 0 3	8 .. 0 4	15 2
Rye	9 .. 3 0	10 .. 1½	1 .. 6	16 .. 0 5½	13 .. 0 6½	4 7
Beans	88 .. 27 8	44 .. 6½	1½ .. 9	8 .. 0 3	70 .. 2 11	52 1
Peas	28½ .. 9 6	36 .. 5	2 .. 12	32 .. 0 11	60 .. 2 6	14 4
Swedes	106 .. 35 4	100 .. 14	1½ .. 9	70 .. 1 11	66 .. 2 9	41 11

* *On the Possibility of Growing Wheat successively on the same Land.*

If the analysis be correct, and the foregoing hypotheses also based on proper principles, it follows that wheat can be grown at less expense per acre than any other crop; and it may be equally deduced that it is less exhausting to the land. It also follows that such land as contains the greatest portion of potash and phosphoric acid can be cultivated the most economically.

MR. RIGG'S PAPER ON THE FORMATION OF CARBON BY ANIMALS.*

[We scarcely think the paper of Mr. Rigg, published by us in our number for February 22, (see "The Chemist," vol. III., page 127,) requires any refutation. If any of our readers could be for a moment misled by it, the following letter may serve to put them right. Mr. Rigg's way of dealing with scientific questions is, unfortunately, not confined to himself; too many of our physiologists, chemico-pathologists, and statisticians, proceed in a manner perfectly similar, although more correctly, generally taking care to avoid conclusions so palpably absurd. It reminds us of the well-known speech of a celebrated statesman, in answer to some apparently conclusive calculations, that "he could prove anything by figures." Obviously, anything can be proved by data taken without any regard to their origin, value, or relations.—Ed. *Lancet*.]

To the Editor of the Lancet.

Sir—The arguments by which Mr. Rigg would persuade us that carbon may be formed out of other substances, by the animal economy, seem to me most strangely inconclusive. His line of reasoning is, as nearly as possible, this:—

The average weight of carbon imparted to the atmosphere in twenty-four hours by the respiratory actions of an adult is, according to certain experimenters, 5,963 grains. But the inmates of certain union houses and prisons only receive in the shape of food, on an average, 3,954 grains of carbon per diem. Therefore, according to Mr. Rigg, they create or secrete nearly 2,000 grains of carbon daily: in other words, he asserts that they give out by their breath from 20 to 50 per cent. more carbon than they receive in their food.

Now it appears to me, that the more natural conclusion for Mr. Rigg to have come to would have been this:—If these paupers do not receive the average quantity of carbon, so neither do these give it out. For it will be observed that his whole theory—a theory utterly subversive of all the best ascertained principles of modern chemistry—rests entirely on the supposition that these paupers do, spite of their restricted diet, give out as much carbonic acid as the average of well-fed people. But, at all events, if he persists in this theory, let him prove by actual experiment, made with the concurrence of impartial persons, that his paupers do emit the average quantity of carbonic acid.

What should we think of a man who (to use a homely illustration) made a statement to this effect:—"My kitchen-chimney, in the year 1810, was ascertained to give out every day nearly 40,000 gallons of smoke, which it requires eleven bushels of coal to produce. But now I only use seven bushels of coal per diem, ergo, my kitchen range forms or secretes four bushels of coal per diem; and as a good deal of water disappears there, too, I suppose that the oxygen and hydrogen are converted into coal." This is exactly Mr. Riggs's theory: but before admitting it, we must ask him, in Liebig's words, "to weigh the smoke."

I cannot help shrewdly suspecting that Mr. Rigg is a covert ally of the poor-law commissioners. Certes, the idea that oxygen and hydrogen (in other words, cold water) are convertible into carbon, and become a source of heat, and that people who do not eat enough to support nature have an eternal means of generating food and warmth, savours very strongly of the usual logic and science of Somerset House.

However, joking apart, it is sad to see a man of ingenuity and talent employing himself in such visionary speculations, whilst he might, instead, be adding to the sum of human knowledge and happiness. I am, sir, your most obedient servant,

Delta.

* *Lancet*, March 15.

KOHL RABI. —— IMPORTANT TO FARMERS.*

BY MR. ALFRED GYDE, PAINSWICK.

Such was the heading of the following letter, addressed a few weeks since to the Editor of the *Gloucester Chronicle*:—

"Everything which in the slightest degree tends to the improvement of agriculture is in the present day of so much importance that I need not apologise to you for troubling you with a few lines on a subject so interesting to the cultivator of the soil. The deficiency of the turnip and Swede crop, in

* *Agricultural Gazette*, March 8, 1845.

consequence of the extraordinary drought of the last summer, and the innumerable destructive insects, fly, grub, &c., produced by the dry and warm weather, has been a lamentable blow to the farmers of the county, and should lead agriculturists to endeavour either to eradicate those enemies to their hopes, or to introduce a root equally valuable with the turnip, while it is less obnoxious to the attacks of those vermin.

The kohl rabi appears likely to supply this desideratum, and I wish, by giving you an account of my experience of it, to induce the farmers of this county to try it on a larger scale. If we can obtain a root—if I may use the Irishism of calling that a root which grows above ground—producing an amount of food, 1st, equal in bulk to the largest crop of Swedish turnips; 2dly, of equal or more nutritive qualities; 3dly, not obnoxious to the attack of grub or fly; 4thly, better adapted for keeping through the winter; 5thly, capable of being planted at any season of the year; 6thly, enduring the frosts of winter; we shall have obtained one of the most useful and valuable plants which the Giver of all good has bestowed upon man. Perhaps I may be considered too sanguine if I attribute all these qualities to the kohl rabi, but I feel convinced that it deserves the greatest part if not all the praise which I abscribe to it. I obtained a small quantity of seed in Germany last year; and in the month of March I sowed, thickly, a patch in a sunny bed in my kitchen garden; the plants soon came up as thickly as possible, much resembling young broccoli plants. The dry weather set in, and I despaired of being able to plant them out in the ground which I had prepared for them. After waiting several weeks, and fearing that the plants had become too old to transplant, I ventured to do so lest the season should go by. I made a good strong compost with cow-dung and water from a muddy pond, and another sort of diluted pigs' urine; and in planting each plant, about 18 inches apart, I made a good puddle about the roots; in less than a week the plants held up their heads and began to grow vigorously. In October I gathered in my crop, and the result is as follows:—On a plot of ground, as nearly as I can reckon containing 18 perches, I had 27 baskets-full, weighing on the average 80lbs. each, or deducting 5lbs. for the basket, 75lbs. each. Thus I had 2025lbs. on this small piece of ground, besides 4 or 5 cwt. of mangold wurzel and self-set potatoes. This, however, was the produce of the driest season in the memory of man. I have no doubt that more than double the amount might be expected in any other season. Several of the bulbs weighed 6 or 7lbs. I purpose sending a few specimens to the Gloucestershire Agricultural Show.—*Samuel Lysons, Hempsted Court, Nov.* 11, 1844."

According to the above promise, Mr. Lysons sent to the Agricultural Show at Gloucester some fine specimens of the kohl rabi, which attracted considerable attention, and elicited many inquiries from those persons seeking information respecting the new root, —which, by the bye, has been for many years known in England — whilst others freely passed their opinion on its merits or demerits. Some shook their heads, and thought it would not answer; others confidently asserted that it never could be introduced as a field crop; a third party inquired what Mr. Lysons knew of feeding cattle, seeing that he was no practical farmer; whilst a fourth thought it might succeed, but it required to be tried as food, and to have its merits and defects made known, before the practical farmer would place confidence in it. The expression of these various opinions by persons to whom the kohl rabi was evidently new, led me to make application for, and to obtain a specimen for analysis, the results of which I promised to publish in the *Agricultural Gazette*, and which I now beg leave to forward for insertion, together with the analysis of the Swede, orange globe, mangold, and white turnip. The latter three plants were grown on the estate of W. H. Hyett, Esq., at Painswick, in a light calcareous soil, resting on the lower oolite, whilst the kohl rabi was produced under circumstances with which I am unacquainted, except from Mr. Lyson's letter. The specimen submitted to analysis was a fair sample of those exhibited at Gloucester; it weighed about 5lbs., was globular in form, green on the outside, and when cut into was firm, solid, and had the appearance and taste of the stem of a cabbage.

On submitting 1000 parts of the kohl rabi, together with the same quantity of the Swede, orange globe, and turnip to the temperature of 230° F. until perfectly dry, the amount of solid matter contained in these bulbs respectively, together with the water they contained, was ascertained to be as follows, viz.:—

	Kohl Rabi	Swede.	Orange Globe.	Turnip.
Water	897	891	853	936
Solid Matter	103	109	147	64

Thus far, at least, we ascertain, that of the weight of solid matter contained in these

bulbs, the kohl rabi has the advantage of the white turnip, and this in the ratio of 103 to 64, or, that it would require 1609lbs. of the white turnip to yield as much food by weight, as 1000lb. of kohl rabi; at the same time it will be seen that the kohl rabi in this respect is inferior to the Swede, or orange globe mangold. The amount of inorganic matter contained in these several bulbs was found to be largest in the kohl rabi, and least in the Swede. 1000 parts of each of these bulbs were found to contain inorganic matter in the following proportions, viz.:—

Kohl Rabi.	Swede.	Orange Globe.	Turnip.
11	6	10	8

The inorganic constituents of crops being obtained directly from the soil in which the plants are grown, becomes a direct indication of their exhausting power, as far as the mineral constituents are concerned. Hence, we find that the kohl rabi would remove from the soil a larger amount of inorganic matter than either of the other bulbs, if taken in their ordinary state; but when compared with that contained in a given weight of the dried bulbs, the white turnip contains the largest quantity of earthy and saline matter, *i. e.* for the weight of dry food it would yield.

The proximate principles of which these four bulbs were composed, as ascertained by careful analyses, were found to be as follow, viz.:—

Organic.	Kohl Rabi.	Swede.	Orange Globe.	Turnip.
Non-nitrogenous.				
Water	897	891	853	936
Fibre	27	33	32	30
Pectin and Sugar	62	65	101	23
Nitrogenous.				
Gluten .... Albumen & Caseïn	3	5	4	3
Inorganic.				
Ash	11	6	10	8
	1000	1000	1000	1000

From the above analyses I think we are justified in assigning to the kohl rabi a place amongst the bulbs (at least as far as its feeding properties are concerned), far above the white turnip, although not quite equal to the Swede: and if, as stated by Mr. Lysons, it can be cultivated so as to produce an amount of food equal in bulk to the largest crop of Swedes; if it be not liable to the attacks of grub or fly; better adapted for keeping through the winter; capable of being planted at any season of the year, and enduring the frosts of winter; the kohl rabi is worth the attention of those English farmers who occupy lands on which the cabbage arrives at perfection, since it is nearly equal to the Swede turnip as food, and appears to be possessed of some advantages over it. At the same time I would suggest that it be harvested before the stem becomes too woody, or this circumstance may become a source of objection to a plant otherwise possessed of advantages to the practical agriculturist.

CHEMICAL ANALYSIS OF RED RUST.*

As an instance of the benefit of analysis, I may here briefly mention, that on a recent chemical examination of the soil of several of our fields at Flocktun, Mr. Haywood, the analytic chemist, whom we employed, discovered in it an appreciable quantity of phosphate of iron, and traced this substance to the cause of the red rust (?) which so frequently attacks the wheat. To satisfy himself that the presence of this substance was the cause of that disease, he collected a quantity of this rust, which he minutely analysed, and he satisfactorily proved that it was composed of phosphoric acid and iron, a combination which, it appears, is injurious; but to ascertain a mode of correcting or decomposing this injurious compound, he subjected it to the action of quick lime, when he discovered that two fertilising substances were immediately formed, namely, phosphate of lime, which is the chief ingredient of bones, and peroxide of iron, which also is a substance possessing fertilising properties, both from its being an ingredient in many cultivated plants, and from its having the power of fixing ammonia. The known fertility of many of the red soils is owing to the presence of this latter compound, and the efficacy of burnt clay, as a tillage, is, in a great measure, due to the conversion of the protoxide into the peroxide of iron, by the operation of burning. On afterwards examining fields of wheat which had been lately limed, and others which had not been so treated for many years, in the former we did not discover the rust, but in the latter it was very prevalent.

* *On the Possibility of Growing Wheat successively on the same Land.*

A CHEMICAL CERTIFICATE AN INSTRUMENT OF DECEPTION.

Dicere verum—quis vetat ?
HORACE.

CHEMISTRY has very properly been designated as the science of truth, in the pursuit of which all reasoning gives way to fact, and errors are corrected by an appeal to actual experiment—it is a science whose results cannot be anticipated by analogy or hypothesis, any more than its arguments may be shaken by sophistry or falsehood—its language simple, clear, and decisive, cannot be misunderstood or perverted without departing at once from the acknowledged limits of the science, and any attempt at quackery or imposition is certain to bring with it a speedy detection and exposure. We have been led to these reflections by observing in the papers an advertisement, containing extracts from a certificate of guano, signed AND. URE, M.D., &c., of which the following is a copy :—

"This is a cleanly collected primitive guano in a state of entire preservation, being the genuine excrement of the birds, and the proportion of rich organic matter is double of that in the average Ichaboe (African) guano, and the ammonia is 70 per cent more.

"From its large amount of *potential* ammonia, its beneficial operation will continue for four or five seasons."

What we are to understand by the term "primitive guano" seems about as doubtful as the fact of its "entire preservation"—the world at large will perhaps conclude that this is an antediluvian guano which has been kept in "a state of entire preservation" by some miracle. Now to be of any use as a manure, guano, and every thing else so employed, should certainly possess a ready power of decomposition, and this tendency to decompose and form new compounds not only exists in guano, but actually constitutes its leading feature. So far then from being in the same "state of entire preservation" in which it was left by "the birds," we fearlessly assert that it is not in the same state in which it was imported, and that even this latter state differs vastly from its condition on being shipped, as any one who has sailed in a guano vessel can abundantly testify. The decomposition which ensues during the time of the voyage (not to mention the antediluvian date referred to) is alone a notorious and disgusting fact, sufficient to annoy even the hardened olfactories of a sailor, and rapidly destroy the vessel. Yet this truly wonderful Peruvian guano is "in a state of entire preservation" according to Dr. Ure; from the same veracious authority, we also learn that "its proportion of rich organic matter is double of that in the average Ichaboe," and that its "ammonia is 70 per cent. MORE," from which we naturally conclude that this incredibly rich guano contains 70 per cent. of ammonia plus the quantity of ammonia contained in the average Ichaboe guano, or in round numbers 75 per cent. If Dr. Ure's statement has any meaning at all, this is its true meaning; this is in fact, the construction which will be put upon it by the purchaser. To attempt to explain it in any other way would be to create a meaning which the words do not convey; added to which, this construction is borne out by the concluding paragraph, in which Dr. Ure speaks of the "large amount of *potential* ammonia" in this guano. Truly 75 per cent. is a large and potent amount of ammonia to exist in guano, especially when we consider that the pure sesqui-carbonate of that alkali contains only 28·81 per cent. of ammonia, according to Mr. Phillips, the accuracy of whose analysis is unquestionable.

We should like to know the reasons which induce Dr. Ure to value guano by the quantity of ammonia it contains, and why he selects this body as the standard by which to estimate its power as a manure. Is Dr. Ure unaware that Liebig, the great maintainer of this doctrine, has lately considerably modified his views on this subject? Is he also ignorant of the uncertain and fleeting effects of sulphate of ammonia in many cases, and of its positively injurious operation, unless applied in very minute quantity, in others? We would ask the doctor whether the phosphate of lime in guano is to be overlooked as a manure, or whether we ought not rather to base on this article the commercial value of guano—its superiority over bones being, we imagine, easily accounted for by the minuteness of its state of division? On this

head we could bring forward a mass of practical evidence, and much of it from the doctor's own native land, which might astonish and enlighten him. With respect to phosphate of lime, the average African guano is quite on an equality with that from Peru.

Are we then to regard this statement of Dr. Ure as an error springing from ignorance, or as dictated by something worse? If individuals practically acquainted with chemistry are unable to understand a certificate drawn up for the purpose of describing the chemical qualities of an article, we ask, in the name of common sense, of what use can such a certificate be, either to the seller or the buyer of that article?—can such a certificate, in short, be of any other service than to enable the one party to mislead and dupe the other? May this not have been the sole end, aim, and intention for which this document was issued forth to the world?

We leave the public to judge for themselves—"sub judice lis est."

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

ON THE USE OF ARSENIC IN MEDICINE.*

DISCUSSION AT THE SOCIÉTÉ MÉDICALE DU TEMPLE.

THE danger presented, in our opinion, by the employment of arsenic in pharmaceutical preparations, induces us to make known the different opinions expressed in a medical society relative to the use of this poison.

M. BÉNET.—For some time the medical journals have announced, as new, the treatment of intermittent fevers by arsenic. However, this mode of treatment has, from a very remote period, been in use in India, principally against tertian fevers. The English physicians treat these affections thus, and I have myself employed arsenic a very great number of times in such cases, and always with success. In France, there is some hesitation and doubt as to whether it is safe to make use of arsenic as a medicine. There would, however, be a great advantage in employing a remedy whose efficacy is well proved, and which is much cheaper than sulphate of quinine.

M. AMÉDÉE LATOUR. — It is very recently, indeed, that attention has been again called to the employment of arsenic in intermittent fevers. It is M. Boudin who has extolled this medicine among us. But what M. Bénet has just said, M. Boudin has also said. He did not pretend to have discovered this mode of treatment, but only to have made it better known in France than it was, notwithstanding the works of Foderé, and several others.

M. SZOKALSKI. — I have lately been attending a patient afflicted with intermittent fever, and, after having in vain treated him with sulphate of quinine, I cured him by means of arsenic, which I gave him in the dose of one-tenth of a grain, mixed with pepper. At the end of ten days the patient relapsed; I adopted the same means, and with the same success. I had an opportunity, in Poland, of proving the efficacious action of this treatment. Confined in a fortress, and being in want of medicines, I frequently had recourse to arsenic, and I owe to it a great number of cures.

M. BOURRIÈRES.—An analogous fact has just taken place in the Faubourg du Temple. A lady, treated without success by sulphate of quinine, was cured by arsenic, in the dose of five centigrammes dissolved in 500 grammes of water. She took a teaspoonful of this solution, morning and evening, in a half a glassful of *eau sucrée*. Before the twelfth spoonful, the fever had disappeared.

M. BONNAFONT. — When sulphate of quinine fails, it is because the dose is not raised sufficiently high. There need be no fear in going to two grammes. Then success is certain.

M. AMÉDÉE LATOUR.—It is not so much a question as to whether arsenic cures—that appears indisputable—but as to whether it is not dangerous to popularise this means.

M. BÉNET.—What danger is there in prescribing 24 pills containing together about 25 centigrammes of arsenic, and of which only two are to be taken in a day? Even if the patient were to take the fancy of doubling or tripling the dose, what serious inconve-

* *Journal de Chimie Médicale*, March, 1845.

nience could result from it? Besides, do we not daily employ medicines which are quite as dangerous?

M. Géry.—When we possess a means so good as sulphate of quinine, what is the good of seeking one which may become injurious? But when sulphate of quinine has failed, success has been obtained with the extract of quinquina, or quinquina itself in its natural state. Thus, therefore, without leaving the preparations of quinquina, all the requirements of therapeutics may be satisfied.

M. Felix Legros applauded the philanthropy which dictated the fears relative to the employment of arsenic. But be it observed, that *materia medica* has long been in possession of this remedy for other diseases than intermittent fevers. At St. Louis, it is made frequent use of in certain cutaneous affections. He thinks that this medicine should remain in therapeutics.

M. Forget.—The employment of arsenic is imprudent: for we can never determine beforehand the precise limits of the action of a medicine when we do not know the constitution of the patients. Besides, to justify the use of the means in question, facts, and not assertions, are requisite. The case just mentioned of a cure obtained by arsenic after fifteen days' useless employment of sulphate of quinine, might, in strictness, be an ordinary case. In fact, a medicine does not always act immediately; it must be assimilated, identified, so to speak, with the organism, molecule to molecule: now, a certain time is required for the accomplishment of this saturation. If, tired of awaiting this result, another medicine be given, and if at the same time cure be effected, one may be led to attribute to the last remedy employed that which is only a delayed effect of the first.

M. Szokalski.—In the case which I have mentioned, it might, in strictness, be supposed that I have wrongly attributed to arsenic the first cure obtained, when the sulphate of quinine had been previously employed. But when the relapse occurred, arsenic was given at once; the patient certainly was not then saturated with the salt of quinine; still the result was as prompt and satisfactory as the first time.

M. Foy said that he had read, in the *Dictionnaire de Matière Médicale*, that the prolonged use of arsenical preparations gave rise to hypertrophy of the heart, liver, &c.

M. Bénet.—M. Forget has asked for positive facts proving the efficacy of arsenic. Here they are: I have myself prescribed arsenic for five years, and have always found it attended with success. I declare, moreover, that I have not observed, at the conclusion of its use, any affection of the circulatory apparatus, nor of the liver, nor of any other organ. Thus the objection mentioned by M. Foy is only an hypothesis.

M. Géry.—In a medico-legal point of view, and in the face of the numerous crimes which are committed, new and legitimate considerations are opposed to the employment of arsenic as a medicine. In case of death, traces of this substance will be found in the organs, which may give rise to an erroneous suspicion of poisoning, or disguise it if it be true.

M. Guide.—I have yet to find a case perfectly invincible by sulphate of quinine. Its pre-eminence over arsenic is therefore, to me, a well demonstrated fact, and I will not dwell on it here. But it has been said, that the employment of arsenic is especially advantageous in the country: now, in my opinion, it is there, especially, that it would be dangerous. The country practitioner seldom visiting his patient except at intervals of several days, is obliged to leave him a supply of medicine sufficient to last until his return. It is known, besides, that peasants may take liberties with the remedies which are prescribed for them; they often think to do well in taking in one dose that which they have been told to take in several days. We cannot, therefore, without danger, put a certain dose of arsenic into their hands.

M. Bonnafont.—In villages, where intermittent fevers are endemic, all the peasants have by them sulphate of quinine in packets or in pills, and administer it to themselves without the order of a physician. As to arsenic, its use is certainly attended with danger. How is it, moreover, that a medicine proved, it is said, by so many years of success, cannot yet be patronised among us?

M. Bénet.—In consequence of the spirit of routine. How is it, also, that morelle is regarded as dangerous, whilst under no climate, developed in the sun or in the shade, has it any injurious qualities? It is very good to eat in salad, since I have eaten it myself, and have given it to one of our colleagues.

It is evident from this discussion that opinions are still divided, and that some practitioners do not fear to extol the use of a poison which may cause the most serious danger.

VALERIANATE OF ZINC.*

A communication was read from Mr. Curtis, in reference to the valerianate of zinc, in

* *Medical Society of London*, Feb. 10, 1845.

which it was stated that he had administered this medicine with great advantage in a variety of cases of tinnitus aurium, nervous deafness, amaurosis, and muscæ volitantes. The dose was a grain. He introduced the medicine to the Society with the view of inducing the members to try it in cases of nervous debility; and as, at present, it was not easily procurable in London, he had placed some of the medicine in the hands of the President, to supply any gentleman who would wish to give it a trial. This remedy had the advantage of having mineral and vegetable properties.

Mr. Hird had employed this medicine in two cases of hysterical neuralgia. In one case it was of advantage.

A Member had also used the valerianate of zinc in a case of brain affection closely resembling delirium tremens. It was of much benefit.

RADIX ONONIDIS SPINOSÆ IN AMENORRHŒA.*

BY DR. J. BERNAYS.

I KNOW a sure remedy in cases of suppressed menstruation, formerly kept as an arcanum, which I have communicated to several medical men, who have found it serviceable in many cases of their practice; this remedy is radix ononidis spinosæ in powder. Its effect is truly surprising, especially when infusum sennæ is employed previous to its administration. The active constituent part of this root seems to be a friable matter which is of a pungent taste, occasioning salivation, which I prepared from it in the same manner as the resin of jalap is extracted. No alkaloïd is present in the root.

TREATMENT IN CASES OF BURN.†

DR. WILLIAM JONES, of Lutterworth, has sent to us for publication, the following fact:—"About twenty-five years since, I was called to a youug lady who was dreadfully burnt on the thighs and abdomen. It appeared to us a hopeless case. It immediately struck me that the administration of a very strong *opiate* afforded the *only shadow* of a *chance* for the patient. That, therefore, I immediately gave her. A tranquil sleep resulted. When its effects went off I repeated it, and I was quite surprised not only to see how little she suffered, but how kindly the burnt parts cicatrized and healed. I have always since used it with great advantage.

* Buchner's *Repertorium*, XXXV. 3, 1844.

† *Lancet.*

POISONING BY ARSENIC TREATED WITH HYDRATED OXIDE OF IRON.*

BY DR. KRAFFT.

THE chimney-sweeper K——, of B——, kept in the house a mixture of white arsenic and flour, for destroying rats. His sister one evening, during his absence, unwittingly prepared for the family a meal-pap for supper, with the above mixture. Of this the three children partook, about eight o'clock in the evening; one a boy of 13, another of 10, and the last a girl of 11 years old; the remainder was swallowed by the aunt. The children ere long felt unwell, and hurried to bed, but shortly after began to complain of severe pains in the belly, and sense of urgent constriction referred to the stomach. The aunt was similarly attacked. Under aggravation of these symptoms the whole party were seized with violent and continued vomiting. About this time, 10 o'clock P.M., K—— returned home, and learned the cause of the mischief. He thought to subdue it by administering copious draughts of milk. But as the vomiting persisted, and the pains became more intense, he got alarmed for their safety, and sent for Dr. Krafft at one o'clock in the morning. From the statement of the messenger, and the nature of the symptoms, there could be no doubt as to the fact of arsenical poisoning; the doctor therefore ordered hydrated oxide of iron to be exhibited to each of the children, in the dose of half a table-spoonful every half hour, and to the aunt a whole table-spoonful at like intervals, with an injunction that word should be sent him in the course of the morning of the result, himself being laid up with influenza at the distance of a league. He received intelligence in the afternoon that the three children were completely recovered; but the aunt still suffered from pain in the stomach, headache, and tremors of the limbs, for which ten leeches to the epigastrium, and internally an oily emulsion, were prescribed. During the day he paid the family a visit, and found all well, comfortably sitting round the stove. The parent spoke in the highest terms of the value of the antidote, and remarked that the girl and younger boy, just convalescent from an attack of influenza, were almost exhausted with the frightful cramps, lying stiff, with their bodies drawn backwards, the hands and face cold, covered with a clammy sweat, and apparently near death; while the two others seemed almost worn out through continued vomiting, sense of choking, and

* Casper's *Wochenschrift*, Oct. 1844, and *Medical Gazette.*

racking pain of the belly. They had, however, scarcely taken two doses of the hydrated oxide before improvement was visible, which proceeded steadily with each spoonful. The pain of the belly was somewhat obstinate, but the vomiting ceased at once, and was followed by frequent liquid stools; after which the children slept quietly till morning, awaking fatigued, but in good health. The aunt did not get well so rapidly, though nothing more was required in the way of medicine than the leeches and emulsion. The quantity of arsenic contained in the pap could not be ascertained, as K. had deposited the remainder of the rat-poison in the ground.

[It is to be presumed that the copious draughts of milk and water must have served to dissolve whatever arsenic remained in the stomach, otherwise the hydrated peroxide of iron would have proved effectless.—Ed. *Med. Gaz.*]

ON THE PREPARATION OF CITRIC AND TARTARIC ACIDS.*

BY THE EDITOR OF "THE DUBLIN HOSPITAL GAZETTE."

In the preparation of citric and tartaric acids it is necessary to employ a precaution similar to that just described in the preparation of phosphate of soda. If cold lemon-juice be poured upon chalk, the carbonic acid liberated by the union of the citric acid with the lime, will not entirely escape, but will in part unite with a portion of the chalk, forming bicarbonate of lime, which will dissolve in the washings. Now, in this case, the inconvenience will result from the whole of the lime used not being present in the precipitate, for the quantity of dilute sulphuric acid used for the decomposition of the citrate of lime formed, is measured by the quantity of chalk employed. You are to use eight times the weight of dilute sulphuric acid, as of the carbonate of lime added to the lemon-juice, because it is calculated, that in this proportion there is just the amount of real acid necessary for saturating the earth. But if a part of the lime be carried off in the supernatant liquid, under the form of bicarbonate, it is evident that enough will not remain to neutralize the given quantity of sulphuric acid. So that when this acid has decomposed the citrate of lime, and freed the citric acid, the liquid which will dissolve the latter, will likewise contain a notable quantity of sulphuric acid, sufficient, upon evaporation, to char the crystals of citric acid, and render them discolored. It is necessary, therefore, for the success of this preparation, that the whole of the lime employed, should exist in the precipitate either as *citrate*, or carbonate, if an excess of the latter be used; to gain this end, all the carbonic acid liberated by the lemon-juice should be expelled, and to force its expulsion, the lemon-juice should be added to the chalk at a boiling temperature.

The necessity for the employment of a boiling heat is equally the case in the preparation of tartaric acid; but the theory of its cause is so nearly identical that it is not necessary to describe it.

Mr. Philips, junior, has shown (see the first number of this Journal,) that this tendency of carbonic acid to form soluble super-salts is commonly the occasion of a considerable loss in the preparation of the saccharated carbonate of iron.

I may be permitted, in conclusion, to notice, that it is this property of carbonic acid which most probably influenced the compilers of our pharmacopœias to direct the solutions of sulphate of magnesia and carbonate of potash to be mixed boiling, in the manufacture of carbonate of magnesia. The *magnesia alba*, or carbonate of magnesia of commerce, is a sub-carbonate, consisting of a combination of neutral carbonate of magnesia and hydrate of magnesia. The neutral carbonate of magnesia is a crystallisable salt, which may be frequently observed depositing in the bottles containing solutions of the super-carbonate introduced by Sir James Murray, and so much employed in pharmacy at present. This neutral carbonate is decomposed when put into water; if the water be cold, it is decomposed into *magnesia alba* and bicarbonate; if the water be boiling, it is resolved wholly into *magnesia alba*, or the sub-carbonate, the excess of carbonic acid being expelled.

Now, precisely a similar series of changes occur when solutions of sulphate of magnesia and carbonate of potash are mixed together. These neutral salts yield by double decomposition the materials for producing neutral salts. But the neutral carbonate of magnesia is decomposed by the water, and it depends on the temperature whether the whole of the magnesia be precipitated as a sub-carbonate, or a portion of it be washed away in the form of soluble bicarbonate.

* From *The Dublin Hospital Gazette*, March 1, 1845.

AMMONIA GIVEN OFF BY ALBUMINOUS FLUIDS.*

M. Turck communicated a memoir to the Academy of Sciences, on the chlorides oc-

* *Gazette Médicale de Paris*, Feb. 8, 1845.

curring in albuminous liquids. All the albuminous liquids he has yet examined, namely, the saliva, blood serum, and white of egg, are continually disengaging ammonia. The formation of this gas is due, he thinks, to the simultaneous presence in these liquids of muriate of ammonia and caustic soda, which react unceasingly one upon the other, until the soda completely disappears. In fresh albuminous fluids there is not the least trace of chloride of sodium; it is only in virtue of the above reaction that the salt is progressively formed. In proportion as the soda replaces the ammonia a very singular phenomenon of crystallisation is exhibited, which merits the attention of the crystallographer. He concludes by stating that the evolution of ammonia from albuminous liquids performs an important part in the animal economy.

CASE IN WHICH MATICO LEAF FAILED TO ARREST HÆMORRHAGE.*

BY CHARLES HOGG, ESQ.

I WAS called, on the 23rd inst., to see a child, twenty months old, suffering from a severe attack of pneumonia. Being of full habit of body, I advised the immediate application of half-a-dozen leeches; and not wishing the bleeding to continue after the first hour, the usual and ordinary remedies were employed, such as lint, with compress, vinegar, saturated solution of alum, beaver from a hat, cotton, wool, &c., &c., without the slightest effect. I also employed Mr. Gossett's ingenious method—viz., cutting a small piece of glazed card about the size of a silver penny, wiping the wound dry, and applying it immediately, pressing it firmly with the finger for a short time. This also failed, after repeated trials. I then persevered, for at least an hour, with the matico leaf, carefully following the directions given by the correspondents in *The Lancet*, but all to no purpose. The blood had now been flowing freely for upwards of six hours, amounting to ℥x or ℥xij., I judge so, from the saturated rags and linen. Evident symptoms of exhaustion becoming manifest, I felt called upon to adopt some more decisive means. I may observe, that the child was exceedingly restless, accompanied with an incessant cough, and two of the wounds evidently gave out arterial blood. I resorted to actual cautery, which succeeded in a short time. It appears to me much simpler, and is easier accomplished, than the twisted suture, advised by Dr. Boldes.

* *Lancet.*

OINTMENT TO AID THE GROWTH OF HAIR.*

BY J. KALLHOFERT.

THOUGH I consider the formula of an ointment communicated in Buchner's *Repertorium*, vol. 34, No. 2, by Mr. Stegge, apothecary to the court, to be far better than the following, which is my own, I think I ought to make it known, as several eminent physicians, especially my friend, Dr. Sedelbauer, and myself, have found it highly efficient; individuals who had lost almost all their hair, by the use of this ointment preserved the few remaining, and promoted the growth of new ones.

Unguentum contra Alopeciam.

℞ Extracti Chinæ fusc., grana xv.
Rotanhiæ ℥v., grana viij.
Succi citri med., gtæ. xxx.
Balsam Indic. ℥ss.
„ Nucist. expres. ℥ss.
Extract. Bardan. ii. Cons. gr. x. ana drachm. duas.
Camphor. triturat., grana xv.
Alcoholis 30° B., drachm. duas.
Medull. bovis, unciam unam et semis.
Ol. Provinc., unciam unam.
„ Odoration., q. p.
Misce fiat unguentum lege artis.

VOLATILE PRINCIPLES OF COAL-TAR.†

COAL-TAR yields on distillation a large quantity of thin, dark-colored, volatile oil, which, when agitated with dilute sulphuric acid to remove ammonia, and twice rectified with water, becomes nearly colorless; it is very volatile, much lighter than water, very inflammable, and possesses, in a high degree, the property of dissolving caoutchouc, on which account it is very extensively used in the manufacture of water-proof fabrics containing that material.

It appears that this coal-oil, or artificial naphtha, is a mixture of a number of hydrocarbon, some of which possess basic properties, and form crystallisable compounds with acids; at least, a variety of different substances have been procured from the liquid in question; the remark formerly made respecting the doubtful pre-existence of substances thus obtained also applies here. The great bulk of the coal-oil appears to be made up of a neutral volatile oil, resembling eupoine; the basic principles to be described constitute but a small part of the whole.

* Buchner's *Repertorium für die Pharmacie*, xxxvi. 1.

† *Lancet*, Feb 1.

ON THE ARABIC TREATMENT OF SYPHILIS.*

BY DR. JOANNES.

This mode of treatment is employed by the physicians of the south of France, in cases of inveterate syphilis, when they have resisted regular medical treatment.

It is said to have been imported into Marseilles about one hundred and fifty years ago, by a Spanish apothecary. It has been handed down by tradition in this town and the surrounding localities.

It consists of pills, an opiate, a draught, and a peculiar regimen, known under the name of dry diet.

ARABIC PILLS.

℞ Quicksilver and bichloride of mercury	}	*aa* 2 grammes or parts.
Senna	}	*aa* 2 grammes
Root of pyrethre		
Agaric		
Honey		q. s.

The vegetable substances are reduced to powder; the quicksilver is accurately divided with the bichloride of mercury, until the metallic globules have completely disappeared; afterwards, with the honey, a mass is made, which is divided into pills of 20 to 25 centigrammes. The patient takes two or three per day.

ARABIC OPIATE.

℞ Sarsaparilla	150	grammes or parts
Squine and senna	90	"
Torrified shells of hazel-nuts	30	"
Cloves	4	"
Honey	q. s.	

From 8 to 15 grammes are prescribed, morning and evening.

The sudorific draught is made with sarsaparilla and squine; it is the only drink allowed to the patient, who drinks a quart or two of it in the 24 hours.

The regimen consists in rejecting the ordinary aliments, and in using only crumpets, dried raisins, nuts, dried figs and burnt; it is generally regarded as an indispensable accompaniment.

The following is the order according to which the remedies are administered: one pill in the morning, besides a glass of the draught; an hour afterwards the opiate in the dose indicated, with a second glass of the draught. In the evening, the same thing is repeated; the remainder of the draught is consumed in the course of the day.

The dose of corrosive sublimate which enters into the composition of these pills may seem too high; however, it has never been heard that these pills have produced any injurious effects.

The duration of this treatment seldom exceeds forty days. Sometimes ptyalism shows itself towards the end, but the treatment may thus be regarded as terminated.

At first, the patients accommodate themselves well to the diet imposed on them. Towards the end, they are tired of the monotonous regimen which they have followed, and await with impatience the period of resuming their ordinary mode of life.

This more energetic treatment, and accompanied by a much more severe diet than that which has been extolled by Felix, seems to be of a nature too profound to modify the economy in inveterate syphilis. It is in this respect it is worth knowing; but it must be of a very difficult application in general, at least if we may judge from what occurs in Paris, where serious difficulties are experienced in making persons attacked by syphilis follow, for a proper length of time, the very simple treatment by corrosive sublimate and protiodide of mercury.

* *Journal de la Société Pratique de Montpellier.*

ON THE PREPARATION OF PHOSPHATE OF SODA.*

BY THE EDITOR OF "THE DUBLIN HOSPITAL GAZETTE."

Phosphate of soda is prepared, according to the directions of the "Dublin Pharmacopœia," by forming a soluble super-phosphate of lime by the action of sulphuric acid on bones burnt to whiteness, and decomposing this super-phosphate by means of carbonate of soda; the excess of phosphoric acid unites with the soda forming phosphate of soda which dissolves, while insoluble phosphate of lime is thrown down, and carbonic acid is given off with effervescence.

Now, I have observed, that when the solutions of super-phosphate of lime and carbonate of soda are mixed cold, and the mixture filtered for the purpose of separating the precipitated phosphate of lime, the liquid, when poured into a Wedgewood dish, and placed in a sand-bath, becomes turbid as it grows warm, and the effervescence at the same time becomes renewed; these phenomena continue until the solution attains a boiling temperature, so that it is necessary to refilter before crystallisation is permitted to take place.

* From *The Dublin Hospital Gazette*, March 1, 1845.

The explanation of these facts is as follows:—when the excess of phosphoric acid decomposes the carbonate of soda, the carbonic acid liberated does not all escape, but some of it unites with a moiety of the lime, forming bi-carbonate of lime, liberating phosphoric acid which combines with the undecomposed phosphate of lime regenerating the super-salt. Now, both the bi-carbonate of lime, and super-phosphate of lime are soluble, so that when the solutions are mixed in the cold, the whole of the carbonic acid does not escape, nor does the whole of the phosphate of lime precipitate. A precisely similar reaction takes place when a stream of carbonic acid gas is passed through water holding phosphate of lime in suspension; the carbonic acid gradually dissolves the phosphate of lime. When this solution is boiled, the carbonic acid is driven off, the lime it was combined with reunites to the excess of phosphoric acid in the super-salt, and insoluble phosphate of lime is again formed. This is the reason why the mixed and filtered solutions of super phosphate of lime and carbonate of soda becomes turbid and effervesce upon the application of heat; phosphate of lime precipitating upon the escape of the carbonic acid which held it in solution.

To avoid the inconvenience, therefore, of being forced to refilter the liquid, it is necessary, in the preparation of phosphate of soda, to mix the solutions of super-phosphate of lime and carbon of soda boiling, when the whole of the carbonic acid will escape, and the entire of the phosphate of lime will become precipitated.

ON HYDRIODATE AND HYDROBROMATE OF SILVER.*

BY J. KALLHOFERT.

The above compounds are easily and quickly prepared, if a diluted solution of the fused nitrate of silver in distilled water be filtered, and if in a dark place, a very dilute solution of hydriodate or hydrobromate of soda or potassa be added to it; the silver is precipitated, which on being filtered, washed, and placed on white filtering paper, spread on clay, to become dry, the said preparations were obtained in the form of a woolly pale-yellow mass, which in the rays of the sun or candle-light, were immediately changed into black.

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* Buchner's *Repertorium*, vol. xxxvi., p. 1.

ON THE FATAL EFFECT OF HYDROCYANIC ACID, AND SUBSTANCES CONTAINING THAT BODY.*

BY DR. IGN. HAYN.

As the effect of this poison is so rapidly fatal, killing in a few minutes, we have not hitherto been able to confirm the fact, proved almost to evidence in animals, that the primary action of hydrocyanic acid was decomposition of the blood, and that the effect produced in the spinal marrow relative to its motory function, and in the vagus, by which, finally, paralysis of the respiratory system is induced, are only secondary ones.

The author, therefore, mentions a case in which the effect of the poison was slow, and the above-mentioned *modus operandi* fully corroborated:—

A middle-aged man, of a hypochondriacal disposition, found means of procuring aqua lauracerali, of which he drank one ounce and a half; when the symptoms of poisoning appeared, he did not deny having committed the crime. Not until after three hours was paralysis noticed in both the hands and feet; the head hung down, he being unable to raise it; involuntary exretions of feces and urine; the extremities were motionless and cold, but not devoid of sensation; the pulse little, the voice hoarse, but distinct. The full consciousness of the patient was remarkable. He felt pleasure in finding his debility to be on the increase, and notwithstanding the antidotes administered, he calmly expired in the evening, in consequence of the paralysis of the lungs. On dissection, the blood was found to be of the peculiar strikingly dark color and unctuous condition, BUT NO ODOR OF PRUSSIC ACID COULD BE PERCEIVED.

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* Schmidt's *Jahrbücher*, xli. 2, 1844.

ON THE INJECTION OF TANNIN FOR THE CURE OF ANEURISM.†

BY R. A. VARICAS.

A new and practicable means of cure for this disease has been for a long time desired; indeed, few in the profession but have had occasion to deplore the want of one, where the ligature is considered impracticable. Often has it been their painful lot to stand by, powerless, watching the unerring progress of the disease to its fatal termination; and even when the ligature has been determined upon, how frequently are its results considered hazardous or doubtful! Aneurism by anastomosis cannot always be cured, although

† *Lancet*.

the carotids themselves be tied. Others cannot be reached by the needle with certainty and safety, from the nature of their situation and connexions. Even disease of the coats of the vessel, and excessive debility, will be sufficient reasons to cause one to pause and wait the efforts of nature either to work the cure or produce the awful catastrophe. How seldom the former, and how frequently the latter, every one of experience cannot but have been a too frequent witness! Under these circumstances, I am emboldened to lay before the profession a new method of producing an artificial coagulum of blood, to any extent, in the interior of vessels; and hope, at the same time, it will receive a dispassionate consideration.

From the known chemical action of tannin upon albumen, and from its facility of application, I am induced to recommend its injection into vessels thus diseased. The effect is to produce the coagulation of blood, and thereby obstruct the circulation in that direction. This desirable object has been attempted before by other means, but such have been proved inefficacious, inasmuch as the coagulum obtained has been too inconsiderable to carry out the end in view.

There are a few precautionary measures to be noticed in the performance of the operation of injection; I will therefore proceed to explain the manner in which it should be conducted, and the kind of instrument adapted to its application.

A small syringe, either of silver or glass, should be fitted accurately, with a long, very fine tapering canula and stilet, much curved, and furnished with a stop-cock. The vessel having been cut down upon sufficiently to enable the operator to arrive at it easily with his finger, the canula is to be directed along it into the artery, with its concavity turned towards the heart; and the syringe, previously filled with a saturated solution of tannin, (all air being, of course, excluded), should then be attached to it, whilst an assistant, compressing the artery at its distal side, so as to retard the circulation through it, the process of injection should commence, with a force proportionate to the remoteness from, or propinquity to, the heart; also to whether it be performed on the distal or cordal side of the aneurism. In aneurism by anastomosis, or nævus, sufficient power should be applied to carry the injection some way into the vessels, and the canula should be introduced with its convexity towards the heart.

As soon as the aneurismal pulsations cease, the pressure should be gradually withdrawn from the artery, and time be allowed for the heart to exert its force against the coagulum: if the pulsations of the sac recommence from this having taken place, the injection should be performed anew, before withdrawing the instrument. It is possible that, after the lapse of time, the coagulum (if formed) will contract disproportionately with the sac, and pulsations would of course return in it; but even were this to occur, the facility of performing the operation would authorise its reapplication.

Injection of solution of tannin is equally applicable to wounds of arteries. In such cases, pressure being applied to both sides of the artery, the canula should be introduced into the wound, and the injection conducted at the same time that the pressure upon the artery is being gradually withdrawn.

In varicose conditions of veins it might also be used with equal, if not greater prospect of success than those means already in use, and certainly with less probability of supervening inflammation, than those by ligature, caustic, needles, &c. In such cases, the circulation should be arrested at the side of the vein nearer the heart (for obvious reasons) before commencing the operation.

When the freedom of this proposed mode of cure from all the disadvantages connected with that by ligature, such as destroying the cellular connexions, the ulcerative process, and accompanying constitutional irritations, secondary hæmorrhage, and many others with which every experienced surgeon is acquainted, is taken into consideration, I think it cannot but be considered as worthy of trial by such as have it constantly in their power to make the experiment without any possibility of accident.

I intend instituting some experiments upon the lower animals with this process, and when I have done so, will publish the results.

ON REINSCH'S TEST.*

BY BERZELIUS.

I MENTIONED in my annual report for 1843, p. 174, the method of Reinsch, who directs the precipitation of the arsenic by means of copper, which this chemist considers as a particularly delicate test. This method was examined by Audouard, who found that no perceptible result could be obtained by this process from a mass which, on being subjected to the Marsh's apparatus, gave most distinct traces of arsenic.

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* Berzelius's *Jahresbericht*, 1844, p. 268.

PROCESS FOR THE DETECTION OF ARSENIC.*

BY H. LETHEBY, M.B.,

LECTURER ON CHEMISTRY AT THE MEDICAL SCHOOL OF THE LONDON HOSPITAL.

Of the many processes which have from time to time been suggested, and more or less lauded, too, for the detection of arsenic in cases of poisoning, I am confident that but few of them have been put to the test of experiment with organic mixtures, by way of determining their applicability and delicacy; and the manipulator who is accustomed to seek for arsenic, often in very small quantities, in the contents of the stomach, or in the tissues of poisoned animals, has learned to look with distrust upon every new suggestion, relying more upon his own tact with regard to some particular process to which he is accustomed, than caring to re-educate himself by adopting any new one. I put forward, however, the following observations, by reason of some little experience in this department of chemistry, and I am convinced, to speak in a general way, that the apparatus described in the *Lancet* of Feb. 1st, and which was proposed by Berzelius, for the detection of arsenic in cases of poisoning, is not at all applicable, and more particularly in the hands of a novice; for you cannot by that means combat the frothing of the liquor, or prevent small particles of the zinc escaping from the central tube into the acid liquor without, and so liberating and losing the arseniuretted gas. Again; to be certain of decomposing this gas, the exit pipe must be heated from the commencement, and if care has not been taken to expel the air, it will be sure to explode. Moreover, there is tact required in the application of the heat, or we either get the tube softened and bending, and perhaps blowing out, or the arsenium, by the great heat, is carried over, or else the gas is not decomposed at all; and if it so happen that the tube is a little too small, or gets closed up by a film of arsenium, the gas then bubbles through the acid liquor and is lost, and we cannot make a quantitative analysis; all these are difficulties in the way even of the experienced, but they are most formidable to the novice.

In place of this process, which I have long given up, I adopt the following, which is a modification of Lassaigne and Reinsch, and I never find the least difficulty in detecting even very small quantities of arsenic:—Supposing I have the contents of a stomach to test. They are first boiled in distilled water, slightly acidulated with acetic acid, then filtered, and again and again boiled and filtered; mix those liquors and divide into two equal parts. Evaporate one part nearly to dryness, and then heat with about twice its bulk of sulphuric acid, until it is quite charred, and begins to evolve the acid. This mixture is then to be diluted and introduced, by little and little, into a large Woulfe's bottle, from which hydrogen is being slowly evolved from pure zinc and dilute sulphuric acid. The gas is to be conveyed by means of a bent tube, drawn fine at its remote point, to the bottom of a tall glass, containing a solution of nitrate of silver; it will occasion a black precipitate in it, and, by taking the precaution of not allowing the gas to come over too quickly, and by means of the fine tube to make it pass through in very small bubbles, we shall be certain to decompose the whole of it. To this black turbid solution of nitrate of silver is then to be added muriatic acid, until all the silver is precipitated, and a little of the acid remains in excess; boil for a few minutes, and filter and evaporate the filtered liquor to dryness; the residue, if there be any, is to be dissolved in a little distilled water, and carefully precipitated by ammoniacal nitrate of silver; if any arsenic had been present, it would by this process have been converted into arsenic acid, and this with the ammoniacal nitrate would produce the red precipitate of arseniate of silver, every 464 grs. of which is equivalent to 100 grs. of arsenious acid, or 76 of arsenicum. This arseniate of silver may be again reduced by means of black flux, or charcoal, and preserved for evidence. The other half of the suspected liquor is to be slightly acidulated with muriatic acid, and boiled for an hour or two with a known weight of clean copper in strips; if arsenic is present, it will attach itself to the metal, giving it a black coating. The copper being removed, dried, and weighed, indicates how much heavier it may have become, and to be certain of this being arsenic, they should be placed in a small green glass tube, closed at one end, and by means of a spirit lamp, with a large flame or a blow-pipe, kept at a red heat for a little time; if arsenic be there, it will sublime either as a white or black ring. After this, the copper is to be again weighed, and its loss estimated.

Supposing, however, that the substance to be tested were a piece of muscle or liver. It is to be charred by means of sulphuric acid, as in the first case, and the liquor diluted and divided into two portions; the one to be treated by Lassaigne's process with the nitrate of silver; the other to be nearly neutralised by carbonate of soda, and

* From the *Lancet*, March 15, 1845.

boiled, as in the last case, with the clear copper.

Now the advantages of these processes are, that they are readily applicable, and will detect small quantities. The frothing is obviated, and you are not likely to be annoyed by the fallacies which antimony may occasion. Not but that antimony will decompose the nitrate of silver, and be deposited on the copper like arsenic; but then the subsequent stage of the processes determines this.

In conclusion, I may mention as a caution, that there is again in the market a large quantity of sulphuric acid contaminated with arsenic; I have found, in a recent sample, as much as one grain in the fluid ounce.

URIC ACID EXCRETED BY BUTTERFLIES.*

According to Heller, butterflies excrete more uric acid in proportion to their bulk than any other creature. It is chiefly the product of the metamorphosis which goes on in the pupa; the reddish or yellow liquid which butterflies void during or after emerging from the pupa into the perfect state consists principally of urate of ammonia.

STEAROPTEN IN OLEUM IMPERATORIÆ.†

BY J. KALLHOFERT.

I HAVE lately observed a curious circumstance in about two drachms of the above oil. To preserve it from becoming volatilized, I placed it in a cold cellar: in about a fortnight I examined the same oil and found on the upper empty part of the sides of the glass, white, shining, densely grouped, scaly, crystalline lamellæ, resembling those noticed in winter in ol. foeniculi; the odor which they exhaled was weaker than that of the oil itself; the taste, however, was more burning and persistent. I regret having been prevented from examining them more fully, by an error of a female servant, who, with the intention of cleaning the laboratory, during the time that I was occupied in another part, threw the crystalline lamellæ on the ground. I may, perhaps, succeed in again obtaining the same crystals, and will then examine them.

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* *Oesterreich. Medicin. Wochenschrift*, 1844, p. 1048, *and Medical Gazette*.

† Buchner's *Repertorium*, vol. xxxvi., p. 1.

ADULTERATION OF IODIDE OF POTASSIUM WITH CARBONATE OF POTASSA.*

EXTRACT OF A LETTER FROM M. DESTOUCHES TO M. BOUDET.

M. RIVAUD, a distinguished pharmacien of Saumur, wishing to notice the adulteration (by voluntary fraud or ignorance) of a chemical product sent to him from Paris, has requested me to examine it simultaneously with himself; he has sent me a bottle, bearing the seal and address of M. Paton, pharmacien at Batignolles, and labelled *Iodide of Potassium*.

This iodide is amorphous, and run into plates after the manner of *cautery stone*. It is of a milky white; the taste is pungent and alkaline. It quickly restores to blue powerfully reddened litmus paper: it effervesces with the feeble acids; it does not contain chloride; the alteration is due to carbonate of potassa.

In order to determine the intrinsic value of this product, it was compared with pure iodide of potassium, procured from the firm of Robiquet and Co.

5·0 of Robiquet's iodide (No. 1) were dissolved in 100 grammes of distilled water.

5·0 of the doubtful iodide (No. 2) were likewise dissolved in 100 grammes of distilled water. This solution was slightly opalescent.

Both solutions were heated by a solution of acetate of lead, until cessation of precipitate. To No. 2, dilute nitric acid was added to remove the carbonate of lead formed, and in order to put the two precipitates in the same condition, nitric acid was likewise added to No. 1.

Both precipitates of iodide of lead were well washed with cold distilled water, and thrown on filters, dried and weighed. The following was the result obtained:—

No 1 gave 6·62. No. 2, 5.18.

Thus—6·62 : 5·00 :: 5·18 : 3·91.

Or nearly 22 per cent. of carbonate of potassa, and only 78 of iodide of potassium, an enormous adulteration, and so much the more reprehensible as it is a medicine on which medical practitioners ought to be able to depend, and which is administered only in small doses.

The results obtained by M. Rivaud, by different means, are very nearly the same.

I beg you to be kind enough to insert this note in an early No. of the *Journal de Pharmacie*.

* *Journal de Pharmacie*, February, 1845.

PREPARATION OF SUBOXIDE OF COPPER.

BY DR. J. G. WITTSTEIN.*

The property possessed by several kinds of sugar of reducing oxide of copper to the state of suboxide, though known for a long time, has, however, never been made use of as a means of preparing the pure suboxide of copper. A few years ago Böttger, the able chemist of Frankfort, to whom we are indebted for many interesting discoveries, communicated, in his contributions to *Physic and Chemistry*, "*A new, untiring, and easy method of preparing, in the humid way, pure suboxide of copper*,"† according to which 9 parts of humid (pressed) hydrated oxide of copper are heated with solutions of 27 parts cane sugar in 60 parts of water, and 18 parts of caustic potassa in 60 of water. In following the above direction a fine suboxide of copper is, indeed, obtained; but, owing to a great waste of sugar and caustic potassa, the preparation becomes expensive. In conjunction with Tromsner,‡ I found that, of all species of sugar, milk sugar reduces oxide of copper to the state of protoxide with the greatest rapidity, and employ the same to obtain the above end, saving thereby as much material as possible: the method adopted is as follows:

I dissolve 1 part of crystallised sulphate of copper and 1 part of milk sugar in 10 parts of water to the filtered cold solution, adding, if necessary, so much of caustic potassa until the precipitate, thrown down at first, is redissolved; 3 parts of potassa ley of 1·333 spec. grav. are sufficient for that purpose. I heat the deep-blue liquid in a water-bath with constant stirring; this at first assumes a gray-green tint, becomes turbid, soon after turning brown, is gradually rendered limpid, and finally almost vermilion-red. arrived at that stage of the operation, I immediately remove the vessel from the fire, separate the precipitate from the yellowish supernatant liquid, wash and dry it. The suboxide of copper obtained in this manner is anhydrous possessing a pure and bright copper-red color; it is readily and completely dissolved in hydrochloric acid, and entirely free from organic matter. If the application of heat by the water-bath be continued for a longer time, the lively yellow-red (amorphous) precipitate passes over into the crystalline state, assuming a dark crimson-red color. Beyond this point, however, the heat ought not to be continued, or else a higher oxidation would occur, and the preparation present a blackish appearance. If the preceding method itself be deemed too expensive for preparing the suboxide of copper to be employed for certain purposes, then a number of copper sheets are heated to redness in a crucible, and the black coating thus produced scraped off from the copper, when the sheets are again heated and scraped, and the same treatment repeated until the sheets are as thin as paper. All the scrapings are then rubbed together, when they represent a gray and crimson-red crystalline powder, but which cannot contain more than $\frac{1}{20}$ grain of the suboxide.

* Buchner's *Repertorium*, xxxvi. 1.

† *Ibid*, No. 77.

‡ *Ibid*, No. 75.

ON CEYLON MOSS.*

BY M. SCHACHT, OF BERLIN.

For some time there has existed in commerce a lichen known under the name of Ceylon moss, which is employed in the same cases as carrageen.

This lichen is the *fucus lichenoides*, or *spharococcus*.

In order to ascertain the advantages to be derived from the employment of this moss for the preparation of a jelly, M. Schacht has made some experiments on the new plant, on carrageen, and on Iceland moss; and it results that, to obtain 100 grammes of jelly of the same consistence, it is necessary to employ 24 grammes of Iceland moss, 4 of carrageen, and 6 of Ceylon moss. The latter is less mucilaginous than carrageen, but it has the advantage over it of furnishing a less colored jelly.

A German physicist, Dr. Liegmund, has maintained, that this moss does not contain iodine, but the presence of this principle in it is easily demonstrated. For this purpose, this moss is incinerated, the residue is treated several times by hot water, the united liquids are evaporated to dryness. The solid mass obtained with alcohol is agitated, and evaporated. The solid product is placed in a glass tube, a few drops of concentrated sulphuric acid are poured on it, it is closed with a starched paper, heated, and the blue tint of iodine is very soon manifested.

By redissolving, in distilled water, the product of the evaporation of the alcoholic solution, and by precipitating it by nitrate of silver, the proportion of iodine may be determined.

In Ceylon moss, it is about 0 grammes 000·460; in carrageen, about 0 grammes 000·570; so that this last species contains more iodine than the former.

* *Journal de Pharmacie*, February, 1845.

THE PERCHLORIDE OF GOLD AS AN OUTWARD APPLICATION IN RHEUMATIC GOUTY AFFECTIONS.

Dr. WILCZEWSKI has recommended this preparation made into a salve with lard. It is said to relieve the pain often in a truly wonderful manner. The purple stains are speedily removed by washing the part with a little urine; a fact that was discovered by accident.

THE NEW METHOD TO PREPARE GRAY MERCURIAL OINTMENT.†

BY SIGNOR OROSI.

It has been observed that if, on preparing mercurial ointment, an addition were made, either of old ointment, or if rancid fat were employed, an irritant action on the skin was produced; hence Orosi deemed it necessary to prepare the Neapolitan (gray) ointment with entirely fresh hog's-lard and precipitated metallic mercury.

It is known that the reduction of mercury to its metallic state is generally brought about by means of either phosphoric acid or chloride of tin. Sig. Orosi preferred the latter, and proceeds as follows:—One pound of corrosive sublimate is to be dissolved in a sufficient quantity of boiling water, and the filtered solution mixed with an excess of chloride of tin and hydrochloric acid; a moderate heat is applied, and the whole shaken for some time, afterwards allowing the extremely finely-divided gray mercurial particles to settle. After having removed the supernatant liquid, the precipitate is repeatedly well washed with hot water, dried on blotting-paper, and mixed with the usually prescribed quantity of fat. Owing to the extremely finely-divided state of the precipitated mercury, the ointment is very soon prepared. The only difficulty to which this method is liable, is that the precipitated gray mercury is apt to run into shining globules, which is chiefly the case, when, after having removed the supernatant liquid, the precipitate is allowed to stand and dry. This running of the mercury, however, is to be obviated by rubbing with grease the sides of the vessel in which the mercury is to be precipitated. The ointment prepared according to Orosi's method, shows no mercurial globules on being examined under the microscope; it may, perhaps, contain a trace of oxide of tin, if the quantity of the hydrochloric acid employed were not sufficient, or the precipitate not washed with sufficient care. Orosi's mercurial ointment is, indeed, more expensive than the common Neapolitan ointment, but it has the advantage, that it is not in the least rancid, if recently prepared, and that, the mercury being once precipitated and properly washed, the preparation of the ointment requires no long rubbing.

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* *Medicinische Zeitung*, No. 11, 1844.

† *Gazetta Toscano delle Scienze Medico-fisiche*, 1844. Marzo.

ON SULPHATE OF COPPER AND AMMONIA, AND OIL OF TURPENTINE IN NERVOUS DISEASES.*

BY DR. KARACSONYI.

As the result of numerous experiments with the above substances, Dr. K. states, that if properly administered, and the doses augmented, they are highly active remedies in nervous affections; the sulphate of copper and ammonia seems to be less powerful than oil of turpentine, the former being only of service in St. Vitus' dance, whereas the latter was successfully used in epilepsy, and in cases in which the former failed to cure the St. Vitus' dance, the latter produced the desired effect. The action of the said substances is, however, connected with the following conditions:—

1. They must be administered in the requisite form. 2. The doses must be gradually augmented, in such a manner as not to incommode the patient; and until either the complaint is removed, or such phenomena are noticed as prove that the remedy has acted fully, or it is necessary to diminish the doses.

The following are the formulæ recommended by the author:—

℞ Sulphatis cupri ammoniacales, grana vi.
Aq. menthæ cripæ, uncias tres.
Tincturæ Theb aicæ, guttas sex.
Syrupi capilorum veneris, unciam dimidiam.

Take four times a day from 1—4 teaspoonfuls.

℞ Mellis despumati, uncium unam et semis.
Olei terebinthinæ, unciam semis.

Take four times a day 1—4 tablespoonfuls.

If the patients do not bear it well in this form, some distilled water must be added.

Dr. K. saw a case of universal callismus in which the doses of the sulphate of copper and ammonia were raised to 12 grains per diem, and taken without injuring the patient in any of his functions, causing neither pressure in the stomach, nor vomiting, diarrhœa, etc.

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* *Magyar orvos-sebészi, etc.*, June, 1844.

ON POISONING BY HYDROCYANIC ACID.*

BY DR. H. MEYER.

NOTWITHSTANDING it has been shown by the experiments of Emmert and Magendie, that the assertion hitherto adhered to, that narcotic substances produce an immediate effect on the whole nervous system, there is no doubt but these bodies act only after having first entered the vascular system; yet in the case of hydrocyanic acid we meet with many peculiarities and contradictory circumstances. To supply this deficiency, the author instituted a series of experiments on rabbits, pigeons, lizards, frogs, tritons (triton litoreus, a molluscous animal), and fishes, with freshly-prepared Ittner's hydrocyanic acid, from all of which it appeared—

1. That in the place to which the hydrocyanic was applied, it had a paralyzing action on the peripheric nerves, with which it comes into contact; that is to say, it suppressed sensation and motion, and occasioned congestion, and augmented secretion (which was chiefly the case in the cavity of the mouth).

2. It acted only when received into the vascular system. On trying the heart and the large vessels, hydrocyanic acid did not act, though the communication between the peripheric nerves and their centre remained undisturbed, and the action of the acid began only after the circulation of the blood was restored.

3. The rapidly fatal effect of the hydrocyanic acid was by no means so instantaneous, like a flash of lightning, as it was formerly believed. (It usually required 1½ minute.)

4. The fatal effect produced by hydrocyanic acid was owing to the paralysis of the heart, induced by the blood (almost in the same manner as air acts when blown into the veins), it then lasted about ½ minute, till it produced the deadening effect, which is the time necessary for its arriving in the heart; whether the hydrocyanic acid was applied to the heart or to parts remote from it, the case remained the same. That this paralysis of the heart was the consequence of the topical action of the blood mixed with hydrocyanic acid on the heart, and not induced by the affection of the central parts of the nervous system, is proved on the one hand, by the almost instantaneous paralysis of the heart, on applying the poison to the lungs; and, on the other hand, by the absence of any of the phenomena usually attending the poisoning of the whole nervous system. Hence, it is that amphibia, in which, for the maintenance of life, the action of the heart is less material than in warm-blooded animals, are less rapidly killed by hydrocyanic acid.

5. The real nature of poisoning by hydrocyanic acid is analogous to that of other narcotics, and consists in a paralysis of the whole nervous system after previous irritation (of the vessels?). To obtain the just stated effect of hydrocyanic acid, and not the local one, the author administered the requisite quantity, with the precaution that no paralysis of the heart might be caused. The symptoms which appeared were tetanic fits, repletion of the venous system, and watery exudations in the serous sacs (indications of paralysis in the system of the vascular nerves), &c.

Notwithstanding it is generally believed, that in cases in which death is occasioned by hydrocyanic acid, the blood does not coagulate, the author observed that, in all animals killed by him, coagulation took place; and after the blood was allowed to flow out, coagulation was rapidly effected. In the blood cells, the author could never distinguish any change.

* Schmidt's *Jahrbücher*, vol. xli., 1844.

DELETERIOUS INFLUENCE OF THE EXCESSIVE USE OF TOBACCO.

BY DR. CHAPMAN.

TOBACCO is denounced by Dr. Chapman as a fertile source of dyspepsia. The *British and Foreign Review* remarks—

"On this subject an American physician may truly speak, *ex cathedrâ*. Although we cannot as yet pretend to compete with Americans in the practice of this disgusting vice, we see too much of it, not to know that, even in the small way here perpetrated, it is almost as injurious to the health of our young men as it is nauseous to the senses of their unsmoking victims. We almost wish they would, like Dr. Champan's senator, come to 'chewing and snuffing as well as smoking,' that the full abomination of the thing might cure itself.

"'By a member of Congress from the West, in the meridian of life, and of a very stout frame, I was some time since consulted; he told me that, from having been one of the most healthy and fearless of men, he had become 'sick all over, and as timid as a girl.' He could not even present a petition to Congress, much less say a word concerning it, though he had long been a practising lawyer, and served much in legis-

hative bodies. By any ordinary noise he was startled, or thrown into tremulousness, and afraid to be alone at night. His appetite and digestion were gone; he had painful sensations at the pit of his stomach, and unrelenting constipated bowels. During the narrative of his sufferings his aspect approached the haggard wildness of mental distemperature. On inquiry, I found that his consumption of tobacco was almost incredible, by chewing, snuffing, and smoking. Being satisfied that all his misery arose from this poisonous weed, its use was discontinued, and in a few weeks he entirely recovered.

"'Distressing as was this case, I have seen others, from the same cause, even more deplorable. Two young men were in succession brought to me for advice, whom I found in a state of insanity, very much resembling delirium tremens. Each had chewed and smoked tobacco to excess, though perfectly temperate as regarded drink. The further account given me was, that early in life, adopting the bad practice, it grew with their growth. Dyspepsia soon occurred, attended by great derangement of the nervous system, and ultimately the mania I have mentioned. But I have also seen the same condition very speedily induced.'"

HISTORY OF A LEGAL INVESTIGATION RELATIVE TO A SUSPICION OF POISONING WITH A PREPARATION OF LEAD.*

BY M. ORFILA.

COMPLICATED criminal cases like the above, published by Orfila in the *Annales d'Hygiène*, 1844, No. 61, pp. 131—294, are happily of but rare occurrence, especially in Germany, where the demoralisation is far from having reached to that height at which it stands in France.

The history of this case, which is most circumstantially described by Orfila in 163 octavo pages, is particularly remarkable, on account of the chemical investigations, which were instituted *ex officio*, and the depositions made at the assizes by the learned witnesses; it is, therefore, of a nature to induce men of science to draw important inferences from it, and deserves all our attention.†

Notwithstanding, it is very difficult to sketch out the main feature from public documents so complicated and circumstantial as those in question, we will attempt the laborious task in the following:

I. THE INDICTMENT.

J. J. Pouchon, a countryman at Vorey, widower and father of several children, married as his second wife a certain Mary Camus, who entertained an illicit intercourse with a man named Rocher, a dyer, who lived apart from his wife and children. Pouchon, suffering by a chronical ulceration of the stomach, entered the hospital of Puy, as he was at home neglected by his wife and deprived of all attendance. By a careful treatment he was cured in the hospital, and returned home in perfect good health. A few weeks after, however, the following symptoms molested him again,—burning colic pains, vomiting, spasms, convulsions in the extremities; all these reached to such a height on the 12th of July, 1842, that he betook himself to his bed, and never left it again. According to a legal statement since made by a witness, he said the same day to his wife—"You have given me some chick wine drink, which makes me very ill; you told me that I should shake the bottle, because it contains something which would cure me; but I never drank anything so bad as this; but I have been cautioned." He complained of burning pains in his stomach, and rejected all that he swallowed. He remained in this state until the 14th of July, when he died. The death was preceded by such violent pains, that the cries of the patient were heard in the neighborhood, and that in the corpse signs of great sufferings could be perceived. The neighbors who came to offer assistance observed, that his wife was the only person who showed no sympathy.

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* Bucher's *Repertorium*, vol. xxxv., p. 3, 1844.

† Several pamphlets were published relative to this case; the following are the titles of those given by Buchner—

Affaire d'Empoisonnement par un Composé de Plomb, &c.; par M. Orfila. Paris: J. B. Baillière, 1844.

Consultation Medico-légale, relative à une Accusation d'Empoisonnement par le Plomb, par Alph. Dupasquier. Paris: J. B. Baillière.

Procès Pouchon, Accusation d'Empoisonnement par le Plomb, Compte-Rendu des Débats des Cours d'Assizes de la Haute Loire et du Puy-de-Dôme. Paris: Louis Colas.

Consultations de M. Rognetta de MM. Dingey et Flandin, et Compte-Rendu de l'Affaire Pouchon. In the *Annales de Thérapeutique*, Jan. 18, 1844, and *Experience*, March, 1844.

This want of feeling excited among the public the suspicion of a murder by poisoning; the intimacy between Mary Camus and Rocher was no secret, it having transpired at the same time that the woman wished to get rid of her husband. The attention of the authorities having been directed to this point, they took the necessary measures; the houses of both the suspected persons (Rocher and Mary Camus) were searched, when part of the matters vomited by Pouchon was found. Then the body was exhumed and subjected to a legal *post mortem* inspection. The Drs. Reynaud and Porral, who were charged with the investigation, discovered no signs of the presence, or the effect of an irritating poison; they declared that if poisoning had taken place, the poison must have acted by absorption; and as in the stomach no lesion, which might have preceded death, could be found, the two physicians formally pronounced that it was impossible to attribute death to the disease under which Pouchon previously labored. The chemical investigation of the organic matters taken from the body, the remainder of the vomitted substances found in the dwelling of Pouchon, and, finally, some other matters which were seized on searching the house of Rocher, being substances employed in dyeing, was undertaken by M. Barsé, apothecary and chemist at Rion, in Puy, in the presence of the above medical men. The result of the chemical analysis was, *that a considerable quantity of lead was found in the organs of the body and in the vomitted matters.*

This result and some equivocal words uttered by Mary Camus and Rocher in the presence of persons, who afterwards appeared as witnesses against them, gave a high degree of probability to the suspicion that they had committed the murder by poisoning. To this must be added the circumstance, that on the 10th of July Rocher took with him into a public-house a man named Chanal, a soldier on furlough, offering him 200 francs if he would poison his (Rocher's) wife, her two children, and Pouchon. Chanal with indignation refused to enter into that scheme, and informed his father and mother of it, who recommended him to keep it as a secret; it was, however, mentioned before several persons. To this were added other statements of witnesses, upon which the highest degree of probability of poisoning having been committed was to be based. Mary Camus and Rocher, however, in their examinations, obstinately maintained their innocence, but which could not in the least lessen the suspicion raised against them.

II. ABSTRACT OF THE REPORT OF THE DRS. REYNAUD AND PORRAL ON THE EXHUMATION AND POST MORTEM EXAMINATION OF J. J. POUCHON.

These physicians state in their report, that, having been summoned on the 22nd July, 1842, by the Procurateur Royale, they on the 23rd July, in the presence of the legally commissioned persons, ordered the exhumation of the body of Pouchon, who died 14th July; that they, previous to this, examined Mary Camus and other persons with reference to the symptoms which preceded death, when the following were ascertained. That Pouchon was sickly for four years, and during this time twice an inmate of the hospital; that he suffered from pains in his stomach, frequent vomiting, and that his alvine excretions were frequently mixed with blood; that in the last six weeks of life he seemed to be better, and did not vomit for twenty days; that, however, the three days previous to death, on having partaken of a salad, he was attacked by violent pains in his stomach, repeated vomiting of a blackish matter, alvine dejections mixed with blood, and convulsions, and when in the full possession of his senses death supervened.

The report gives a most exact account of the state of the earth covering the grave, with the remark, that it was separately collected: first, the earth from the hill over the grave; second, that immediately covering the coffin; and, finally, that under the coffin; and was chemically examined. The condition of the coffin, and the signs perceived on the exterior surface of the body, which was already in an advanced state of decomposition, are carefully described. All these, as well as the description of the state of the intestines, we pass over in silence, merely remarking, that the stomach was highly distended, and in one spot concreted with the liver; that in this concreted place on the inner side a sac was formed; a cicatrix, attributable to a previous ulceration being noticed at the same time: that, however, a later lesion could not be detected; that, moreover, the contents of the stomach consisted of a small quantity of a liquid, and a considerable amount of small and hard corpuscles, varying in form and consistence, with some black spotted kernels of cherries; that these little corpuscles, though in small quantity, were likewise found in the reddened œsophagus. At the close of the report, the following opinion is given:—1. That from the results obtained at the *post mortem* examination, it cannot be concluded that the death of Pouchon was caused by poisoning. 2. That the morbid symptoms indicating a previous disease might perhaps suffice to ac-

count for the death, if an anatomical lesion of a recent date could have been discovered. 3. That if Pouchon, indeed, came by his death in consequence of poisoning, the poison must have been affected by absorption, and not as a topical irritant.

III. ABSTRACT OF THE REPORT OF THE ABOVE TWO PHYSICIANS, ON THE CHEMICAL INVESTIGATION INSTITUTED IN THE LABORATORY OF PHARMACY OF THE HOSPITAL AT PUY.

The substances, chemically investigated, at the request of the judge, were no less than twenty. Most of them we pass over as immaterial, viz., several matters employed in dyeing, as verdigris, sulphate of copper, sulphate of iron, cream of tartar, powder of bricks, oil, hogwood, which were seized at Rocher's, and begin with the consideration of the investigation—(1) of the *stomach*: (2) of the *œsophagus*. (3) of the *contents of the stomach of Pouchon*.

On the interior membrane of the stomach was found a great number of small, hard, irregular crystalline corpuscles of various forms and dimensions, resembling blackish spotted grains of quartz. Most of these corpuscles were hard, insoluble in water, without odor and taste; some other corpuscles were blackish, of a lamellar structure, and very friable; others were of a yellowish color, and partially soluble in water; they were, therefore, extracted with cold water, and an addition of acetic acid; but this extract, on being treated with sulphuretted hydrogen, infusion of galls, and sulphuric acid, was not rendered turbid.

The above-mentioned organic residues were then divided into three portions, one of which was put into a glass vessel, sealed up and handed over to the judge who was present; one of the two other portions was cut into shreds with a carefully cleaned knife, put into a glass retort, with distilled water, and boiled for six hours.

This extract was examined by the following re-agents:—ammonia, sulphuretted hydrogen, and caustic potassa gave no precipitate; infusion of galls produced a whitish caseine coagulum. One portion of the extract was tested for arsenic, by Marsh's method, with the improved apparatus of Flandin and Danger, but the result was negative. Portions of the liver, the lungs, the intestines, the bladder, and the remainder of the stomach, after being boiled, were heated in a clean porcelain capsule with the sixth part of their weight of sulphuric acid, and calcined until an almost dry and friable coal was obtained, which, after an addition of a little nitric acid, was evaporated again, and then extracted with boiling distilled water, and treated in Marsh's apparatus. But no traces of arsenic or antimony were obtained. The operation was repeated with some modifications, but gave the same negative results.

Counterproofs were afterwards made, the said parts taken from the corpse were on purpose mixed with very small quantities of arsenic, and treated with Marsh's apparatus, in the above manner, when instantly stains of arsenic were obtained. By this it was proved that no arsenic was contained in the organs of Pouchon.

A portion of the organic substance carbonised with sulphuric acid, was extracted with boiling distilled water, and the filtered mass examined by several reagents; even in them no copper could be detected.

Portions of the stomach, the intestinal tube and the liver were cut into shreds, and boiled in a porcelain vessel with water previously acidulated by acetic acid, to ascertain whether any vegetable poison was present. The filtered mass, on being evaporated to the consistance of an extract and treated with nitric acid became yellow, and not red; treated with muriate of iron it was rendered brown and not blue. From this it was concluded that the body contained no vegetable poison.

These experiments were brought to a close on the 4th of August.

IV. ABSTRACT OF A REPORT OF A SECOND SERIES OF CHEMICAL EXPERIMENTS INSTITUTED WITH THE ASSISTANCE OF M. BARSE, APOTHECARY, AT RIOM.

The second series of experiments was commenced on the 5th of August, and lasted several days. No one had access to the laboratory except the persons officially commissioned with the investigation. The organs taken from the body were first investigated and tested in three different respects, viz.:—1. For vegetable poisons. 2. For metallic substances to be discovered by Marsh's apparatus. 3. For other species of fixed poisons.

To decide upon the first question, viz., whether any vegetable poison was present, portions of the œsophagus, stomach, and heart were cut into shreds, boiled in a porcelain capsule with distilled water, acidulated by acetic acid, and the decoction filtered. It contained no fat, and was but little colored. The greatest part of the animal substances was then thrown down by the basic acetate of lead; the acetic liquid, filtered off from the precipitate, was then first freed from its whole amount of lead by sulphuretted hydrogen gas, then filtered again, and evaporated to dryness in a water-bath. This extract had not in the slightest degree any

bitter taste; its behavior relative to nitric acid and muriate of iron was found to be the same as stated in the first series of experiments, and all this was considered as a confirmation of the absence of vegetable poisons.

To answer the second question, relative to arsenic and antimony, it was intended to extract portions of the liver, spleen, lungs, bladder, and of the small intestines by boiling them with caustic potassa in a cast-iron pan. But to prevent any mistake arising from the iron pan, it was, previous to the operation, washed with cold nitric acid, afterwards with caustic potassa ley, and, finally, with distilled water. These three liquids were evaporated, then heated with sulphuric acid, and, after cooling, examined by Marsh's test. Neither arsenic nor antimony were to be discovered. Another portion of the liquid, acidulated by sulphuric acid, was saturated by potassa; when, by a further examination, no iron was found. It was proceeded with to the investigation of the said portions taken from the body, which were cut and boiled with diluted caustic potassa in the iron pan. The dark-brown alkaline extract was strained through new linen previously washed with potassa, and allowed to cool. The fat was removed, and then the liquid saturated with nitric acid, and evaporated to the consistence of an extract. In this condition the mass was mixed with double its weight of nitrate of potassa, dried at a moderate heat, and then in small portions thrown into a new hessian crucible, and thus detonated. The produce of this detonation was a saline mass, deliquescing in the air, and of a blue color.*

This produce of the detonation was heated with pure diluted sulphuric acid, by which fumes of nitrous acid were disengaged with effervescence, a whitish precipitate of a gelatinous consistence being thrown down at the same time. The liquid, acidulated by sulphuric acid, was heated until the whole of the nitrous and sulphurous acids was expelled. Hydrogen was then disengaged by Marsh's apparatus; examined and found to be free from any metallic admixture; finally, the above sulphuric saline mass was investigated; but no trace of arsenic could by any means whatever be discovered.

A portion of the saline matter was carbonised with nitric acid, the coal extracted by distilled water which was acidulated with hydrochloric acid. On directing a current of sulphuretted hydrogen into the latter liquid, a dirty-gray precipitate was thrown down, which was collected on a filter, and afterwards poured into boiling nitric acid. To this liquid was then added sulphuric acid, by which a precipitate fell. But in that, likewise, Marsh's apparatus showed neither arsenic nor antimony.

To the same series of experiments was also subjected the insoluble residue obtained by boiling the liver and other organs in the alkaline liquid; it was only observed that the produce of the incineration was still more intensely blue, and that with sulphuric acid a copious precipitate was obtained.

To answer the third question, whether any fixed poison was present, a portion of the produce of the above detonation was first dissolved in distilled water. The solution was of a blue color; it was divided into several portions, and examined by the following reagents:—

A bright piece of iron and phosphorus could not betray any copper in the acidulated liquid.

Sulphuric acid produced a very large white precipitate.

Sulphuretted hydrogen gave a black precipitate.

Iodide of potassium produced no change.

Bicarbonate of potassa threw down a white precipitate.

As it might be concluded from these phenomena that lead was present, the following experiments were made:—

Of the residues which were insoluble in water and left by the above viscera, one portion was by itself carbonised in a cast-iron pan; the coal was rubbed into powder, mixed with pure caustic potassa, and fused in a hessian crucible. After one hour's fusion the coaly mass was taken from the crucible, extracted with distilled water, and allowed to repose. The coal deposited from the liquid, on being viewed with the microscope, showed no metallic globules; when it was again fused with potassa in a covered crucible, after one hour the fused mass was thrown on a stone. Considerable numbers of metallic globules could now be discovered with the naked eye. These globules were soft and ductile, could be flattened by pressure, and were convertible into a yellow oxide by the blow-pipe flame. To this decisive result it could perhaps be objected that the organic substances from which the lead was extracted were previously subjected to a long series of experiments; that, to find a vegetable alkali, acetate of lead was employed; and that the residue of this investigation might accidentally have been mixed with that portion which was finally examined, with the view of detecting any fixed metals

* It appeared from later experiments, that this blue color was to be derived from an amount of iron contained in the mass, hence from the Prussian blue formed.

present. To remove these doubts and objections the following series of experiments were undertaken :—

I. The cast-iron pan employed in the process of carbonisation was scoured with sand and hot nitric acid, and this acid was tested by sulphuric acid; no trace of lead could be discovered in it.

II. Into this iron pan, which was now known to be perfectly free from any amount of lead, were put the thick intestines, and a portion of the small intestines, which were now taken from the body of Bowden, and had not yet been employed in the previous experiments, and carbonised. The coal was in the above manner fused with caustic potassa in a new hessian crucible; after one hour the fused mass was thrown on a stone and extracted by pure water. In the insoluble ashy residue, the metallic lead was with perfect certainty to be ascertained by the naked eye, as well as by the known physical and chemical means, which we need not mention here.

III. Against this positive proof a doubt might be raised whether, as was maintained, in the normal state the human body does not contain any lead. To remove this doubt, a body interred eight days since was exhumed, and its viscera, viz., liver, spleen, kidneys, bladder, stomach and internal tube, and a portion of the lungs, were first washed and then carbonised. Portions of the small intestines and of the thick one were separately carbonised. Both these portions were, in the above manner, carefully fused with potassa, the fused mass poured off and extracted with water, to examine carefully and repeatedly the ashy residue by physical as well as by chemical means. But no trace of lead could be discovered.

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(To be concluded in our next.)

IV. REVIEWS AND NOTICES OF BOOKS, &c.

THE LONDON MEDICAL DIRECTORY. London: John Churchill, Princes-street, Soho.

A most admirable idea, carried out in a liberal spirit. It cannot fail to prove of infinite service to the medical profession generally, and to a numerous class of the public, who may here find the name, address, qualification, and principal literary and scientific productions of nearly every medical practitioner in London: we say *nearly*, for as there is an "addenda" of "names omitted" attached to the volume before us, we may fairly presume there may be some few omissions yet, which will doubtless be corrected in future editions. We cannot doubt that this work will become an "annual," and a most valuable one.

THE MECHANICS' MAGAZINE, (No. 1128) for March 22,

CONTAINS, among a number of valuable scientific articles, a paper "On Warming and Ventilating Buildings, Fermentation, and Putrefaction, &c.," by F. COXWORTHY, Esq., of the Ordnance Department. The following is an extract :—

"When I commenced this correspondence (No. 1,098, Aug. 24, 1844), it was not with the view of at present agitating the question of spontaneous ventilation, but to introduce, as I conceived, a cheap and efficacious means of warming and ventilating apartments, particularly offices, at that time of the year, when, *from the temperature of the external atmosphere, cold air cannot be admitted into the room*, and that I am not altogether singular in preferring the downward to the upward current, I may remark, that on reference to the last number of the *Royal Engineer Papers*, it will be seen that Major Jebb, the Inspector General of Military Prisons, introduces the pure warm air at the top of the cell, and draws off the foul air at the bottom, and the same method is followed by Messrs. Haden, of Trowbridge, a plan they have doubtless adopted for the obvious reason, that man's respiratory organs are turned downwards, and were not intended by nature as receptacles for dust. But by respiration, atmospheric air, a compound of one of oxygen to four of nitrogen, is taken into the lungs, and oxygen, nitrogen, and carbonic acid gas are expelled, and as carbonic acid gas occupies the same space as the oxygen consumed, it is quite clear, that for every volume of carbonic acid gas generated, four volumes of nitrogen must also be liberated into the room, supposing none of it to be consumed; and as this gas is as fatal to respiration as carbonic acid gas, unless it be also got rid of, the ventilation must be very imperfect.

"It is also a question of vital importance, and which has yet to be decided, whether

any and what portion of the nitrogen of the air is consumed during the process of respiration. Liebig asserts, and his assertion has gone the round of the philosophical press, that a portion of the oxygen of the air taken into the lungs during respiration combines with the hydrogen of the food and forms the vapor given off by perspiration. Whatever might be my own opinion on this question (for after all this *can* be but matter of opinion), I should be doubtful of its correctness when opposed to that of such a man as Liebig; and I should hesitate in now agitating the question, were it not that this great philosopher had long since proved that such a process as this is at variance with the provisions and wisdom of Nature. He has shown that ammonia is essential to vegetation; and nature, we know, never creates a necessity without providing for it. And to what source are we to trace the production of ammonia but to the hydrogen of organic matter? That hydrogen has a greater affinity for oxygen than any other substance under the influence of combustion or the electric spark, there cannot be a question; but unless I am most egregiously deceived, hydrogen never does combine with oxygen *to form vapor* under the influence of negative electricity. Some years since, I made many experiments on the decomposition of organic matter, particularly animal, in order to determine what action air had on it during the process of putrefaction. For this purpose, I made use of a copper still, to the feeder of which I connected a pipe that went to the bottom of the still, and to the top of the still I connected a stop-cock. Having partly filled the still with human secretions (mixed with water), which I had previously washed with dilute hydrochloric acid to deprive them of all the ammonia, I luted on the top, and left the still quiet for two or three days for the process to go on. I found as a general result that putrefaction was more rapid when the vessel was in connection with the earth than if insulated; and that the process was still more rapid if the still were immersed in the earth. The gases which I drew off at first consisted principally of sulphuretted hydrogen and carburetted hydrogen and the components of the air. And after the process had gone on for some time, ammonia and carbonic acid were amongst the gaseous compounds in abundant proportions, and with these latter bodies the water in the still was highly charged as well as with sulphuretted hydrogen. About the same time that I was engaged in these experiments a cesspit was emptied in the yard adjoining to my own, and the weather being then very hot and what is termed sultry, the stench that arose from it became unbearable. With a view to get rid of the nuisance, as well as to form some idea of the amount of the hydrogen compounds that were being generated, I mixed together the proper proportions of manganese and salt in an old jug and placed on the top of the mixture a cup full of dilute sulphuric acid. And when I had lowered the jug to the bottom of the pit, I upset the acid by pulling a string previously attached to the handle of the cup. I had no sooner done this than the whole yard became filled to the second floor windows with a dense white smoke, which continued issuing from the pit for at least fifteen or twenty minutes in large volumes, and was still perceptible the next day. Now if the hydrogen of matter under the influence of putrefaction, or a negative state of electricity, possessed the same affinity for oxygen that it has during combustion or the formation of chlorine by the decomposition of hydrochloric acid, it is quite clear that none of these compounds could be formed, as the whole of the hydrogen would be converted into vapor; and it is equally evident that as the source from which the animal kingdom derives its food contains no ammonia and but little nitrogen, that the greater part of the nitrogen that enters into the composition of the ammonia, must be obtained from the atmosphere, either during the process of respiration or putrefaction, but more probably both. And I suspect that if Liebig had stated that a portion of the nitrogen taken into the lungs during respiration enters into combination with the hydrogen of our food, and forms the ammonia that escapes from our system with respiration, he would have been much nearer the truth than in making the assertion he has.

"During the decomposition of animal matter there is liberated carbonic acid gas, ammonia, carburetted, phosphuretted, and sulphuretted hydrogen. Carbonic acid is given off from the lungs of animals by respiration; ammonia escapes through the pores of the skin with perspiration, and is contained in great quantities in animal secretions, as well as sulphuretted, carburetted, and phosphuretted hydrogen, the decomposition in both cases taking place under the influence of negative electricity."

NOTA BENE.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of the Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th; Advertisements on or before the 27th.*

THE CHEMIST.

I. CHEMISTRY.

AN APPLICATION OF THE ELECTRO-CHEMICAL THEORY TO SOME CASES WHICH HAVE BEEN CONSIDERED EXCEPTIONS.

BY MR. REUBEN PHILLIPS.

When a piece of zinc is immersed in sulphuric acid, its easy solution does not take place, unless some conductor, as a piece of copper, is also immersed in the fluid, and connected with the zinc. Now, a particle of zinc, about to combine with the radical, assumes, or has a tendency to assume, the electro-positive state. The hydrogen is also electro-positive, and the copper conductor opens a way whereby the positive electricity of the hydrogen can be transferred to the zinc. It is observable, that the negative electricity of the radical remains without alteration. Now, from the foregoing, it clearly appears that the electricity of the hydrogen cannot be directly imparted to the zinc, but it must travel by some longer way, through adjacent bodies; for, if such is not the case, where is the necessity for interposing a conductor in order to effect the transfer?

The explanation is perhaps thus:—If it may be conceived that the electro-negative end of the compound be turned towards the zinc, then it at once appears that, for the hydrogen's electricity not to pass through adjacent bodies, it must pass through the radical itself, which must, during its transit, have its electro-negative powers either impaired, or annihilated (according as the transmission is either instantaneous or continuous), and that, too, in the presence of *two* atoms, *either* of which suffices to give it its maximum charge.

To return: if, when the hydrogen is about to quit the radical, and, consequently, about to combine with another atom of radical, as in the ordinary way, whereby the charge is transmitted to the copper, if there be presented to the hydrogen an atom, A, previously in an unelectrified condition, but capable, by its proximity to another atom, B, and the other circumstances commonly modifying chemical affinity, of receiving the charge, and if B can give an electro-positive charge to the zinc, we should then have a combination of A and B, produced by the electro-motive power of the zinc and acid. If we suppose the atom A not to exist, but suppose that the hydrogen combines at once with B, and that B acts as in the former case, we have at once a consistent explanation of the cause of the effects of nascent hydrogen.

For A substitute platinum, and for B the oxygen of nitric acid, for zinc substitute silver, and for sulphuric acid some of the oxygen of nitric acid, we now have the explanation of the following experiment:—When an alloy of platinum and silver is acted on by nitric acid, the platinum is attacked and dissolved. Here the silver takes oxygen from the nitric acid; the argentine atom gives an electro-negative charge to another atom of oxygen, while the first atom of oxygen simultaneously gives an electro-positive charge to an atom of platinum; hence the electro-motive power of the silver and oxygen causes the combination of the platinum with oxygen.

The explanation is more complicated if it is supposed that the combination of oxide with acid contributes to the effect; but, with whatever atomic changes any one may conceive to occur, we think the above will suffice to point out the manner for viewing it according to this theory.

The same explanation holds good for the alloy of copper, nickel, and zinc, which is dissolved by sulphuric acid.

We now come to apply this view to the decomposition of a more stable compound, by its proximity to a body in the act of decomposition, taking, for an example, the peroxide of hydrogen and oxide of silver. Peroxide of hydrogen and oxide of silver, when mixed, instantly decompose.

To have the explanation of this, on the electro-chemical theory, we must bear in mind that peroxide of hydrogen is represented by water+oxygen, and that, before the electro-negative oxygen separates from the

water, a way must be opened for its electricity to escape, and the same of the water. During the decomposition of unmixed peroxide of hydrogen, the atoms assume such an arrangement as will enable the resinous electricity of the oxygen to combine with the vitreous of the water. Let, now, oxide of silver be mixed with the peroxide of hydrogen, the resinous electricity of the oxygen of the oxygenated water will pass to the silver, and the vitreous electricity of the water of the peroxide of hydrogen will pass to the oxygen of the oxide of silver.

We cannot help here remarking, that a very strong proof of Berzelius's theory of heat is afforded by the electro-chemical theory; which evidence, as far as we are aware, has been entirely overlooked; it is, that *decomposition*, as well as combination, produce much heat: by the electro-chemical theory both decomposition and composition equally disturb the electric equilibrium, and hence they should both produce equal quantities of heat, for according to Berzelius, heat is produced by the opposite electricities neutralizing each other.

If, instead of peroxide of hydrogen and oxide of silver there is substituted for oxide of silver a solution of sugar, and for peroxide of hydrogen yeast, we approach to having fermentation explained on electro-chemical principles. When, in the solution of sugar, active yeast emits carbonic acid and other gases or vapours, the electricities which, previous to decomposition, bound the atoms of yeast together, must simultaneously with the decomposition either neutralize each other, or must cause the atoms of sugar to assume such a grouping as shall enable them to appropriate the electricities of the decomposing particles of yeast, and at the same time supply to the yeast such an amount of electricities as to reduce the liberated atoms to a neutral state.

Different ferments produce different groupings of the atoms of sugar, as a consequence of the different quantities of electricites, which different ferments, by their dissolution, can deliver to the sugar, which so groups as to employ it. It may so happen that the electricities, yielded by some decomposing bodies, cannot be appropriated by the saccharine solution; therefore such bodies are not ferments.

We must now remark on the sense in which an atom may be said to deliver different quantities of electricity. If we take any atom as a, and add to it another atom b, and call $a + b = a^1$, by repeating this, $a^1 + b = a^2$, $a^2 + b = a^3 \ldots$ $a_{n-1} + b = a_n$. Now, a_n may contain any number of atoms we are at liberty to suppose, and every atom b may, by uniting to its corresponding radical, liberate a constant quantity of electricity; and consequently were b taken by chemical affiinity from a_n, b from a_{n-1}, &c., equal quantities of electricity would have to be neutralised; but regarding a_n as an atom of one kind of yeast, from which two equivalents of electricity are to be appropriated; and again supposing A_n, as an atom of another sort of yeast, from which, either more or less electricity than that which two atoms yield can be appropriated, the atoms of sugar, which previously grouped themselves so as to employ the first quantity, may easily be conceived to group themselves differently to appropriate the second quantity.

The chemical action of contagions and miasms admits of an analogous explanation. The principal uncertainty in representing these actions arises from the atomic grouping of the higher organic products being unknown; for instance, sugar may be $126 + 12$ water, or $12(CO) + 12H$, and many others; hence it becomes impossible to say what atoms in the fermentation of sugar and analogous actions change their grouping.

We think it well worthy the attention of physiologists, whether the vital force (meaning by vital force that power which produces effects in opposition to the ordinary laws of chemical action) of animals, at all events, is not identical with that force which causes the solution of platinum in nitric acid. During life there is the formation of new substances, invariably accompanied by the destruction of other parts.

REMARKS MADE BY DR. HARE, AT A LATE MEETING OF THE AMERICAN PHILOSOPHICAL SOCIETY, ON A RECENT SPECULATION BY FARADAY ON ELECTRIC CONDUCTION AND THE NATURE OF MATTER.

To the Editors of "The Chemist."

Philadelphia, Nov. 30, 1844.

MESSRS. EDITORS.—At the last meeting of the American Philosophical Society, I made some verbal remarks on a recent "speculation" of the celebrated Faraday, published in the London and Edinburgh Philosophical Magazine for February last. Of course a brief notice will be given of these remarks in the bulletin of the proceedings. I send you for publication a statement of my reasoning

* Communicated by the Author.

on the questions at issue, hoping that it will not be found unworthy of the attention of philosophical chemists.

Your friend, ROBERT HARE.

Faraday objects to the Newtonian idea of an atom being associated with combining ratios. These he conceives to have been more advantageously designated as chemical equivalents.

This sagacious investigator adverts to the fact that after each atom in a mass of the metal potassium, as combined with an atom of oxygen and an atom of water, forming thus a hydrated oxide, the resulting aggregate occupies much less space than its metallic ingredient previously occupied; so that taking equal bulks of the hydrate and of potassium, there will be in the metal only four hundred and thirty metallic atoms, while in the hydrate there will be seven hundred such atoms. Yet in the latter, besides the seven hundred atoms, there will be an equal number of aqueous and oxygenous atoms, in all two thousand eight hundred ponderable atoms. It follows, that if the atoms of potassium are to be considered as minute impenetrable particles, kept at certain distances by an equilibrium of forces, there must be, in a mass of potassium, vastly more space than matter. Moreover, it is the space alone that can be continuous. The non-contiguous material atoms cannot form a continuous mass. Consequently the well-known power of potassium to conduct electricity must be a quality of the continuous empty space, which it comprises, not of the discontinuous particles of matter with which that space is regularly interspersed. It is in the next place urged, that while, agreeably to these considerations, space is shown to be a conductor, there are considerations equally tending to prove it to be a non-conductor; since in certain non-conducting bodies, such as resins, there must be nearly as much vacant space as in potassium. Hence the supposition that atoms are minute impenetrable particles, involves the necessity of considering empty space as a conductor in metals and as a non-conductor in resins, and of course in sulphur and other electrics. This is considered as a *reductio ad absurdum*. To avoid this contradiction, Faraday supposes that atoms are not minute impenetrable bodies, but, existing throughout the whole space in which their properties are observed, may penetrate each other. Consistently, although the atoms of potassium pervade the whole space which they apparently occupy, the entrance into that space of an equivalent number of atoms of oxygen and water, in consequence of some reciprocal reaction, causes a contraction in the boundaries by which the combination thus formed is inclosed. This is an original and interesting view of this subject, well worthy of the contemplation of chemical philosophers.

But upon these premises Faraday has ventured on some inferences which, upon various accounts, appear to me unwarrantable. I agree that *a* representing a particle of matter and *m* representing its properties, it is only with *m* that we have any acquaintance; the existence of *a* resting merely on an inference. Heretofore I have often appealed to this fact, in order to show that the evidence both of ponderable and imponderable matter is of the same kind precisely: the existence of properties which can only be accounted for by inferring the existence of an appropriate matter to which those properties appertain. Yet I cannot concur in the idea that because it is only with *m* that we are acquainted, the existence of *a* must not be inferred; so that bodies are to be considered as constituted of their materialized powers. I use the word materialized, because it is fully admitted by Faraday, that by dispensing with an impenetrable atom *a*, we do not get rid of the idea of matter, but have to imagine each atom as existing throughout the whole sphere of its force, instead of being condensed about the centre. This seems to follow from the following language.

"*The view now stated of the constitution of matter, would seem to involve necessarily the conclusion that matter fills all space, or at least the space to which gravitation extends, including the sun and its system, for gravitation is a property of matter, dependent on a certain force, and it is this force which constitutes matter.*"

Literally this paragraph seems to convey the impression, that agreeably to the new idea of matter, the sun and his planets are not distinct bodies, but consist of certain material powers reciprocally penetrating each other, and pervading a space larger than that comprised within the orbit of Uranus. We do not live upon, but within the matter of which the earth is constituted, or rather within a mixture of all the solar and planetary matters belonging to our solar system. I cannot conceive that the sagacious author seriously intended to sanction any notion involving these consequences. I shall assume, therefore, that, excepting the case of gravitation, his new idea of matter was intended to be restricted to those powers which display themselves within masses at insensible distances, and shall proceed to state the objections which seem to exist against the new idea as associated with those powers.

Evidently the arguments of Prof. Faraday

against the existence, in potassium and other masses of matter, of impenetrable atoms endowed with cohesion, chemical affinity, momentum, and gravitation, rest upon the inference that in metals there is nothing to perform the part of an electrical conductor besides continuous empty space. This illustrious philosopher has heretofore appeared to be disinclined to admit the existence of any matter devoid of ponderability. The main object of certain letters which I addressed to him, was to prove that the phenomena of induction could not, as he had represented, be an "*action*" of ponderable atoms, but, on the contrary, must be considered as an *affection* of them consequent to the intervention of an imponderable matter, without which the phenomena of electricity would be inexplicable. This disinclination to the admission of an imponderable electrical cause, has been the more remarkable, as his researches have not only proved the existence of prodigious electrical power in metals, but likewise, that it is evolved during chemico-electric reaction, in equivalent proportion to the quantity of ponderable matter decomposed or combined.

According to his researches, a grain of water by electrolytic reaction with four grains of zinc, evolves as much electricity as would charge fifteen millions of square feet of coated glass. But in addition to the proofs of the existence of electrical powers in metals thus furnished, it is demonstrated that this power must be inseparably associated with metals, by the well known fact, that in the magneto-electric machine, an apparatus which we owe to his genius and the mechanical ingenuity of Pixii and Saxton, a coil of wire being subjected to the inductive influence of a magnet, is capable of furnishing, within the circuit which it forms, all the phenomena of an electrical current, whether of ignition, shock, or electrolysis.

The existence in metals of an enormous calorific power must be evident from the heat evolved from mere hammering. It is well known, that by a skilful application of the hammer, a piece of iron may be ignited. To what other cause than their inherent calorific power can the ignition of metals by a discharge of statical electricity be ascribed?

It follows that the existence of an immense calorific and electrical power is undeniable. The materiality of these powers, or of their cause, is all that has been questionable. But, according to the speculations of Faraday, all the powers of matter are material; not only the calorific and electrical powers are thus to be considered, but likewise the powers of cohesion, chemical affinity, inertia and gravitation, while *of all these material powers only the latter can be ponderable!!!*

Thus a disinclination on the part of this distinguished investigator to admit the existence of one or two imponderable principles, has led him into speculations involving the existence of a much greater number. But if the calorific and electrical powers of matter be material, and if such enormous quantities exist in potassium, as well as in zinc and all other metals, so much of the reasoning in question as is founded on the vacuity of the space between the metallic atoms, is really groundless.

Although the space occupied by the hydrated oxide of potassium comprises two thousand eight hundred ponderable atoms, while that occupied by an equal mass of the metal comprises only four hundred and thirty, there may be in the latter proportionably as much more of the material powers of heat and electricity, as there is less of matter endowed with ponderability.

Thus, while assuming the existence of fewer imponderable causes than the celebrated author of the speculation has himself proposed, we explain the conducting power of metals, without being under the necessity of attributing to void space the property of electrical conduction. Moreover, I consider it quite consistent to suppose that the presence of the material power of electricity is indispensable to electrical conduction, and that diversities in this faculty are due to the proportion of that material power present, and the mode of its association with other matter. The immense superiority of metals, as conductors, will be explained by referring it to their being peculiarly replete with the material powers of heat and electricity.

Hence Faraday's suggestions respecting the materiality of what has heretofore been designated as the properties of bodies, furnish the means of refuting his arguments against the existence of ponderable impenetrable atoms as the basis of cohesion, chemical affinity, momentum and gravitation.

But I will in the next place prove, that his suggestions not only furnish an answer to his objections to the views in this respect heretofore entertained, but are likewise pregnant with consequences directly inconsistent with the view of the subject which he has recently presented.

I have said that of all the powers of matter which are, according to Faraday's speculations, to be deemed material, gravitation alone can be ponderable. Since gravitation, in common with every power heretofore attributed to impenetrable particles, must be a matter independently pervading the space throughout which it is perceived,

by what tie is it indissolubly attached to the rest? It cannot be pretended that either of the powers is the property of another. Each of them is an *m*, and cannot play the part of an *a*, not only because an *m* cannot be an *a*, but because no *a* can exist. Nor can it be advanced that they are the same power, since chemical affinity and cohesion act only at insensible distances, while gravitation acts at any and every distance, with forces inversely as their squares; and, moreover, the power of chemical affinity is not commensurate with that of gravitation. One part by weight of hydrogen has a greater affinity universally for any other element than two hundred parts of gold. By what means then are cohesion, chemical affinity, and gravitation inseparably associated, in all the ponderable elements of matter? Is it not fatal to the validity of the highly ingenious and interesting deductions of Faraday, that they are thus shown to be utterly incompetent to explain the inseparable association of cohesion, chemical affinity and inertia with gravitation; while the existence of a vacuity between Newtonian atoms, mainly relied upon as the basis of an argument against their existence, is shown to be inconsistent both with the ingenious speculation, which has called forth these remarks, and those Herculean "researches" which must perpetuate his fame.

ON THE DISENGAGEMENT OF GASES FROM PLANTS.*

BY DR. H. HOFFMANN.

From Liebig's important disclosures concerning the vegetation of various plants, the study of the respiration of these beings has obtained a new interest; by them I was induced to institute similar experiments, giving the preference to the less studied class of cellular plants, the result of which I take the liberty of communicating in the following:—

I first directed my attention to the quantity of carbonic acid exhaled, the motives for which will be stated below.

METHOD ADOPTED IN THE INVESTIGATION.

The fungi, &c., were collected when growing most luxuriantly, cut off as close as possible to the texture of the root, and freed from earth, &c., and not otherwise wounded, placed into a bowl, of three hessian, schoppen (2½ pints), covered with a glass plate, and luted over. The air, previous to entering, passed through tubes filled with chloride of calcium, and caustic potassa, and on issuing, traversed a tube which descended to the bottom, over a drying apparatus filled with chloride of calcium, then a double potassa apparatus, the first filled with potassa ley, the other with dry caustic potassa to compensate for the evaporated liquid. The difference in the potassa apparatus before and after the experiment, indicated the amount of carbonic acid. At the end an aspirator was attached with a crane. All these experiments were instituted in autumn.

I. *Agaricus detonsus*, Fries.—The fungi, weighed when damp = grammes 31·062 (5·8 per cent. solid residue = grammes, 1·8 dry fungi) were allowed to stand in the bowl over night, and a considerable quantity of carbonic acid was accumulated. The experiment (in the morning) lasted 195 minutes, during which time the volume of the air contained in the bowl was changed about six times. Temperature 59° F. The carbonic acid resulted = grammes 0·165. Hence, for every 100 grammes of the damp fungus in 100 minutes CO^2 = grammes 0·272; or, for (at about 212° F.) dried fungus, CO^2 = grammes 15·111.

II. *Hypnum repandum*, L. — Collected two evenings previously. The experiment (in the morning) lasted 195 minutes; the other circumstances as in No. I. Temperature 59°F.; weight of the fungus = grammes 154·3 (30·9 per cent. residue = 47·6) gave CO^2 = grammes 0·099. Hence, for 100 grammes damp fungus in 100 minutes, CO^2 = grammes 0·032; and for dry substance (as above) grammes 0·067.

III. *The same fungi* were allowed to remain over night in the same closed vessel. Experiment in the morning, weight as above. Light as in the former experiments, that of a dull autumnal day, CO^2 = grammes 0·889. Hence, 100 grammes of the damp fungi in 100 minutes, CO^2 = grammes 0·258. This striking increase of CO^2 is to be accounted for by the commencing decomposition of the plant, and the accumulation of the carbonic acid.

IV. *Agaricus puniceus*, Fries.—Grows free in the sun. This fungus is very watery, rapidly putrifies, disengaging a large quantity of ammonia; collected the preceding evening. Experiment in the morning, lasting 360 minutes; weight of the fungus = grammes 202·3 (9·8 per cent. residue = grammes 19·8), light as above; temperature 59°F., yielded CO^2 = grammes 0·117. Hence, 100 grammes of the damp fungus in 100 minutes = CO^2 grammes 0·015; the same dry = CO^2 grammes 0·078.

V. *The same fungus* was allowed to remain in the open vessel twice the time of the last

* Liebig's *Annalen der Chemie und Pharmacie*, February, 1845.

investigation. Experiment in the morning; duration 285 minutes. Temperature 48° F. Light and weight as above; CO^2 = grammes 0·080. Hence, 100 grammes damp fungus in 100 minutes = CO^2 grammes 0·013; the same dry = grammes 0·065.

VI. *Russula emetica.*—Collected 2½ days previous, of good appearance, though somewhat carious; had almost sprouted out. Experiment in the morning; duration 165 minutes, light clear, during one hour feeble sunshine. Temperature as above. Weight = grammes 112·4 (for 12·7 per cent. dry substance = grammes 14·274) CO^2 = grammes 0·060. Hence, 100 grammes damp in 100 minutes = CO^2 grammes 0·032; ditto dry = grammes 0·224.

VII. *Agaricus nitens*, BATSCH.—Grows between two furrows. Almost sprouted out. A very damp fungus, gathered 16 hours previously. Experiment in the morning; duration 135 minutes; light clear, without sunshine. Weight = grammes 138·7 (for 12·07 residue = grammes 16·6) temperature 54°F. CO^2 = grammes 0·078. Hence, 100 grammes damp in 100 minutes = CO^2 grammes 0·042; ditto dry = grammes 0·253.

VIII. *The same fungi*, remained during the day uncovered in the bowl. Experiment in the evening, when dark; duration 120 minutes. Quantity of the changed air, greatly approaching to that in the morning. Temperature 54°F. Weight as in No. 7, CO^2= grammes 0·030. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·016. By which it appears that, under the influence of light, at the same temperature, a plus of carbonic acid was obtained.

IX. *The same fungi* the following evening presenting the appearance of incipient decay, notwithstanding the low temperature in which these, like all other plants of the experiments were constantly kept. Experiment in the evening; duration, 90 minutes; no light; temperature and weight as above; CO^2 =grammes 0·075. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·059, yields a plus of CO^2 (to be attributed to the putrefaction, compare with below).

X. *Cantharellus cibarius.*—Collected about 14 hours previously. Experiment at noon; duration 135 minutes; weight = grammes 93·6 (to 14·8 per cent. residue = grammes 13·8), CO^2 = grammes 0·022. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·017; dry = grammes 0·123.

XI. *The same species* several days old, kept in a tin box apparently well preserved. Experiment at noon; duration 185 minutes; light clear, without sun; temperature 50° F.; weight = grammes 124·3, CO^2 = grammes 0·075. Hence 100 grammes damp in 100 minutes = CO^2 0·032.

XII. *The same fungi as in No.* XI.—Experiment the same day, in the evening, when dark; weight as above; CO^2 = grammes 0·019; duration 130 minutes. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·012.

On comparing the foregoing with the experiment instituted at day-light a minus of CO^2 results.

XIII. *The same fungi* the following day. Experiment in the morning, in feeble sunshine, (10th October); duration 120 minutes; temperature 46°F.; weight as above, CO^2 = grammes 0·030. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·020.

Results: a plus by the action of light.

XIV. *Boletus luteus*, L.—Collected on the borders of forests, about 24 hours previously, but very well preserved, most of them being young. Experiment at noon, weather rainy; duration 90 minutes; temperature 50°F.; weight = grammes 129·1, (for 6·6 per cent. residue = grammes 8·5). Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·032; dry = grammes 0·376.

XV. *The same fungi* the same day remained between this and that time uncovered. Experiment at dusk; duration 80 minutes; weight as above; CO^2 = grammes 0·053. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·051.

We may account for this difference, by the more frequent change of the volume of air which took place in the interior of the bowl.

XVI. *Lycoperdon coelatum*, BULL.—Grows between two furrows. The fungi were collected two days previously, presenting in the place cut through a pure white aspect, the cuticle being unbroken. Experiment in the morning, feeble daylight; duration 90 minutes; weight = grammes 73·1, (to 7·4 per cent. residue = grammes 5·4). Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·006; dry = grammes 0·111.

XVII. *The same fungi* having been uncovered in the mean time, the following day. In this, and in all the following experiments, the volume of air exchanged was invariably the same, wherefore they may serve for accurate comparisons.

In the preceding experiments, the circumstance just mentioned was less considered. Experiment in the day, light dull; the volume of air passed through during the experiment corresponds to 9½ schoppen (about 1 gallon). Hence 3½ times the volume of the empty bowl. Weight as above; duration 140 minutes; CO^2 = grammes 0·008. Hence 100

grammes damp in 100 minutes = CO^2 grammes 0·009.

XVIII. *The same fungi* the same day. The maturation of the fungi made considerable progress; the exterior envelope commenced to burst open. Experiment in the afternoon; duration 150 minutes; weight as above; CO^2 = grammes 0·014. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·013.

XIX. *Agaricus virgineus*, Pers.—In sunny furrows, fresh, collected about 8 hours previously; a little wounded at the stalk in gathering it. Experiment in the night; duration 135 minutes; weight = 99·6 grammes, (for 6·1 per cent. residue grammes 5·87) CO^2 = grammes 0·020. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·014; dry = grammes 0·238.

XX. *The same fungi* the following day. Experiment at a clear sunshine; duration 120 minutes; temperature in the shade 50°F.; weight as above; CO^2 = grammes 0·028. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·023; dry = grammes 0·391.

Results: a plus in sunshine.

XXI. *Agaricus spectabilis*, Fr.—Collected 12 hours previously, grows in a shaded light, in forests. Experiment in the morning, at a feeble sunshine; duration 105 minutes; weight = grammes 182·8 (for 8·5 per cent. residue = grammes 15·5) CO^2 = grammes 0 043. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·022; dry = grammes 0·142.

XXII. *The same fungi*, the same day, remained uncovered in the meantime. Experiment in the evening, in the dark; duration 125 minutes; weight as above; CO^2 = grammes 0·040. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·017.

Results: a plus in sunshine.

XXIII. *Agaricus muscarius*, L.—Collected about 16 hours previously, apparently very sound, grows wild in the forest. Experiment in the morning, daylight dull; duration 15 minutes; temperature 52°F.; weight = grammes 116·5 (for 5·0 per cent. residue = grammes), CO^2 = grammes 0·039. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·029; dry = grammes 0·498.

XXIV. *The same fungi* remained uncovered until the evening; experiment when dark the same day; duration 107 minutes. Weight as above. CO^2 = grammes 0·035. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·027.

XXV. *The same fungi.*—Experiment the following morning at a feeble sunshine; duration 120 minutes. Weight as above. CO^2 = grammes 0·049. Hence, 100 grammes damp in 100 minutes, = CO^2 grammes 0·035.

XXVI. *The same fungi.*—Experiment in the evening of the same day as 25; duration 120 minutes. Weight as above. CO^2 = grammes 0·030. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·020.

From the above experiments, it appears that the quantity of carbonic acid exhaled by fungi is considerable (a frog expires for each 100 grammes of its body in 100 minutes 0·02; a cat, 0·26; a pigeon, 0·48, carbonic acid); that this quantity is considerably augmented, in proportion as the fungus advances towards maturity, and chiefly under the influence of sunlight, darkness causing the reverse to take place; whereas, in plants endowed with vessels, the sun diminishes the quantity of carbonic acid exhaled.

EXPERIMENTS WITH GREEN PLANTS.

XXVII. *Hypnum triquetrum*, L.—Collected 16 hours previously. The superior green portion which served for the experiment was separated by means of scissors from the inferior portion, which partially had died away. Experiment in the morning, feeble sunshine; duration 100 minutes. Temperature 48°F. in the shade. Weight = grammes 20·0 (for 40·6 per cent residue = grammes 8·2). CO^2 grammes 0·042. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·210; dry = grammes 2·561.

XXVIII. *The same lichen* remained the whole day uncovered; experiment in the evening, when dark; duration 130 minutes. Temperature as above; weight the same. CO^2 = grammes 0·022. Hence 100 grammes damp in 100 minutes = CO^2, grammes 0·084; dry = grammes 0·024. Yields a plus for the time of sunshine.

XXIX. *Hypnum tamariscinum.*—Was kept for several days in a tin box, and remained well preserved. For the rest, as above. Experiment in the morning, clear sunshine; duration 115 minutes. Weight = grammes 12·0 CO^2 = grammes 0·012. Hence damp 100 grammes in 100 minutes CO^2 grammes 0·087.

XXX. *The same lichen* remained the whole day uncovered. Experiment in the evening of the same day, 139 minutes; Weight as above. CO^2 grammes 0·011. Hence 100 grammes damp in 100 minutes = CO^2 grammes 0·066 Yields a plus under the influence of the sun. This lichen was less aqueous than that of No. 27. The lichens seemed to behave like the fungi.

VASCULAR PLANTS.

XXXI. *Euphorbia, peplus*, L.—Entirely fresh, cut off at the *nodus vitalis*, but well

preserved, when placed in the bowl. Experiment in the morning, light very dull, rainy weather; duration 130 minutes. Weight =grammes 19·5. CO^2=grammes 0·014. Hence 100 grammes damp in 100 minutes =CO^2 0·059.

XXXII. *Urtica urens*, L.—Quite fresh when cut off, and well preserved, as above. Experiment in the morning, very little sunshine; duration 130 minutes. Temperature 50°F. Weight=grammes 22·2 (for 17·5 per cent. residue=grammes 3.885) CO^2 =grammes 0·015. Hence 100 grammes damp in 100 minutes=CO^2 grammes 0·048; dry grammes 1·236.

XXXIII. *The same plants* remained uncovered until the evening. Experiment in the evening, in the dark; duration 175 minutes. CO^2=grammes 0·042. Hence 100 grammes damp in 100 minutes=CO^2 grammes 0·108. Results: a minus in sunshine.

XXXIV. *Cichorium endivia*, L. (Salad).—One of the superior tufts of leaves was cut off when entirely fresh. The wound thus inflicted was soon closed by the effused latex itself. Experiment in clear sunshine, which chiefly fell on the inferior surface of the leaves, in the morning; duration 90 minutes. Weight=grammes 62·0 (for 9·2 per cent residue= grammes 5·704). CO^2= grammes 0·013. Hence, 100 grammes damp in 100 minutes=CO^2 grammes 0·023; dry, =grammes 0.403.

XXXV. *The same plants* remained uncovered for some time previous to the experiment. Experiment the same evening, in the dark; duration 125 minutes. Weight, as above. CO^2=grammes 0·080. Hence, 100 grammes damp in 100 minutes=CO^2 grammes 0·103. From this results a minus for the time of sunshine.

It would appear from these observations that the condition of the vegetation of the cellular plants—of the green ones no less than of all others, differs from those of the vascular plants.

On considering the place which parasitic plants hold in the vegetable kingdom, we find them to be either such as feed upon the unaltered sound juices of other plants (and animals); these are looked upon as injurious. To these we reckon *viscus loranthus, cytinus hypocistis*; or they live on, and by the remainder of a former vegetation in humus, on a mouldering bark; to these belong most fungi, mosses, lichens. They are considered as indices, not as causes of a deteriorated condition of the juices.

It appears, at one glance, that this mutual relation corresponds to that existing in animals, representing two general divisions of beasts of prey; the one feeding on carrion, and the others being fresh-flesh eaters. They stand in the same relationship with the other animals, the producing ones, as the parasitic plants to the others, which convert the simple constituents of air and soil into the more complicated forms, to afford food and life to plants organised otherwise; and this they may effect, either during life, or only after having died away. Here, again, we recognise the admirable means by which the infinite mass of forms of life, the production of which seems to be the main scope of nature, is kept up in a state of equilibrium.

I need scarcely add, that, with reference to the above, the carbonic acid is decisive, and this must be more the case until we are better informed as to what are the nearer nourishing substances of the mother plants, as well as of the parasitical ones. Plants which form their texture of simple matters, for instance, carbonic acid and water, exhale oxygen under the influence of the sun, carbon and hydrogen remain in the plant; whereas plants which require prepared nourishing matters, and in all other cases, perhaps, humus substances must, of course, contrast in this respect, they must exhale carbonic acid. They are even devoid of organs for secreting oxygen; they are generally without chlorophylle, and, if endowed with a green color, this is different in appearance.

On comparing the homogeneous cases in the above series of experiments, by selecting from each species of the examined fungi one experiment instituted during the day, it appears that the *amount of water* contained has no influence on the carbonic acid disengaged.

In these experiments no permanent difference was found between fungi growing in the sunshine, and others which lived in forests.

The objection, that the incipient decomposition is the principal cause of the disengagement of carbonic acid, is refuted partly by the aspect and condition of the fungi employed, and partly by the circumstance that the amount of carbonic acid does not become augmented or diminished at the time of the collecting of the fungi, and not even though the water contained in the fungi, would be taken into consideration as a momentum favoring the putrefaction. (*Vide* experiments 6 and 7, 14 and 16, 4 and 21, &c.)

Nor is the influence of temperature permanent, since the temperature remaining the same, at various degrees of light the same species yield various quantities of gas, as in experiments 6, 7, &c.

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ON THE CONVERSION OF CANE SUGAR INTO LACTIC ACID.*

BY H. BLUCHER.

1. With the view of repeating the experiments of Pelouze and Gelis relative to butyric acid, I added, to a solution of 1·800 grammes of cane sugar in 9,500 grammes of water, caseïn which had been previously repeatedly washed, and washed chalk, allowing it to stand in an open glass vessel, and stirring it once almost every day for four weeks, during which it was kept in a drying-room, the temperature of which was from 86° to 95°F. After the lapse of two weeks and a half, crystalline masses were first separated, accumulating after some days to such an extent as to be carried towards the surface by the gas bubbles formed, and, aggregating there, formed crusts of one inch in thickness, which could with difficulty be perforated by a glass rod. After a fortnight, with a visible disengagement of gases, the whole liquid was converted into a dense crystalline pulp; a glass rod was immersed and fixed in an upright position. The water evaporated during this period in small quantities, and was mostly resupplied. The whole mass was filtered through fine linen, and strongly pressed, then again dissolved in boiling water, recrystallised by cooling, and furnished with the salt obtained from the ley which was evaporated and employed in the crystallisation; 1,121 grammes of crystallysed lactate of lime, besides its water of crystallisation, contained almost 1½ per cent. of moisture. This large quantity of lactate of lime was perfectly white after the first crystallisation, and only after a longer and more powerful drying it became brownish; on being recrystallised, the salt remained white, and was found to be pure.

2. As under the circumstances mentioned (1) no butyrate of lime was formed, although Pelouze and Gelis observed its formation at from 77° to 86°F., I suspected that the temperature of the drying-room might have been too high, and undertook, therefore, several new experiments in a small drying-room, the temperature of which, at the height in which the sugar solution was placed during the day, was 86°F., and during the night 77°F.; the solutions did not very briskly follow this change of temperature, and were, therefore, mostly between 80° and 86°F, but exhibiting, apparently, completely similar phenomena, they underwent the same metamorphoses.

A. 600 grammes of cane sugar, 2,800 grammes of water, and 150 grammes of moist caseïn (answering to 42 dried in a water-bath), an addition being made of a sufficient quantity of washed chalk, furnished, after 4½ weeks, 469 grammes of very dry crystalline lactate of lime, containing no mechanically admixed moisture. The expressed liquid, on evaporation by steam, left behind a residue of 82·1 grammes, containing 16·91 grammes of lime.

B. 300 grammes of sugar, 1,400 grammes of water, &c., gave, under similar circumstances, 213 grammes of crystallised lactate of lime. The residue left, on evaporating by steam, weighed only 29 grammes, in which 6·01 grammes were lime.

The unavoidable losses by filtering, pressing, and crystallising larger masses may certainly amount to some per cents.; the result obtained is, therefore, somewhat more considerable than the above numbers indicate.

These experiments prove, at all events, that 92 per cent. of the constituents of sugar which did not escape as gases, were converted into lactic acid, and if the 6·01 grammes of lime contained in the concentrated residue are supposed to be combined with 17·2 grammes of lactic acid, we may assume, with probability, 99 per cent. The transformed caseïn was not taken into consideration in this calculation; its weight, however, amounted only to a few per cents. of the sugar employed: it is also improbable that part of its elements should have been converted into lactic acid.

C. 250 grammes starch sugar, which was formed by treating starch with very diluted sulphuric acid, furnished, under the same circumstances as in A and B, 213 grammes of crystallised lactate of lime, and the evaporated residue weighed 42 grammes. This conversion, however, was effected at least eight days sooner than in experiments A and B.

D. 600 grammes of potatoe starch treated in the same manner as the cane sugar (A) appeared not to furnish any lactate of lime; the whole of the liquid, on being evaporated, left only 4·8 grammes residue, which consisted partially of acetate of lime.

The experiments A, B, and C, having given the same results relative to the formation of lactic acid, as obtained in (1), to obtain salts with lactic acid, the following process was adopted:—

3. 1,400 grammes of cane sugar dissolved in 6·000 grammes of water, with an addition of 400 grammes of moist caseïn (answering to 94 grammes of dry matter), and a sufficient quantity of washed chalk were exposed to a temperature of from 77° to 86°F. The first fortnight, except a sediment being

* Poggendorff's *Annalen der Physik und Chemie*, xi. 1844.

formed, the liquid remained limpid, and retained a sweet taste; soon after, however, crystallised lactate of lime was separated, and under an apparently stronger disengagement of gases; after four weeks the whole mass was converted into a crystalline pulp, as was the case in experiments 1 and 2. After one recrystallisation, 870 grammes of crystallised lactate of lime were obtained, and on a further concentration, the mother-liquor yielded 162 grammes of crystallised mannite; the latter, however, was not completely pure, which could not have been otherwise anticipated, as it was crystallised from a solution of lactate of lime, and contained 3·6 per cent. of lime, most probably combined with lactic acid. From the liquid, after filtration off from the mannite expressed and evaporated by steam, an additional quantity of small crystals of mannite separated, which could not, however, be removed from the liquid, being then of a syrupy consistence, wherefore the whole was evaporated in a water-bath.

The residue obtained in this manner weighed 732 grammes, and contained 12¾ per cent. of lime.

350 grammes of this dry residue were dissolved in 1,200 grammes of water, and after having added caseïn and washed chalk, placed in the drying-room; after seven days, this was converted into a thick crystalline pulp, from which, by recrystallisation, &c., 210 grammes of crystallised lactate of lime were obtained. The mother-liquor on evaporation in a water-bath yielded a residue of 26 grammes, in which 5·53 grammes of lime were found. On adding the product of 210 grammes to the whole residue of 732 grammes, this would have furnished 439·1 of the salt; hence the above 1,400 grammes of sugar, 1,309 grammes of crystallised lactate of lime, and estimating the loss approximatively to 3 per cent., 1,348 grammes. Notwithstanding, therefore, the formation of mannite a greater amount of lactic acid was formed than in experiments 1 and 2.

In what manner the formation of mannite (3) is connected with the production of lactic acid I have not yet ascertained. That in the so-called lactic fermentation, cane sugar is not resolved into lactic acid and mannite, appears unquestionable from experiments 1 and 2; the cane sugar, however, may perhaps first be converted into mannite, and afterwards into lactic acid, which by the product of the decomposition of the residue of 350 grammes, is not improbable. In order to fully settle this question, I put a portion of the obtained mannite, with caseïn, &c., in the drying-room; but after the lapse of eight days no formation of lactate of lime was noticed: but it is necessary that this experiment should be continued longer; and on exposing a solution of cane sugar with caseïn, &c., to heat, and evaporating with precaution, after eight days, I could not observe any vestige of formation of mannite. In this respect it is manifestly necessary to examine the sugar solution the moment it undergoes the so-called lactic fermentation, at various periods, and I made arrangements to undertake this kind of experiment.

In conclusion, I may remark that in the above manner great masses of lactate of lime are easily obtained, and at a cheap rate; and the method given by Boutron and Frémy of preparing lactic acid, &c., which has hitherto been considered the best, is by far more complicated and expensive; and which, moreover, as appears from comparative experiments, even after repeated recrystallisation, yields a salt of lime, which does not possess the same degree of whiteness.

By decomposing equivalent quantities of chloride of zinc and subchloride of iron with lactate of lime, and washing the small crystals in a glass funnel, closed below by cotton wool, as in Gay-Lussac's method, I readily obtained large quantities of lactate of zinc, and of lactate of suboxide of iron. A solution of the latter salt reduces a solution of nitrate of silver; metallic silver is separated, and the suboxide is converted into the *ferroso-ferric oxide*.

Lactate of strontia crystallises in grains and groups of a concentric radiated structure, externally much resembling lactate of lime, crystallising, however, from the aqueous as well as from the alcoholic solution, only gradually after the lapse of several days. 1·194 grammes of the salt, dried for a long time at 90°F. and exposed to a white heat, yielded 0·521 carbonate of strontia; hence the composition of the salt, considering the atomic weight of lactic acid to be 1012·4, corresponds exactly to the formula L+SrO +4 Ag.

—nt—

ON AN ETHEREAL OIL CONTAINED IN THE NEEDLES OF PINUS SYLVESTRIS.*

BY ROBERT HAGEN.

In preparing the so-called forest wool, (waldwolle), from the needles of *Pinus sylvestris*, Mr. Weiss, of Ziegenhals, obtained, as a collateral product, an ethereal oil, in inconsiderable quantity; the needles were

* Poggendorff's *Annalen der Physik und Chemie*, xii, 1844.

for some time boiled with water, and the disengaged steam condensed, when on the surface of the water thus condensed, a layer of oil accumulated, which, according to the age of the needles employed, presented a more or less yellowish-green color. Mr. Weiss had the goodness to send me a certain quantity of this ethereal oil, as well as of the water which passed over with it, with the view of its being investigated. The latter possessed a feeble acid reaction, owing to the presence of a small quantity of formic acid.

The ethereal oil is a thin liquid, possessed of a yellowish-green color, an agreeable aromatic odor, resembling that of oil of lavender; on being inflamed, it burns with a flame producing much soot; its specific gravity being 0·8859 at 54° F. It is soluble in alcohol and ether; in the former liquid, the more so the less water it contains. Caoutchouc is dissolved in this oil with the same facility as in oil of turpentine. Distilled by itself, it boils above 212° F., and the boiling point becoming continually higher, at first a colorless, and afterwards a yellowish oil distils over. A small quantity of a brown resin is left behind in the retort, which, if the heat be continued, is converted into fetid gases, and a coal is left.

On subjecting the raw oil with water to distillation, a thin fluid colorless oil passes over with the aqueous vapors, whereas a small quantity of a green tenacious oil is left behind in the retort. The thin fluid oil refracts light, possesses an agreeable aromatic odor, its specific gravity being 0·868 at 54° F.; mixed with fused potassa it may be distilled without change of odor; being brought into contact with potassium, at a common temperature, no reaction is noticed; on applying heat, however, it becomes colored, and brown flakes are separated from it. A certain quantity of this oil, dried by chloride of calcium, and subjected to an elementary analysis, yielded:—

I. 0·3135 1·001 carbonic acid, and 0·3245 water.

II. 0·222 0·7075 carbonic acid, and 0·234 water.

Or, in hundredths:—

	I.	II.
Carbon....	88·287	88·126
Hydrogen..	11·502	11·711

Hence, this oil has the same per centage composition as oil of turpentine, being, however, materially different from it, with regard to its odor, and its behavior with regard to dry hydrochloric gas. If a stream of dry hydrochloric acid be driven through this oil, it is absorbed to a considerable extent, heat being disengaged, becoming at the same time of a black brown color. It is, however, very difficult to obtain from it any solid camphor. I repeatedly subjected several ounces of this oil to the action of hydrochloric acid gas, yet succeeded only once in obtaining a small quantity of solid camphor, which I regret to state was not sufficient for an elementary analysis. From the above it appears that the oil extracted from the needles of pinus sylvestris, like that obtained from pinus abies, as shown in the case of the latter, by Wöhler,* is *sui generis*, and isomeric with oil of turpentine.

—nt—

ON THE HYDRATE OF OIL OF TURPENTINE.†

BY C. RAMMELSBERG.

Blanchet and Sell‡ were the first who investigated this substance, and found in it:

Carbon	70·91
Hydrogen	12·05
Oxygen.....	17·04

From this they derived the following formula:

$C^{10} H^{20} O^2$, or $C^{10} H^{16} + 4H^2 O$.

Dumas and Peligot‖ afterwards mentioned a crystalline hydrate of oil of turpentine, the composition of which was:

Carbon	63·8
Hydrogen	11·4
Oxygen	24·8

and obtained the same result from crystals of oil of basilicon and cardamom. They concluded from this, that the combination was $C^{10} H^{22} O^3 = C^{10} H^{16} + 3H^2 O$.

Wiggers§ and Hertz¶ have likewise communicated observations on the formation of this substance, the composition of which, according to Mitscherlich** coincides with that of Dumas and Peligot. I received some time ago well formed crystals of this substance, which admitted of a nearer crystallographic determination; they were deposited from a mixture of oil of turpentine, hydrochloric acid, spiritus cochleariæ and spiritus serpilli.

As the above analyses show a difference of one atom of water, I instituted an elemen-

* *Annalen der Chemie und Pharmacie*, vol. xlvii., p. 237.

† Poggendorff's *Annalen der Physik und Chemie*, vol. xii. 1844.

‡ *Annalen der Pharmacie*. vol. vi. p. 267.

‖ *Annal. Chim. Phys.* vol. lvii. p. 334.

§ *Annalen der Pharmacie*, vol. xxxiii. p. 358.

¶ Poggendorff's *Annalen*, vol. xliv. p. 190.

** *Lehrbuch der Chemie*. 4th edit., vol. i. p. 426.

tary analysis, with the somewhat brownish crystals, by which the following result was obtained:

0·462 grammes of substance, on combustion with oxide of copper in a current of oxygen, furnished 1·085 of carbonic acid = 0·22591 of carbon and 0·445 water = 0·04899 hydrogen, hence in 100 parts:

Carbon	64·05
Hydrogen	10·61
Oxygen.....	25·34
	100·

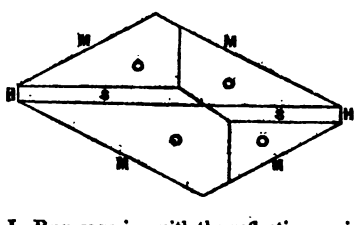
This composition, which coincides with those of Dumas and Mitscherlich, gives by calculation:

10 atoms carbon	750·00	= 63·17
22 ,, hydrogen ..	137·28	= 11·56
3 ,, oxygen	300·00	= 25·27
	1187·28	100·

The substance was not purified previous to operating by recrystallisation, not to lose the crystals, the determination of which was the main point; the result, therefore, could not be fully accurate.

The crystals represent rhombic prisms, the acute lateral edges being in some places obtuse; the terminal apex is four-sided, belonging to the rhombic octahedron, the more acute terminal edges of which are flattened in some places by two planes situate on the acute lateral edges of the prism. They, therefore, belong to the system of one—one axis (two and two members).

The diagram represents the crystal viewed from its terminal planes:—



I. By measuring with the reflective goniometer, I found the more acute inclination of the prismatic planes M M, which gave the best images, as a mean of five observations = 77° 37′. From this it follows, that the obtuse angle of the prism was M M = 102° 22′.

II. As a mean of seven measurements, the inclination of M O was ascertained; or of a prismatic plane to that of the rhombic octahedron = 126° 58′. If we assume, with Weiss, the shorter diagonal parallel of the axis A, the longer parallel B, and put axis c vertical, it follows from I. that the inclination of the octahedron planes to axis c = 53° 2′.

III. The inclination of the two octahedron planes in the more acute edges was found = 125° 18′. Yet the images were not sufficiently marked for an exact observation.

The plans H and s I observed only in some crystals which I could not measure.

By the known formula of the two and two membrous system, the following angular magnitudes are calculated from I. and II.:—

O O in the more acute terminal edges = 124° 6′ 47″.

O O in the more obtuse terminal edges = 135° 43′ 8″ 4.

O O in the lateral edges = 73° 56′.

O s = 152° 3′ 23″ 5.

The more acute terminal edges $\frac{2}{3}$, or their obtuse planes s in the lateral edges of the rhombic octahedron = 50° 30′ 21″ 32.

The same in the terminal edges = 129° 29′ 38″ 68.

The same, axis B = 25° 15′ 10″ 66.

The same, axis C = 64° 44′ 49″ 34.

The more obtuse terminal edges $\frac{2}{3}$ in the lateral edges of the rhombic octahedron = 50° 50′ 7″ 4.

The same, axis A = 30° 25′ 3″ 7.

The same in the terminal angles = 119° 9′ 52″ 6.

The same, axis C = 59° 34′ 56″ 3.

O O opposite the axis C = 106° 4′.

M H = 128° 48′ 30″.

H s = 115° 15′ 10″ 66.

On calculating the proportion of the axes, it appears that the axis A is the tangent of the half of the acute angle of the prism, that is, of 38° 48′ 30″, whereas axis C is the tangent of the angle of inclination of the more acute terminal edges, B, that is, of 25° 15′ 10″ 66; from this follows

B A = 1 : lg. tg. 38° 48′ 30″ = 9·9053965 = lg. 0·80426, and

B C = 1 : lg. tg. 25° 15′ 10″ 66 = 9·6736602 = lg. 0·47169; consequently,

A, B, C = 0·80426 : 1 : 0·47169.

—nt—

EXPERIMENTS ON THE ORIGIN OF SULPHUR IN PLANTS.*

BY DR. A. VOGEL, JUN.

It is known that a great many plants, especially of the family of cruciferæ, contain sulphur, not only as sulphates but likewise in other states of combination. In former time it was considered probable that substances which we cannot decompose, such as sulphur,

* Buchner's *Repertorium*, xxxvi. 3.

potassium, iron, &c., were produced in a mysterious manner, by the process of vegetation. But carefully instituted chemical analyses of the soil, manure, atmosphere, &c., have proved this hypothesis to be untenable, and have shown, that plants cannot contain other elements than those which they receive from the air, water, and earth.

Some years ago, however, conservator and professor Dr. Vogel, sen., satisfactorily proved by accurate experiments, that it is not always so easy, as might be believed, to find out the origin of the constituents of plants; he allowed garden cresses (Lepidium sativum) to germinate in a powder, composed of quartz and glass, moistened with distilled water, in pure air, and found then that these plants contained a greater quantity of sulphur, than was present in the seed, though the vessel in which the seed with glass powder and water employed were contained in the surrounding air, were free from any sulphate and sulphuretted hydrogen.

Hurant afterwards observed in the *Journal de Pharmacie et de Chimie* (1843) t. iii. p. 360, that the air is rarely totally free from a certain amount of sulphur, and that the greater amount of sulphur found by Vogel in the garden cresses may have originated from this source.

This induced Dr. A. Vogel, jun., assistant in the state chemical laboratory, at München, to repeat his father's experiments, with the utmost circumspection and accuracy; the result of these fresh experiments, communicated to the Royal Academy of Sciences, at Munich, at their sitting of the 9th November, to be published in the Transactions of the said Academy, does confirm the former experiments.

The seeds of the cress of which the amount of sulphur was ascertained by a chemical examination germinated in glass powder free from sulphates and was moistened with distilled water. To remove any sulphuretted hydrogen, which might possibly be contained in the air, the vessel was placed on magisterium bismuthi, and covered by a glass-bell, which was thus closed below by the bismuth preparation, the air in the glass-bell was daily renewed, the fresh air was each time, previous to its admission, shaken with a solution of acetate of lead, contained in a large bottle, to remove any amount of sulphuretted hydrogen or sulphurous acid.

After the cresses had become properly developed, they were dried, reduced to powder, and fused with a mixture of pure carbonate and nitrate of potassa. From the solution of this mass in water, the sulphuric acid produced was precipitated by chloride of barium.

The plants were produced in the above manner from 20 grains of seed, in which were 0·268 grammes of sulphur, showing an amount of 0·521 grammes of sulphur; hence 0·253 more, or almost double as much as the seed employed, though all the seeds put in the glass powder had not vegetated.

Vogel, however, found by other experiments, that cresses likewise receive sulphur from the common ground of gardens and fields, because the plants grown in garden-earth showed an amount of 1·35 per cent., whereas the cresses grown in glass powder contained only 0·68 per cent. of sulphur; he likewise observed, that the plant produced by keeping sulphur from it, is more feeble and requires for its development almost twice as much time as those which grow in the common garden-earth. From this he concludes, that sulphur is an essential, if not, perhaps, a completely indispensable condition for the growth of cresses.

To the question, whence came this portion of sulphur, which could not be demonstrated to have existed either as such as sulphuretted hydrogen, or as a combination of sulphuric or sulphurous acids, in the soil, the water, or the air, Vogel did not attempt to reply.

We may imagine only three cases:—(A) the experimenter in his investigations may have overlooked one of the known combinations of sulphur, or (B) sulphur may be produced by the process of vegetation; or, finally, (C) sulphur may likewise occur in a combination, in which it cannot be detected by the usual re-agents, either as sulphuretted hydrogen or as sulphuric or sulphurous acid. The two first suppositions are not probable, as the experimenter in operating was so careful in taking his precautions that they admit of no doubt; and the possibility that plants may produce sulphur from something which is neither sulphur itself, nor contains any, is not supported by the experience of all ages, by our views of vegetable life, and of bodies which cannot be decomposed. The third supposition, therefore, remains the most probable, viz., that sulphur is contained either in air, distilled water, or powdered glass, in combination, which escapes the attention of chemists, because this combination is neither sulphuretted hydrogen, sulphuric, nor sulphurous acid. This assertion is supported by the behavior of some combinations of sulphur already known, the sulphate of ether, the hyposulphites, which cannot be detected either by acetate of lead or chloride of barium. This investigation deserves, therefore, to be continued.

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RUTHENIUM, A NEW METAL.*

BY PROFESSOR CLAUS.

(Abridged from a Letter.)

After two years' uninterrupted labor, I have finally succeeded in obtaining, in a simple manner, the new metal concerning which I gave some information on a former occasion, in a state of purity, from the residues of platinum. I hasten to apprize you of this, communicating at the same time several statements relative to its peculiar properties. I hitherto have not been able to obtain the metal only as a black-gray powder, considerably lighter than iridium. It belongs to the interesting group of the platinum metals, and its chlorides and double chlorides are very similar to those of iridium. The resemblance between the double chlorides of iridium and potassium and the corresponding salt of the new metal is so great, that Berzelius, to whom I have sent a specimen of it, declared, in a letter directed to me, that it is an iridium salt. Eight days after, however, I received another letter from him, in which he recals his first views, stating it to be a salt of a metal unknown to him. The metal, however, has such well-defined and peculiar properties, that its being *sui generis* admits of no doubt. Its chloride, namely in the highest stage, has a fine yellow-red orange color, and on being treated with ammonia throws down a black oxide; whereas, the solutions of the chlorides of all other platinum metals, on being put together with ammonia, at a common temperature, do not precipitate at all. The solution of this chloride is acted upon by H^2S, (sulphuretted hydrogen); only, after long contact, when a metallic sulphuret falls, which is at first brown, and becomes black afterwards, the supernatant liquid presenting a beautiful azure blue color. This phenomenon is to be attributed to the circumstance, that, by the influence of H^2S, the chloride, on some metallic sulphuret separating, is converted into a blue; which, by H^2S, is scarcely at all decomposed.

If, after acidulating, by hydrochloric acid, the orange-colored chloride, a zinc rod be immersed, after some time a black metallic powder falls, and the liquid becomes of a dark indigo blue; the whole metal is afterwards precipitated, and the liquid rendered colorless.

The metal, as well as its combinations, on being calcined with a great quantity of nitre, gives a black-green mass, which dissolves in distilled water as a liquid of a fine orange color. This solution of the salt, composed of potassa and the metallic acid, renders organic substances black, and is decomposed by these bodies: alcohol, for instance, by the acids, &c., when a velvety-black alkaline oxide is thrown down. This latter, on being boiled with hydrochloric acid is completely resolved into the orange chloride. The chloride is most easily decomposed, especially in its aqueous solution; losing part of its chlorine, it becomes of a dark brown color, which is almost black, sometimes cherry-red, throwing down a black insoluble powder. In this decomposed condition it has an almost incredible coloring power: a few milligrammes of the chloride may render half a pound opaque (one pound contains about.500·000 milligrammes). If the chloride is precipitated by ammonia, the black precipitate dissolved in hydrochloric acid, and evaporated to dryness, a dirty green residue is left behind, which, on being dissolved in much water, yields an opaque, black liquid, verging towards cherry-red; and if evaporated and treated with nitric acid, exhibits the most beautiful variations of color: it is at first violet, then becomes azure blue, red, yellow, and finally, on evaporation to dryness, it is rendered green. The latter property this salt has, in common with the sesquichloride of iridium. All the other properties, however, its behavior with ammonia and sulphuretted hydrogen, are so characteristic that it must be seen at one glance that this metal is *sui generis*. The tendency of this metal to combine with alkalies is so great, that, on being fused with caustic potassa, it is completely dissolved in it, giving, when mixed with water, an orange-yellow liquid, which is likewise a salt consisting of potassa and a metallic acid. Hence, this metal cannot be reduced from its chlorides, as may be done in the case of other platinum metals, viz., by being mixed with soda and strongly calcined. If the metallic chloride is mixed with soda and calcined, the greatest portion of the mass obtained is dissolved in water with an orange-red color. I call this metal *ruthenium*, as it occurs in small quantities in the white body mentioned by Osann, which chiefly consists of silica, titanic acid, oxide of iron, and zircon; and was considered by Osann as a peculiar metallic oxide, which he called oxide of ruthenium.

The discovery of the new metal escaped the attention of Osann, because he repeatedly extracted the oxide of ruthenium by hydrochloric acid; but the extract he left without examining it, and investigated

* Read before the Imperial Academy of Sciences of St. Petersburg.

only the insoluble residue, which he regarded as containing the oxide.

On operating with the residue of platinum, I likewise obtained the ruthenium of Osann, endowed with all the properties described by that chemist. Having treated this oxide with hydrochloric acid, I extracted from it a considerable quantity of my oxide of ruthenium with some oxide of iron. After having become acquainted with the properties of this metal by experiments on the small scale, it was easy for me to prepare it from the residue of platinum. Previous, however, to having more fully ascertained the nature of the ruthenium, I decomposed my store of platinum residue by fusing it one time with nitre (to resolve it on the large scale with chlorine was too complicated a process and required too much time). From the fused mass, I extracted, by a particular method, the various combinations of the platinum metals contained therein, and from 15 pounds residue I obtained 4 ounces metallic osmium. On that occasion I found, before Frémy, the osmiate of potassa, though prepared in a different manner. It is a very beautiful salt, representing regularly crystallized octahedrons, of a deep garnet-red or rose-red color. The color depends on the greater or less rapidity with which it is formed. If they cool gradually, and have time to be deposited, from the not too supersaturated solution, into regular and large crystals, they are black verging into garnet-red and transparent at the edges; if they are rapidly formed from a saturated solution they are garnet-red, and on promoting the separation of the salt by stirring, the crystals represent a powder of a rose-red color. Being rubbed into a powder the salt is almost white; its formula is $KO + Os O^3 + Ag^2$. It is now slowly dissolved in water with the color of manganate of potassa. On evaporation the solution is partially decomposed, free osmic acid, $Os O^4$, is disengaged, and a black oxide of osmium, $Os O^2 + Ag$, precipitated, the liquid is strongly alkaline, and a portion of it crystallised, without being decomposed. The decomposition may be prevented, if much free potassa be added to the solution of the salt. Acids resolve instanter the solution of the salt into oxide of osmium and osmic acid, $Os O^3$ are converted into $Os O^2 + Os O^4$. This fact I noticed likewise before Frémy. This oxide on being heated in a glass tube, presents the remarkable property of being converted with a slight explosion into metallic osmium and osmic acid; from 2 ($Os O^2$) are produced $Os + OsO^4$. I prepared the whole series of the osmium combinations. Osmiate of baryta, $BaO + OsO^3 Ag$, is formed in fine black crystals of an adamantine lustre, if the solution of osmic acid is mixed with an excess of baryta water. The yellow liquid deposits the salt after four weeks. I prepared, morever, from these extracts of the first fusion with nitre a considerable quantity of iridium salts; among others I obtained three ounces of an iridium salt; though this offered very anomalous properties, I yet considered it as iridium, and added to it the chloride of iridium and potassium. I had, then, not yet sufficiently ascertained the properties of the salts of ruthenium. I know that it was chloride of ruthenium and potassium. It now remained to try to prepare ruthenium from the residue of the mass, which was first calcined with nitre, and then extracted with water and acids, that I kept in the mean time apart, with the view of examining it afterwards. I had the good fortune to obtain from this residue, though rather exhausted, nearly 1½ ounce of chloride of ruthenium and potassium in the following manner:— I mixed the residue with an equal portion of nitre, and exposed it for two hours in a hessian crucible to a white heat. The calcined mass was taken out by means of an iron spatula while still incandescent, and on cooling rubbed into a coarse powder. This was extracted with distilled water, by allowing it to stand until it became clear; the liquid becoming perfectly limpid was drawn off, when it presented a fine dark red color. This was continued until no more could be extracted. The liquid could not be filtered, because by the action of the filter it was decomposed, and the filter thus rendered impervious; it contained ruthenic acid, chromate and silicate of potassa; no vestige of rhodium or iridium, and but an inconsiderable trace of osmiate of potassa. Nitric acid was added to the solution with precaution until the liquid ceased to be of an alkaline reaction; potassa combined with oxide of ruthenium, and a little silica deposited as a velvety-black powder, and chromate of potassa remained dissolved. On being washed, the oxide of ruthenium with potassa was dissolved in hydrochloric acid, and evaporated until the silica was reduced to the gelatinous state, and then diluted with water and filtered. In order better to remove the silica we do not evaporate to dryness, because then the chloride of ruthenium would be resolved into an insoluble subchloride. The fine orange-yellow solution was concentrated to a small volume, and mixed with a concentrated solution of chloride of calcium, when the salt $KOCl^2 + Ru Cl^4$ separated as brown-red crystals. The liquid poured off from the crystals on evaporation, also yielded a large quantity of salt. The salt may be

purified by recrystallisation. I shall take the liberty of sending to you some specimens of my preparations, with the request that experiments may be made with them.

—nt—

ON THE MEAN TEMPERATURE OF IRKOUTSK.*

BY M. STCHOUKINE.

The author observed the temperature of the above place from 1820 to the 1st July (old style) 1844.

From 1820 to 1830 the observations were instituted three times daily, at 7 a.m, 2 p.m., and 9 p.m.; from 1830 to 1844, at 7 a.m., 2 p.m., and 10 p.m. It seems, that in the mean temperature of the winter a periodical movement takes place. Irkoutsk has an elevation of 1,330 English feet above the level of the sea.

It may be said, that on the whole the climate of Irkoutsk becomes milder.

Up to 1810 the cold was so extreme, that the mercury remained frozen for three days.†

Between 1810 and 1820 there were nights in which the mercury froze, but became liquid again during the day. Since 1820, this phenomenon did not occur every year. Since 1830, the greatest frost did not exceed 31°F. (28°R. = 35°C.) 95°F. (+ 28°R. = 35°C.) was the extreme limit of the summer's heat, but in 1843 we had frequently 97½°F., and one day 104°F. (+ 31° R.) And it was, moreover, observed, that the harvest was the more abundant the higher the mean temperature of the year became.

—nt—

INVESTIGATION OF THE ORGANIC ACIDS CONTAINED IN UNRIPE PRUNES (PRUNUS DOMESTICA) AND GOOSEBERRIES.‡

BY A. CHODNEW.

The examination of the organic acids occurring in plants, especially in fruits at the various stages of their maturation, is undoubtedly calculated to throw much light on the changes which plants, and chiefly fruits, undergo during the whole of their life.

A similar investigation, however, is connected with many difficulties; it being highly probable, that at the period of the maturation of fruits, one organic acid is converted into another, which is more complicated than the first, this conversion can only gradually take place, hence not only one, but two, three, and perhaps more organic acids may be simultaneously present in one and the same fruit. But in the present state of our analytical knowledge, we are unable, with precision, to distinguish these acids from one another; the existence of each acid can only be corroborated with accuracy by the elementary anlysis. The difficulty of the examination is likewise considerably augmented by the presence of inorganic acids.

For the above reason, I hope to be excused for having communicated these investigations relative to unripe prunes and gooseberries, instituted with the above view, which, owing to want of time, are not entirely complete.

The unripe prunes were freed from their kernels, bruised, rubbed with hot water, and the liquid filtered off. The liquid thus obtained was then boiled, filtered again, and precipitated with a little acetate of lead. The flaky precipitate obtained in that manner was over night completely converted into concentrically grouped needles, which rather distinctly indicated the presence of malic acid.

The crystals were collected on a filter and washed. The liquid, which first passed through, gave, with an excess of acetate of lead, still a copious yellowish precipitate, which, on being allowed to stand for a long time, assumed no crystalline properties, and was not examined more closely.

The above crystalline combination was treated with sulphuretted hydrogen and boiled, until the liquid no longer exhaled the odor of the latter gas; the liquid was then filtered off, and divided into two portions. One of these was saturated with carbonate of ammonia, and afterwards mixed with the other.

On the evaporation of the latter solution, I obtained the characteristic fine crystals of acid malate of ammonia. To prove the presence of malic acid beyond all doubt, I prepared the silver salt of it, determined its atomic weight and made two analyses of it.

0·661 grammes of the salt dried at 212°F. furnished 0·409 of silver, which answers to 61·87 per cent. of silver, or 66·45 per cent. of oxide of silver.

1·051 grammes, on combustion with oxide of copper, gave 0·524 of carbonic acid and 0·112 of water. 1·1655 grammes gave 0·5925 of carbonic acid and 0·1385 of water.

* Read before the Imperial Academy of St. Petersburg, 11th October, 1844.

† Mercury freezes at — 39°F.

‡ Liebig's *Annalen der Chemie und Pharmacie*, February, 1845.

These numbers correspond to the following composition:—

	I.	II.
Carbon.....	13·59	13·86
Hydrogen.....	1·18	1·31
Oxygen	18·78	18·38
Oxide of silver..	66·45	66·45

According to the formula—

$$C^8 H^4 O^8 + 2 Ag O,$$

which expresses the malate of silver, we must have:

Carbon	13·78
Hydrogen.....	1·14
Oxygen.....	18·39
Oxide of silver ..	66·69

The unripe gooseberries, treated in the same manner as the prunes, furnished a lead combination (an excess of acetate of lead being employed) which was again decomposed by sulphuretted hydrogen. The acid liquid was saturated with milk of lime and boiled, when a precipitate was thrown down, and a combination of lime remained dissolved in the liquid. The latter was filtered off, precipitated with acetate of lead, and the flaky-white precipitate, after being allowed to stand, was converted into the characteristic needles of malate of lead.

The difficultly soluble lime combination consisted mostly of basic citrate of lime, and a small quantity of neutral malate of lime, which, owing to its greater solubility, was separated from the former salt by washing with water.

The basic citrate of lime was then dissolved in a considerable quantity of water, and nitrate of silver added to it. In this manner I obtained a white precipitate, which I at first considered to be a pure silver salt; but the analyses and the determination of atoms showed that it was a double combination of lime and oxide of silver. I obtained, namely, from a determination of the oxide of silver and the lime, and two analyses of the salt prepared at various times, the following results:

	I.	II.
Carbon.....	15·50	15·49
Hydrogen	1·24	1·29
Oxygen	19·51	19·47
Oxide of silver..	50·60	50·60
Lime	13·15	13·15

To these numbers corresponds an unusually constituted double salt of citric acid:

$$C^{12} H^5 O^{11} + 2 Ag O + Ca O$$

which requires—

Carbon	15·85
Hydrogen.....	1·10
Oxygen.....	19·38
Oxide of silver ..	51·13
Lime	12·54

This peculiar combination, which was twice obtained, makes it necessary to repeat the examination of the acids occurring in unripe gooseberries.

—nt—

NEW CONTRIBUTIONS ON THE CHEMICO-DYNAMICAL ACTION OF PLATINUM,*

BY DR. J. W. DOEBEREINER

In the last autumnal vacation I found that the spongy platinum obtained by the pyrochemical decomposition of chloride of platinum and ammonia, at the common temperature, and by free access of air acts as energetically on formylic acid combined with 1, 2, or 3 equivalents of water, as it does, under similar circumstances, on hydrogen gas; that is, when slightly moistened with the said acid it almost instantly becomes incandescent, dynamically determining the said acid or its radical formule = C^2H to combine with the maximum of oxygen, and produce carbonic acid, and water ($C^2HO^3 + 2\,O = 2\,CO^2 + HO$.)

This experiment is as surprising as the action of platinum on explosive mixtures, and the more so as all other acids which contain a radical similar to formule, but perhaps constituted in a more condensed state, viz., C^4H^2—for instance:

Succinic acid = $C^4H^2 + 3O$

Malic acid, Citric acid } = $C^4H^2 + 4O$

Tartaric acid, Racemic acid } = $C^4H^2 + 5O$

are not affected by the mysterious power of platinum, under the circumstances in which I usually experiment with this metal, viz. not applying any artificial heat. Some weeks previously I found that spongy platinum, at the common temperature, does not react either on spirit of wine or on pyroxylic spirit; that it, however, is soon excited to an energetical activity, if at the same time, it is brought into contact with a solution of caustic potassa, or soda, in such a manner as to be moistened only by a mixture of the said alkalies with alcohol or wood spirit; it is often heated by this to ignition, converting at the same time the alcohol, partly in etherine or oxide of acetyle, and polyosmon, partly in oxide of retinule (resin of aldehyde), and acetylic acid, and the wood spirit first in formulic acid, and this latter acid soon after in carbonic acid, and water. This disengagement of heat, and process of con-

* Liebig's *Annalen der Chemie et Pharmacie*, February, 1845.

version, continues until the alkali is saturated with the acid formed.

Hence by the presence of the alkalies the platinum sponge acquires the property of the oxyphorus platinum (which is known by the name of platinmohr, (platinum black)), that is the property of absorbing oxygen, and condensing it in such a manner as to become fit to enter into chemical combination with several easily oxidizable organic substances. But this action of the platinum sponge, provoked by the alkali, never becomes equal to that of the properly prepared oxyphorus platinum, which on being alkalised heightens its power of absorbing oxygen to such an extent as to be enabled to convert in a short time any species of sugar, capable of fermentation, and several other organic substances CHO, into carbonic acid and water.

I have not yet succeeded in animating the two allotropical modifications of platinum so far as to convert ammonia into water, and nitric acid, or the chlorides of the alkaline metals into chlorates. They are likewise devoid of the power, at least at the common temperature, of raising the oxidability of hydrogen gas to an extent, enabling it partially to deoxidize carbonic acid, and to convert it into hydrate of oxalic acid, or oxide of carbon and water. —nt—

ON THE BASIC PHOSPHATE OF LIME.*

BY J. BERZELIUS.

SEVERAL chemists having raised a doubt concerning the existence of the basic phosphate of lime, which is composed of 3 atoms of phosphoric acid and 8 atoms of lime, or (6 Ca O, 2 P O^5) + (2 Ca O, P O^5), Berzelius was induced to repeat the analysis of this salt, which he first made 26 years ago.

A solution of phosphate of ammonia was mixed with ammonia free from carbonic acid, and into this solution was thrown, drop by drop, a solution of chloride of calcium, free from chloride of magnesium, until about one half of the phosphoric acid was precipitated. The liquid was then filtered off, and to that which passed through chloride of calcium was again added, until a great excess of it was contained in the liquid.

Both the precipitates, after having been well washed, dried, calcined, and weighed, were analysed in the following manner: first dissolved in nitric acid, the solution precipitated by an excess of acetate of lead, the precipitated phosphate of lead washed, calcined, weighed, and redissolved in nitric acid, the latter solution precipitated by sulphuric acid. By the weight of the sulphate of lead the amount of oxide of lead was calculated, and thus the amount of phosphoric acid ascertained.

The liquid from which the precipitate of lead was separated, was mixed with the evaporated water employed in washing the phosphate of lead, freed from lead by sulphuretted hydrogen, then supersaturated with ammonia, and precipitated by oxalic acid. The oxalate of lime was calcined, dissolved in hydrochloric acid, converted into sulphate, the acids evaporated off, the sulphate of lime calcined by a spirit lamp until its weight was no further diminished. From the weight of the sulphate of lime, the amount of lime was calculated.

The first of these basic salts was found to be composed of:

	Found.	Atoms.	Calculated.
Phosphoric acid	48·5	3	48·737
Lime	51·5	8	51·263

Hence, it had the above composition.

The basic salt, the second precipitated, was left for twenty-four hours in contact with the liquid, in which lime and ammonia predominated, and then filtered, when it contained 46·658 per cent. of phosphoric acid, was, therefore, constituted in its passage to 3 Ca O, P O^5, without being completely converted into it.

A solution of chloride of calcium was then mixed with a great excess of ammonia, and precipitated with phosphate of ammonia, without, however, having thrown down the whole of the lime. On analysis, this precipitate proved to be 3 Ca O, P O^5, and was, therefore, an entirely basic phosphate of lime.

This precipitate differs in appearance from the foregoing. It is not so gelatinous and semitransparent as this, but rather mucilaginous, resembling the artificially prepared fluoride of calcium. Dried and calcined, it exhibits an earthy fracture, whereas the preceding is rather compact, and somewhat shining.

From these observations an uncertainty is left relative to the point of saturation of phosphate of lime, which is contained in bones; bones likewise contain carbonate of lime, and on precipitating a solution of calcined bones in hydrochloric acid, by caustic ammonia, the precipitate thrown down first, is certainly = 8 Ca O, P O^5, but, at the end 3 Ca O, P O^5 is formed, and the obtained precipitate may be a mixture of the two. They, therefore, deserve a new investigation, which may be most properly carried on in the following manner:—the powder of the same bone is divided into two portions: one is

* *Öfversigt af Kongl. Vat. Akad. Förhandlingar*, 1844, No. 6; and Liebig's *Annalen*, February, 1845.

dissolved in hydrochloric acid, the amount of its carbonic acid disengaged being determined with the utmost accuracy; the other portion is then calcined until a white residue remains, from which the amount of lime, as well as of phosphoric acid, is determined, and the proportion of lime relative to the two acids, indicates the point of saturation.

—nt—

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ON THE VALUE OF MANURES.

"Dicere verum—quis vetat."—HORACE.

It has ever been our desire and endeavor, at every opportunity, to devote our columns to the exposure of those abuses which may be legitimately treated of in a chemical journal. We have never flinched from this stern duty, which we imposed on ourselves on establishing this work. In many, if not all, instances in which we have done so, considerable benefit has resulted. Fearlessly did we begin, and fearlessly will we continue, to wield the pen in the cause of honesty; no considerations of the influence or wealth of offenders, ever has or ever shall shield them from our exposure of their ignorance or falsehood; nor will threats, empty or earnest, induce us to retract truth or foster mendacity. Persons who err from ignorance, when their dealings should be based on knowledge, are nearly as culpable as those who act from base motives.

It is not in our power to determine the motives on which men have acted; nor can we undertake to decide whether ignorance or design has to answer for certain abuses; but we may assert that there is no excuse for either.

We are led to these observations by circumstances which have occurred since the publication, in our last number, of an article animadverting on the delusive tendency of a certificate of analysis of a sample of Peruvian guano, signed And. Ure, M.D.

When we undertook to show the valuelessness of this certificate, we had seen only that portion of it which we printed; but we have since made it our business to procure an entire copy, which we subjoin:—

(Copy.)

"This is a cleanly collected primitive guano in a state of entire preservation, as the particulars of the following minute analysis prove." [So in the original.]

"Its specific gravity is only 1·635 to water 1·000, being that of the genuine excrement of the birds when dried or mummified.—The constituents of the said guano in 100 parts are,

1st. Organic animal matter, including 4 parts of the uric acid or urate of ammonia, and other ammoniacal salts containing 3½ of actual and 11½ of potential ammonia, in all 15 of pure ammonia	64·5
2nd. Fixed alkaline salts rich in sulphate of potash, so valuable for grain crops along with sulphate and muriate of soda	4·0
3rd. Phosphate of lime with a little phosphate of magnesia. Bone earth	20·0
4th. Siliceous granitic sand	3·5
5th. Moisture ..... only	8·0
	100·0

"The proportion of rich organic matter in this guano is double of that in the average Ichaboe (African); and the ammonia is 70 per cent. more.

"From its large amount of *potential* ammonia its beneficial operation will continue for 4 or 5 seasons.

"(Signed) ANDREW URE, M.D., F.R.S.E."

The first paragraph of this luminous document is as difficult of comprehension by any ordinary capacity as it is possible to be, and we hesitate not to say, that every one who reads it will give a different construction. Does the doctor mean to state, that the sample he examined contained four parts of uric acid, or four parts of urate of ammonia, or four parts of a mixture of the two? Why are we left in uncertainty as to whether the four parts are uric acid or urate of ammonia? Did he not determine which? Is this a "minute" analysis?—He proceeds: "and other ammoniacal salts *containing* 3½ of actual and 11½ of potential ammonia, in all 15 of *pure* ammonia." For some time we were at a loss to understand the distinction here made between actual and potential ammonia: this difficulty would not have

been so great had he not made use of the word "containing." He means, we believe, by potential ammonia, that amount of ammonia which may be obtained by subjecting the substance to destructive distillation, and which ammonia does not exist as such, but that the guano contains substances capable, under certain circumstances, of producing ammonia. We were also led astray in our conclusions by the amount he found it to *contain*, both actual and potential, as it is not so very much more than it should have contained of actual alone, especially when we consider that this was a very wonderful sample, whose virtues the doctor had to publish to the world.

As regards the potential ammonia, as he calls it—which, by the bye, we wish it to be thoroughly understood is no more ammonia in the substance as it is found than a piece of rump-steak suspended from a hook in a butcher's shop is—we would ask the doctor what proof he has that the substance from which his potential ammonia was formed by heat will ever be converted into ammonia in the soil? It must be borne in mind that the substance from which this ammonia was formed is uric acid, of which we found the guano which was the subject of his analysis to contain, not four per cent., but a most extraordinarily large quantity. Now, this uric acid has existed as such for many centuries; it is, in fact, when in combination, one of the most indestructible of the organic acids, and we should very much like to know on what data he calculates on its conversion into ammonia in the course of four or five seasons; and even should that period be capable of effecting its decomposition, whether he can ensure its remaining stationary so as to shed its "beneficial" influence where it is placed.

We strongly condemn the practice of stating as ammonia in a certificate of this description,—especially when coupled with the word "containing"—the substances which it contains capable of forming ammonia by the application of a strong heat; the word "containing" leads the mind to the conclusion, that that amount of ammonia already exists, whereas, the fact is, that the sample examined by Dr. Ure contained, according to his own analysis only 3½ per cent. of ammonia.*

A chemical analysis should declare the substances which *exist* in the sample when examined, and any additional explanation might point out, if required, the changes which would take place under certain circumstances. Any thing contrary to this is unfair, and most decidedly improper, as the peculiar conditions requisite for the change may never be accomplished. What would be thought of the man who, in performing the analysis of a metallic ore containing sulphur, should return the sulphur as sulphuric acid? This would be a case in point, as, very probably, he converted the sulphur into sulphuric acid to determine its amount. The sulphur in such an ore is as much sulphuric acid as the nitrogen in a sample of guano is ammonia, except, of course, that nitrogen which exists in the already naturally formed ammonia.

It was our intention to have castigated the author of this most miserable document with the severity it so richly merits; but, after due reflection, we concluded that nothing we could say on the subject could be so severe as the publication of the absurdity itself.

The "puff system," in all its various appliances, is a contemptible one, and we invariably avoid all dealings with those who adopt it in trade; but doubly contemptible is it when introduced into science. Never, indeed, have we seen a certificate purporting to be written by a man who desires to be regarded as scientific, which might be so readily altered into an appropriate placard for a grocer's shop—"Moisture only 8!" Why not say, "Bone phosphate of lime only 20?" Wherefore is the "only" thrust in on this occasion?

We are informed by the Dr. that his analysis is a "minute" one. How, in the name of truth, can he call that analysis of guano a minute one which contains only five items? Why a *minute* analysis discovers that number of salts of ammonia alone!

As regards referring the value of guano to the ammonia it contains, we would observe, that in the present imperfect state of our knowledge concerning the fertilising powers of different manures it is absolutely ridiculous to assign a money value for any fertilising agent whatever, or, indeed, to specify a relative value short of that which has been found by actual experiment, and which is correct only under the circumstances and in the particular situation in which the experiments were made: so much, indeed, depends on the weather, the nature of the soil, the nature of the seed, and the proper adjustment of the manure to the exigencies of the case, that the most discordant results have been obtained, not only by different individuals, but even by the same individuals under apparently precisely similar conditions. Thus,

* Our analysis of a sample of the *same* cargo indicated a much greater quantity of ammonia. Of this, more at a future period. —EDS. CHEM.]

a most intelligent farmer in the North of England has found Ichaboe guano superior to the Peruvian for turnips, quite equal to it for wheat, barley, oats, and clover, and inferior only as regards grass; he also finds unburnt bones, ground and delivered with the drill, superior to both kinds of guano for turnips, and sulphate of ammonia superior to both for grass. The soil is the average clay land so common in Northumberland.

Nor can the value of a manure be estimated by the amount of nitrogen or ammonia contained in it, for in this view the phosphate and sulphate of lime would be valueless, and the nitrate of ammonia ought to possess an enormous fertilising power—arguments directly opposed to the facts of the case—for the value of the phosphate and sulphate of lime is abundantly established, and our own practical experiments with the nitrate of ammonia enable us to testify to its very trifling amount of fertilising power in any case, whilst in some instances it is unquestionably hurtful. A question, indeed, arises whether ammonia is to be regarded in any other light than as a stimulant to vegetation; its effects upon plants containing no nitrogen assuredly points out that it possesses an action independant of its power of supplying nitrogen, and in this respect it agrees with the nitrate of soda and other substances possessing an evanescent power, and regarded as mere stimulants to vegetable life, highly beneficial, no doubt, in the particular cases in which stimulants are required, but not generally applicable to crops which remain long in the ground, as corn and turnips.

Of the class of stimulants, we have seen none equal to the nitrate of urea, as regards intensity of effect, quickness of action, or durability, whether applied to corn or grass; but with all this we cannot be brought to regard any one manure as universally applicable even to the same kind of crops. Like the nostrum-mongers who pretend to cure all diseases by one remedy, the manure-merchants of the present day wish to convince us that all disorders of the soil and weather must give way to ammonia, to bone-phosphate, to humas, or to some other infallible specific, which is to be had in any quantity at their several warehouses. Now, it appears to us that until the defective nature of the soil has been pointed out, any attempt at a cure must of necessity be performed by that very antique process termed the rule of thumb, a mode of improvement which, like the progress of a squirrel in a cage, invariably ends where it began.

It may fairly be asserted that, up to the present day, no practical attempt at the analysis of soil has ever been made; the fertilising agents contained in soil exist in such minute proportion that in the quantities usually taken for analysis they are perfectly inappreciable. As an illustration of the fact, let us suppose that an acre of land with an available soil of nine inches in depth has been manured with two cwt. of the best guano; the effect of this would assuredly be very perceptible upon the crops; but let us examine the actual amount of guano which has been thus introduced into the soil, and point out the proportion contained in 400 grains, the quantity commonly taken for analysis and recommended by the authority of Sir Humphrey Davy; now, in an acre of land we find 4,840 square yards, and assuming the specific gravity at no more than 1,500 to water 1,000, we find that, with a depth of only nine inches, our two cwt. of guano has been diffused over 3,000,000 lbs. of soil, or that the soil on being analysed would yield $\frac{1}{18088}$ of its weight of guano; how, then, by employing only 400 grains of soil for an analysis can we expect to determine the real deficiency of a soil, when in upwards of 13,000 grains we find only one of guano, and even one-half of this is, perhaps, useless, and contributes nothing to the effect which such an addition would produce on the crop.

This calculation has been made in a manner the least favorable to our argument, as the average specific gravity of soils is much nearer 2,000 than the sum we have taken.

We have never yet lent ourselves to the mean artifices of applauding one article and condemning another of equal value: our wish is, to encourage every honest endeavor for the improvement of the arts and sciences; when, however, we find individuals striving, not to raise themselves, but to become conspicuous merely by depressing others, we fearlessly step in as the advocates of justice; on this principle, we have undertaken the defence of the African guano, from Dr. Ure's aspersions; and, on this principle, we shall ever be found ready to sacrifice our time, and all we possess for the real interest of our country.

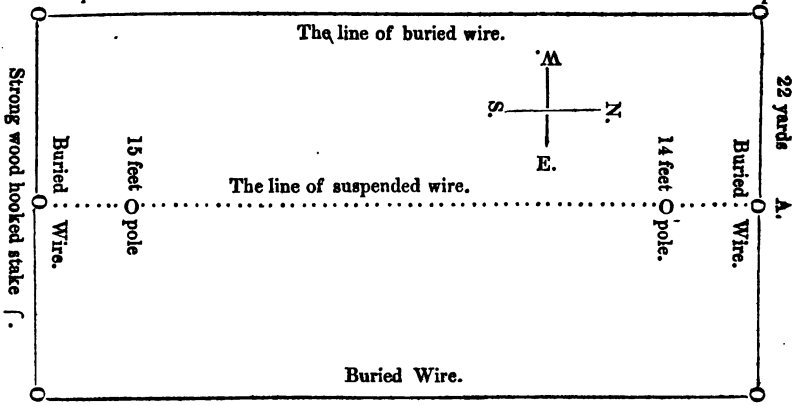
ELECTRICITY APPLIED TO THE CULTIVATION OF CORN.

The following interesting statement of the result of the free electricity of the atmosphere to the cultivation of corn was recently laid before the Geological and Polytechnic Society of the West Riding of Yorkshire:—

Dr. Forster, of Findrassie House, near Elgin, had threshed, weighed, and measured

his electro-cultured chevalier barley, and the product was the enormous quantity of 104 bushels or thirteen quarters per acre! The tail corn was not measured, and each bushed weighed 54½lbs. The weight of the straw was 9,300lbs. per acre. The cost of the electric apparatus is £1. per acre, which will last for 20 years.

The following is the plan of a plot for a quarter of an Acre :—



	COST.	s.	d.
6lb. of iron wire at 4*d.* per lb. for buried wire		2	0
4lb. of ditto at 3*d.* per lb. for suspended wire		1	0
2 poles of dry wood at 6*d.* each		1	0
Labour, &c.		1	0
		5	0

As the area increases, the cost diminishes rapidly.

Convenient and desirable areas are for—

Two acres, 127 by 75 yards.

One acre, 80 by 55.

¾ of an acre, 82½ by 44.

½ of an acre, 73½ by 33 yards.

¼ of ditto, 55 by 22.

⅛ of ditto, 36 by 16½.

The mode in which the plot is laid out is as follows :—With a mariner's compass and measured lenghts of common string, lay out the places for the wooden pins, to which the *buried* wire is attached (by passing through a small staple). Care must be taken to lay the length of the buried wire due north and south by compass, and the breadth due east and west. ***This wire must be placed from two to three inches deep*** in the soil. The lines of the buried wire are then completed. The *suspended* wire must be attached and in contact with the buried wires at both of its ends. A wooden pin with a staple must therefore be driven in at A, and the two poles (one 14 feet and the other 15 feet) being placed by the compass due north and south, the wire is placed over them and fastened to the wooden stake, but touching likewise at this point the buried wire. The suspended wire must not be drawn too tight, otherwise the wind will break it.

The application of the electricity of the atmosphere upon a large scale for the purpose of agriculture is a discovery which, if successful (and there is every theoretical reason that it should be so), will exercise a most important influence upon its interests. Dr. Forster here has obtained more than three times the average amount of both barley and straw (four or five quarters being the average, while he has thirteen quarters, and about 3,000 lbs. of straw while he he has 9,300 lbs.) The condition of the air in regard to electricity has evidently a most striking influence on the rapidity of the growth of plants, most of which increase in the most extraordinary manner during thundery weather. Nitric acid, a most important element in the food of plants, is formed in the atmosphere during thunder storms; and at these periods free electricity in considerable quantity can be drawn from the air by flying kites with wire strings. There is also a general electric current over the earth's surface from east to west, and both the terrestrial and aërial currents are here collected by the suspended and bu-

ried wires, and is again abstracted by the moist earth and the roots, which when wet become conductors of electricity. The application of electricity to field culture is quite in its infancy, and probably many improvements will be discovered; whether one or more suspended wires should be added, or galvanic troughs placed in the field to supply additional electric fluid, are yet subjects to be determined. It is unnecessary to add that the electric fluid acts as a stimulant, and therefore the usual quantity of manures must be applied. Under the direction of Mr. Gordon, the president of the Tring Agricultural Association, many of its members are trying the experiment. Mr. Mechi, at Tip Tree hall, is trying 50 acres; Mr. Briggs, at Overton, near Wakefield; Capt. Newton, of Womerley Grove; are likewise about to try it upon their barley crops.

ON THE QUANTITATIVE DETERMINATION OF SODA, AND ITS SEPARATION FROM POTASSA.*

BY DR. WITTSTEIN.

To separate potassa from soda, Pesier † ercommends us to employ the hyperchlorate of baryta instead of the chloride of platinum. Of the two objections which he makes to the process with platinum, viz. 1, no complete insolubility of the chloride of platinum and potassium; and 2, that the soda is to be determined only by the loss. The first is wholly untenable, because the above salt of potassium, though sparingly soluble in water, is entirely insoluble in spirit of wine, whereas the chloride of platinum and sodium easily dissolves in alcohol; and the second objection has no weight, if the process is conducted with that exactness which is required in all quantitative analyses.

Pesier proceeds as follows: the mixture of potassa and soda (potassa suspected to contain soda, &c.) is to be converted into sulphates, the mass decomposed by an excess of hyperchlorate of baryta, by which hyperchlorates of potassa and soda, and sulphate of baryta are formed, then filtered and the liquid evaporated to dryness. Absolute alcohol extracts from this the hyperchlorate of soda, and the excess of the hyperchlorate of baryta added, leaving behind the hyperchlorate of potassa. The alcoholic solution is to be evaporated, the sulphate of baryta separated by filtration, and the soda obtained from the liquid as a sulphate.

It seems to me that these two expensive methods may be rendered entirely superfluous by the so-called *indirect analysis*.* I will endeavor in the following, by minutely stating the method to be adopted, and by possibly simplifying the calculation, to bring these methods of determining into more general application.

The two bases are first converted into sulphates, then calcined, weighed, and the sulphuric acid found in them determined by a salt of baryta. On subtracting the acid from the saline mixture, the sum of the quantities of the two alkalies becomes known.

The quantity of the *potassa* may be ascertained, if we first subtract from the weight of the sum of the two salts, the product obtained by multiplying the weight of the sum of the two bases by the quotient resulting from the division of the atomic weight of sulphate of soda by the atomic weight of soda, and then divide this by the difference resulting on subtracting the quotient obtained by dividing the atomic weight of sulphate of potassa by the atomic weight of potassa, from the quotient obtained by dividing the atomic weight of sulphate of soda by the atomic weight of soda.

The amount of the soda may be ascertained, if the sum of the weight of the two bases be multiplied by the quotient obtained on dividing the atomic weight of sulphate of potassa by the atomic weight of potassa; and from the product thus obtained, the weight of the sum of the two salts is subtracted; and what is now left is again divided by the difference resulting on subtracting the quotient obtained, by dividing the atomic weight of sulphate of soda by the atomic weight of soda from the quotient obtained, by dividing the atomic weight of sulphate of potassa by the atomic weight of potassa.

We express by—

K, the atomic weight of potassa (kali)
N, ,, of soda (natron)
S, ,, of the two salts
b, ,, of the two bases
q, quotient obtained on dividing the atomic weight of sulphate of potassa by potassa
q, quotient obtained on dividing the atomic weight of sulphate of soda by soda

And thus represent the above by the following equations relative to potassa:—

$$K = \frac{S - b\,q'}{q - q'}$$

* Buchner's *Repertorium*, xxxvi. 3.

† *Journal de Pharmacie et de Chimie*, October, 1844.

* *Handwörterbuch d. Chemie v. Liebig, Poggendorff u. Wöhler*, vol. i., p. 355.

relative to soda:

$$N = \frac{b\,q - S}{q - q'}$$

It is evident that q q′ and q—q′ are unchangeable magnitudes, which are in all cases the same; namely,

$$\frac{q = 1091{\cdot}081}{589{\cdot}916} = 1{\cdot}84955$$

$$\frac{q' = 892{\cdot}062}{390{\cdot}897} = 2{\cdot}28209$$

$$q - q' = 1{\cdot}84955 - 2{\cdot}28209 = 0{\cdot}43254.$$

An example may serve to render the above more clear still. Let us suppose that the two sulphates weigh together 100 grains, the sulphuric acid (determined by baryta) 48 grains, consequently the two bases must weigh 52 grains.

To find K (the weight of the potassa), b (the weight of the two bases) = 52 we multiply with q′ = 2·28209, and then subtract the obtained product = 118·66868 from S (the weight of the two salts) = 100, and divide the rest = —18·66868 by q—q′ = —0·43254 by which 43·15 are obtained. These 43·15 of potassa to be converted into neutral sulphate of potassa will require 36·63 sulphuric acid.

To find N (the weight of the soda), we multiply b = 52 with q = 1·84955, deduct from the product 96 1766, S = 100, and divide the remainder = —3·8234 by q—q′ = 0·43254, by which 8·83 will be obtained. These 8·83 of soda to be converted into neutral sulphate of soda, require 11·34 of sulphuric acid.

43·15 potassa + 8·83 soda, added together, amount to 51·98; and 36·63 + 11·34 sulphuric acid amount to 47·97.

The precision in the determination of the two bases is certainly not susceptible of greater perfection, and scarcely attainable by direct analysis. And it might suffice to calculate only one base, and by deducting its weight from the sum of the weight of both, find the weight of the other base; for the sake of greater accuracy, however, it is preferable to calculate each. It is, of course, understood, that the weight of the two salts and that of the sulphuric acid are to be determined with the utmost care. On converting the bases into sulphates, an excess of sulphuric acid is unavoidable, which is to be removed by calcination. This operation of calcining which is usually a very slow one, may be quickened by throwing, from time to time, carbonate of ammonia into the mass.

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CHEMICAL INVESTIGATION OF THE CARBONATE OF SODA OF COMMERCE.*

BY DR. GEISELER.

Soda is manufactured in large quantities in our days, from common salt, by treating the latter body with sulphuric acid, and thus converting it into sulphate of soda, and hydrochloric acid, and then decomposing the sulphate of soda by coal or turf, &c., and carbonate of lime, when carbonate of soda is produced. The latter is sometimes so little uniform in its amount, so impure, and so little to be depended upon,† that it is frequently chemically examined in the pharmaceutical laboratories, especially, when it is to be employed as a chemical preparation, for the manufacture of soap, to wash linen by steam, &c. We commonly discover in it sulphate of soda, sub-sulphite of soda, and lime, sulphuret of sodium, chloride of sodium, iron, &c., in variable, and often considerable quantities. Dr. Geiseler communicated, in the *Archiv. der Pharmac.* vol. xxxix. p. 12, et seq. a process enabling quantitatively to ascertain the constituents of the soda of commerce, which, though complicated, and not to be executed by every manufacturer, is the more valuable to the scientific chemist: wherefore we communicate this method in the following:—

1. To ascertain the per centage amount of carbonate of soda, 100 grammes of the soda of commerce are dissolved in pure water, and neutralized with an acid, which in 100 drachms contains 34 grammes of concentrated English sulphuric acid of 1·84 specific gravity. Each drachm of this diluted acid indicates 1 per cent. of pure carbonate of soda.

2. 100 grammes of the carbonate of soda under examination, are dissolved in 300 grammes of pure water, and the filtered solution decomposed with chloride of barium. The precipitate contains the whole of the carbonic acid, the sulphuric and hyposulphurous acids.

3. 100 grammes carbonate of soda dissolved in 300 grammes of water, supersaturated with hydrochloric acid, and heated, on being precipitated after cooling by chloride of barium, gives only sulphate of baryta, by which the amount of sulphate of soda may easily be calculated.

4. 100 grammes carbonate of soda, on

* Buchner's *Repertorium*, xxxvi. 3.

† The editor of the *Repertorium* received one day from a manufactory a substance under the name of carbonate of soda, which should rather have been called sulphate of soda contaminated with carbonate of soda.

being dissolved in 300 grammes of water, exactly neutralised with diluted acetic acid, and but gently heated, until the whole of its carbonic acid be expelled, give, with chloride of barium, not only sulphate, but also hyposulphite of barytes; if this precipitate is dried, and then its quantity compared with the baryta precipitates of Nos. 2 and 3, the quantity of the hyposulphite of soda will become known.

5. 100 grammes of the carbonate of soda, supersaturated in the watery solution with diluted nitric acid, then filtered and precipitated with a solution of sulphate of silver, give only chloride of silver, by which the amount of sea-salt contained in the soda is to be calculated.

6. 100 grammes of carbonate of soda are digested with a sufficient quantity of spirit of wine, containing about 75 per cent. of pure alcohol; to the filtered alcoholic liquid, which is to be diluted with water, we add ammonio-nitrate of silver so long as a black precipitate is thrown down, which is sulphuret of silver, and indicates the amount of sulphuret of sodium, and perhaps, likewise, of sulphuret of calcium present in the substance under examination.

7. 100 grammes of carbonate of soda are dissolved in water, neutralised with hydrochloric acid, the solution mixed with so much alcohol as may be done without causing a turbidity in the liquid, and then examined by a solution of chloride of platinum; if a precipitate is thrown down sulphate of potassa is present in the soda.

8. If the sediment obtained by the solution of 100 grammes of soda in 300 of water is dissolved in hydrochloric acid, and this solution supersaturated with caustic liquid ammonia, the precipitate then produced will generally be hydrate of oxide of iron. From the ammoniacal liquid, after filtering, the lime may be precipitated, and quantitatively determined by oxalate of ammonia.

To facilitate the calculation, Dr. Geiseler gives the following:—

100 carbonate of baryta indicate 140 carbonate of soda.

100 sulphate of baryta indicate 138 sulphate of soda.

100 hyposulphite of baryta indicate 62 hyposulphite of soda.

100 chloride of silver indicate 41 chloride of sodium.

100 sulphuret of silver indicate 32 sulphuret of sodium.

100 chloride of platinum and potassium indicate 19 potassa.

100 oxalate of lime indicate 68 carbonate of lime.

Of the kinds of soda examined by Dr. Geiseler, the purest contained 96 per cent., the most contaminated one, 60 per cent. of carbonate of soda.

The chemically pure carbonate of soda may be obtained either by heating the bicarbonate of soda, or as counseller Dr. Wackenroder showed already by causing to freeze a solution of carbonate of soda of commerce. If, the carbonate of soda of the manufactures is dissolved in double its quantity of hot water, and the solution afterwards exposed to a cold of at least 18°F, to cause the whole to become one frozen lump, and this mass again warmed to 42° or 60°F., the sulphate of soda will be dissolved, and the carbonate of soda remain crystallised. If it then still retains some sulphate of soda, the process must be repeated until scarcely a trace of the said salt may be discovered. As, however, the bicarbonate is now manufactured on a large scale, and sold at very reasonable prices, in preparing from it the pure simple carbonate of soda, we have the advantage, that the process of purifying it and conversion into the latter salt may very conveniently be executed in any season of the year. For if it is washed with water until the filtered solution no more reacts on sulphuric and hydrochloric acids, the pure bicarbonate is obtained, which, to be dissolved, requires 13 parts of cold water, and on being moderately heated gives up one half of its carbonic acid. By dissolving in boiling water, and allowing the solution to cool again, the crystalline hydrate of the simple carbonate of soda is obtained.

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EXAMINATION OF THE VIEWS ADOPTED BY LIEBIG ON THE NUTRITION OF PLANTS.*

BY DR. SELLER.

Dr. Seller contrasted Liebig's view of the mineral nature of the food of plants with that which represents their food as organic, and traced out the consequences deducible from this last hypothesis as affecting not merely the vegetable, but the animal kingdom also, the latter being ultimately sustained solely by vegetable substances. He showed that, whereas the view adopted by Liebig in no-wise restricts the duration of the organised kingdoms, so long as they remain exempt from the influence of destructive agencies from without, the opposite view involves the conclusion that the whole of organic nature

* Read before the Botanical Society of Edinburgh, Feb. 13, 1845.

is hastening rapidly to dissolution from inherent causes; and he affirmed that were certain data somewhat more carefully considered, the period of the final extinction of plants and animals, in accordance with this hypothesis, might be pretty nearly determined.

He regarded this question as one not merely of high interest in itself, but as bearing expressly on the solution of the problem, whether the food of plants be organic or mineral. Dr. S. calculates the annual conversion of the carbon of organic matter into inorganic carbonic acid at not less than 600,000,000 tons; and infers, on the most favorable aspect of the amount of soil over the earth's surface, that such an annual loss could not be withstood beyond 6,000 years; and, on a less exaggerated assumption of its amount, probably very near the truth, that the waste would absorb the whole of the existing organic matter of the soil in about 740 years.

Dr. S. contends that the truth of these conclusions remains unaltered, even if it be conceded that much of the carbon of plants is drawn, not from the organic matter of the soil, but from the inorganic carbonic acid of the atmosphere, unless some inorganic source of their hydrogen and oxygen be at the same time admitted. He therefore regards Liebig's view of the inorganic nature of the food of plants as supported, not merely by many special facts—for example, by the increase of the organic matter of the soil, often observed during the growth of plants—but also by the general view of the earth's surface just taken, because there is nothing in its aspect to warrant the idea that its means of maintaining the organic kingdoms are declining with the rapidity indicated in the statements just made.

He next examined Liebig's views of ammonia.

1st. As the sole source of the nitrogen of plants, and thereby of animals.

2nd. As having its exclusive origin from the interior of the earth, and never from the nitrogen of the atmosphere. In regard to these statements he made it appear, as there is no evidence of ammonia being thrown forth from the bowels of the earth at all times, in quantities proportioned to the waste of it necessarily sustained at the surface, by decomposition into uncombined hydrogen and nitrogen, that Liebig's view of ammonia infers the same limitation of the existence of the organic kingdoms to a few thousand years, as is deduced from the hypothesis of organic matter being the food of plants.

Here, therefore, he differed from Liebig, contending that ammonia must be produced from the nitrogen of the atmosphere, and showing the probability of what is taught by Professor Johnson, namely, that the nitrogen of nitrates formed from the atmosphere, is fixed by plants, as well as the nitrogen of ammonia. In conclusion, he reviewed the evidence of potassa, the phosphates, and the other saline matters, of both organic kingdoms, being derived originally from the crumbling of rocks, and dwelt on the retardation of vegetable physiology, by the long scepticism of botanists on this head, owing, as he believed, to their distrust in the conclusions of chemistry; and went on to show that chemistry must be the ground-work of vegetable physiology in its present stage, and that the frequent changes in the aspect and nomenclature of chemistry did not materially affect the facts which it daily affords for the elucidation of the vegetable economy.

WHEN OUR SOIL IS POORLY, LET US CONSULT THE CHEMIST.*

I HOPE the time is approaching when we shall, for complaints of the soil, get advice from proper analytical chemists as readily as we would for ourselves, so that they may investigate the disease, and prescribe a proper remedy, depending on the patient's constitution and the nature of the ailment, whether chronic or transient, whether arising from plethora, starvation, or dropsy, too much water without the means of escape, a very usual complaint in earthy patients. Farmers make frequent mistakes when they trust to their own notions of doctoring: they readily perceive, by the external symptoms, that their patient is in an unsatisfactory state, but make sad havoc with their nostrums. How frequently do we see a dressing of dung forced on an already satiated patient, who cries out for an alterative of chalk or cold clay. Another administers a strong stimulant of lime, where the sufferer is already exhausted by previous excitement, and requires a tranquillising tonic of good old mellow manure. A third gives alkalies, where acids and phosphates can alone avail. They do not consider, that if troubled with stone, gravel, or density of the substratum, a gentle operation with the subsoiler might give ease by causing a looseness of the parts; or that in cases of scurviness and poverty—light, air, cleanliness, exercise, and good feeding, might prove a restorative. To be serious, it would be invidious in me to name particular individuals as well calculated to give proper ana-

* From Mr. Mechi's published *Letter on Agricultural Improvement.*

lysis and advice; but, I apprehend, every farmers' club and agricultural society will find their interest in seeking and availing themselves of those talented individuals who abound, and would be most happy to benefit agriculture and themselves, by the application of their chemical knowledge to increase the powesr of the soil. I hope we shall live to see a complete change in this respect; and that the frequent demand for such information will cause it to be given at a cheap and available rate. Much money and trouble would also be often saved by consulting competent surveyors and engineers, prior to altering and draining our estates.

BONE MANURE.*

Mr. Pusey informed the council, that the Prize Essay of Mr. Hannam, on the application of bones as a manure for the turnip crop, contained, in his opinion, not only a detailed account of the best experiment ever made in Agriculture, but some points of so much importance at the present season, when the time for sowing turnips was coming on, that he felt anxious that not a moment should be lost in communicating the facts to the members, for their information and guidance Mr. Pusey having adverted to the great discovery of the economical and efficient employment of bones as a manure when dissolved by maceration in sulphuric acid, proceeded to read extracts from Mr. Hannam's Prize Essay, of which the following is the substance:—

I. SUPERIORITY IN ECONOMY AND EFFECTS.

This result consisted not only in a greater amount of crop obtained at less cost than in the case of bones employed in their ordinary state, but also in the condition and character of the plant itself, as well as in its growth; the crops derived from dissolved bones being both more abundant, healthy, and less liable to attacks of insects, and the plant more rapid in its growth (by 10 days in the first month, and the gain of a month at the end, the turnips from the dissolved bones being ready for use several weeks before any other), while it exhibited a decided tendency to form bulbs at a much earlier period. The greater and more readily fertilising effects of bones dissolved in acid than any sort of bones not so prepared, were clearly shown in the following results; in which the crops from the dissolved bones took the lead from the commencement:—

Bushels of Bones per acre.	Cost of Bones per acre.	State of Bones.	Amount of Produce per acre.
	£ s. d.		tons. cwt. st. lb.
16	2 0 0	Crushed	10 3 4 8
2	.. ..	Dissolved	9 12 0 0
2	.. ..	Dissolved	11 15 0 0
4	.. ..	Dissolved	12 11 6 4
4	0 19 6	Dissolved	14 6 4 0
4	1 0 6	Dissolved	14 11 7 0
8	1 9 9	Dissolved	13 15 7 0
8	1 13 0	Dissolved	15 2 5 0
8	.. ..	Dissolved	16 1 3 6

From which it appeared, that 2 bushels of dissolved bones per acre would actually produce as good results as 16 bushels of bones in their ordinary state in some cases; that 8 bushels of dissolved bones would greatly surpass 16 bushels of bones used in any other manner; and that 4 bushels per acre would be a fair quantity to apply in the state of solution: the results being greatly superior to those from four-fold the same quantity of bones applied in the usual manner, and the cost of the application less in proportion to the amount of effect produced, evidenced by the weight of the crop, than that of any other quantity.

II. PROPORTIONS.

1. *Bones and Acids.*—The proportion of acid had generally been one-half of the weight of bone employed; but one-third, or even one-fourth of the weight of bone might be used with success, if occasion required. That was an important point in the economy of the manure, as it effected the cost materially. 2. *Water.*—The proportion of water had generally been 100 times the weight of acid; but 50 or 25 times the weight of acid would serve the purpose required very efficiently. That fact was one likely to be of great use, one of the main

* Royal Agricultural Society of England: meeting of April 16, 1845. From the *Agricultural Gazette*, April 19.

practical difficulties to contend with in the application of dissolved bones being the large quantity of water which had been considered necessary. Mr. Hannam's results showed that four bushels of bones (12 st.), 6 st. of acid, and 300 st. of water (50 times that of acid) gave 420 gallons of dissolved bones to be applied per acre; while 150 st. of water (25 times that of acid) would suffice. It was, however, worthy of notice, in reference to the preparation of the mixture, that the bones should be pulverised as much as possible before they are mixed with the acid, in order to render the solution more complete; but should that not be the case, and a few of the larger portions of bone be left undissolved, that circumstance would occasion no injurious effect, as there would be bone enough dissolved for the plant in its early stages, and the larger particles would disintegrate by degrees and come into operation towards the end of the season.

III.—DISSOLVED BONE COMPOST.

Mr. Pusey thought that however great a point was gained by reducing the bulk of the water employed for diluting the solution of the bones, a still greater step was attained by substituting altogether the compost for the liquid form of the application. In the Society's last Journal, p. 596, Mr. Tennant, of Shields, near Ayr, gave the following account of his practice on this head:—"I put 25 bushels into three old boilers (of which every farm here has a supply), and next pour in two bottles of acid, of about 170 lbs. each, and 36 Scotch pints (18 imperial gallons) of boiling water into each boiler. It boils away at a great rate for some time, and in a day or two we empty the boilers into two carts of light mould, and turn the mixture over. At this stage the bones are only partially dissolved, but they heat and decompose in the heap, after being turned over three or four times; and in the course of seven or eight weeks the compost becomes dry, and breaks down with a shovel in a fit state for spreading with the hand in the drill." Mr. Thompson stated that he had himself last year found 4 bushels of dissolved bones applied in the form of compost fully equal to 20 loads of farm-yard manure, there being no perceptible difference in the crop throughout the field.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

METHOD FOR THE STOPPAGE OF OVER-BLEEDING FROM LEECH BITES.*

Dr. Houston recommends the following plan for preventing excessive bleeding from leech bites, and which he has found invariably successful:—

Take a small pinch from the felt of a beaver hat, pile it on the bite; or if there be several bleeding points, pile one respectively on each, and spread over the whole a piece of thin muslin, drawing it tightly, so that any blood which flows must pass directly through both; then with a fine sponge keep drying up the blood as it oozes out, and in a very short time both felt and muslin will have become dried, by the coagulation of the blood in thin fine meshes, and the hemorrhage will have been effectually and permanently arrested. The muslin may then be all cut away, except the adhering points, which, in the course of a couple of days, will of themselves drop off, leaving the parts healed, and free from any such disfiguring marks as those which necessarily follow cautery, caustics, or needles.

The felt of a hat has been long regarded as a styptic; the chief point of novelty in Dr. Houston's plan, is that which relates to the mode of applying it, and which is obviously founded on rational and scientific principles.

* *Dublin Hospital Gazette.*

IODINE AGAINST AN IRON SPLINTER LODGED IN THE MEMBRANES OF THE EYE.*

BY DR. E. REINIGER.

Ch——, a cutler, required my assistance to remove a splinter of steel, which, during his occupation of sharpening, fell into his eye. He had already applied to a surgeon, who operated with pincette and needle, but without success. Eight days had elapsed since the accident had taken place, and the eye was very much reddened. The light shining

* Walther and Ammon's *Journal für Chirurgie*, iv. 1845.

steel splinter could be clearly seen with the naked eye, and stuck immoveably in the cornea in the centre of the pupilla. The patient complained of pungent and burning pains and diminished power of vision. A strongly mounted magnet was brought near to the eye in various directions, but fruitlessly. Nothing, therefore, was left but to remove the splinter, which was of the size of the point of a needle, by chemical means. To employ, as directed by Krimer and Andreae, diluted hydrochloric acid, seemed not advisable, these fomentations with cold water requiring to be continued for several hours, it was probable that the patient might have neglected them. Recollecting, the great affinity of iodine for iron I prescribed the following collyrium.

℞ Iodii, gr. i.
Kali Iodati, grs. x.
Aqua rosarum, ʒjii.

After the first application, the steel splinter became oxidised and tarnished. The redness of the eye soon lessened, the burning and pungent pains gradually remitted, and after the continued use of the collyrium, the steel point diminished to such an extent as to become visible only by the aid of the microscope. The patient fully recovered his sight, and was enabled to resume his work.

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HISTORY OF A LEGAL INVESTIGATION RELATIVE TO A SUSPICION OF POISONING WITH A PREPARATION OF LEAD.

BY M. ORFILA.

(Concluded from our last.)

We do not particularly mention the different objects which were less important, and on investigation gave merely negative results, as, for instance, the shirt of Pouchon, which he had on when found in his coffin, the earth taken from his grave, &c.

However, the investigation of the matters vomited by Pouchon previous to his death was important, which consisted:—

1. Of the substance which was scraped off from the pillow on which Pouchon lay when he expired, and on which portions of the vomited matters were found.

2. Of substances scraped off a trunk which was likewise soiled with the vomited matters.

3. Of scrapings obtained from another surface of the same trunk in which it was not soiled.

4. Of scrapings collected from the portion of the floor of the room situated between the bedstead and the said trunk, and from the spot in which the vomiting took place.

5. A parcel containing flour found on a shelf standing close to the bedstead of the deceased.

6. A shirt which it was maintained that Pouchon had on during his illness.

7. A night-utensil soiled with vomited matters, and alvine excretions. These objects, when brought into the laboratory, were, like the before-mentioned, provided with the seal of the justice.

The scrapings, No. 1, were mixed with pure caustic potassa and carbonised. The mass was then extracted with water to examine the coal left behind. Under the microscope, very small metallic globules became visible; by boiling with concentrated acetic acid an extract was obtained, which, on being evaporated to dryness, and fused at a red heat with potassa and charcoal in a porcelain capsule, furnished a mass in which, by the aid of a lens, metallic globules were noticed of a larger size than those seen before. This mass was afterwards boiled for half an hour with pure acetic acid, the liquid filtered and divided into three portions; into one of which a current of sulphuretted hydrogen was passed; by which a very copious black precipitate was thrown down; into the second portion sulphuric acid was thrown, drop by drop, until a turbidity was produced. In the third portion iodide of potassium threw down a shining scaly yellow precipitate.

The scrapings, Nos. 2 and 4, were not examined; and No. 7, the night-utensil, was also not investigated, owing to its glazing containing lead.

The scrapings, No. 3, which presented no traces of the vomited matters, were examined precisely in the same manner as No. 1; no lead could be discovered in it.

The parcel, No. 5, contained wheaten flour; on being tested for lead, negative results were obtained.

In giving their opinion, the above gentlemen drew the following inferences from their experiments:—

1. That the organs of Pouchon contained a considerable quantity of lead, as ascertained by physical and chemical characters.

2. That this metal was administered to Pouchon when alive, in a certain state of combination, as it was likewise discovered in the vomited matters.

3. That this lead cannot be considered as constituent of the human organisation as some authors have thought; and even though it were to be admitted that the human body in the normal state may contain traces of lead, still these would be merely traces, and not in

such large quantities as were found in the body of Pouchon, nor could it in this case have been contained in the vomited matters.

4. The preparations of lead, given in smaller or larger quantities, being poisonous, it may with certainty be presumed that Pouchon was poisoned by a preparation of lead, by which he came to his death, either as the sole effect of its poisonous qualities, or as a circumstance mightily promoting the fatal issue of the disease, under which the patient suffered already.

5. That the lead found in the organs of Pouchon could not be derived from the earth of his grave; finally, that the lead found in the vomited matters could not be attributed to the wood of the bedstead.

The commission then handed over to the judge four small sealed bottles, provided with inscriptions; one bottle contained the metallic lead extracted from the organs of Pouchon, in another the sulphate of lead prepared from it, in the third the sulphuret of lead thrown down by the sulphuretted hydrogen; finally, in the fourth, the iodide of lead obtained from the intestines. In two other small bottles, likewise given to the judge, were the sulphuret of lead and the iodide of lead, prepared from the matters vomited by Pouchon. In conluding the report, it was, moreover, observed, that the reagents employed in the legal investigation were furnished by the apothecary Barse, and that the nitrate of potassa, the nitric and hydrochloric acids, the caustic potassa, the porcelain capsules, the cast-iron pan, were all delivered up to serve again in any future controlling experiments; finally, the other objects, as the samples of earth collected at the grave, the night-utensil, &c., were likewise handed over to the judge.

Apothecary Barse maintained, that if it would be deemed necessary to nominate a commission controlling his experiments, he had a right to be present on the occasion, when the new chemical investigation would be instituted.

V. ABSTRACT OF THE OPINION GIVEN BY PROFESSOR ALHPHONSE DUPASQUIER, OF LYONS, DEAN OF THE MEDICAL JURY OF THE RHONE DEPARTMENT, &c., AND DIRECTED TO MESSIEURS RICHOND AND VALICON, ADVOCATES AT PUY, THE LEGAL ADVISERS OF THE DEFENDANTS MARY CAMUS AND ROCHER.

(Dated Lyons, 4 June, 1843.)

M. Dupasquier remarks at first, that he considers himself in a position to express a wholly unbiassed opinion, as the facts stated in the indictment are unknown to him, and he is not acquainted with either of the defendants, or their legal advisers. He therefore does ample justice to the chemico-legal investigation, the care and solicitude of the experimenters, and admits that the existence of a small quantity of lead in the body, though not ascertained in which combination, was really demonstrated. But he objects to the inferences drawn from the facts resulting from the chemical analysis, viz.: that all preparations of lead are poisons, that it may be presumed WITH CERTAINTY that Pouchon came by his death by poisoning with lead; that the small quantity of lead found in his body is an incontestable proof of his having been poisoned.

M. Dupasquier proved, by a series of experiments on dogs, instituted by himself in conjunction with Prof. Rey, and by quotations of statements made by Orfila, Devergie, and Chevallier, that all preparations of lead which are insoluble in the gastric juice, viz., sulphuret of lead, in both its states, in the natural, and that which is artificially prepared, by precipitation, the sulphate, oxalate, borate, and tannate of lead, may be taken in large doses,* without injuring health.

M. Dupasquier then compares Orfila's statements in his *Toxicology* (4th edit. 1843) relative to poisoning with lead, with the morbid symtoms, which were observed in the case of Pouchon, to direct attention to the difference of symptoms, and to prove that in the case under examination, certainty was out of the question, and that at most some degree of probability might be admitted. He further directed attention to the circumstance, that the small quantity of lead found, may be attributed to some accidental impurity taken by Pouchon with his food or drink, or to some remedies administered;†

* From four to five drachms of these combinations of lead were administered to the dogs, but no noxious effects were observed.

† In an annotation relative to this passage, Dupasquier says, that from the legal investigation, it afterwards appeared, that 15 or 18 months previous to the death of Pouchon, when in the hospital at Puy, on account of his complaint, 4 grammes (about 1 drachm) of acetate of lead, were prescribed and administered to him in the form of an enema, and this was done several times. On being asked by the president at the assizes, whether the lead found in the body could be derived from the said enemas, Dupasquier said: that in the present state of the science, this question cannot be answered with certainty; it is, by all means, true that the acetate of lead may partially be absorbed by the mucous membranes of the intestinal tube, and

and that in a man who suffered for four years from a chronic disease of the stomach, in whose body traces of a previous inflammation and suppuration of the stomach were met with, the disease which commenced after an error in the diet, and the symptoms attending it, as well as the death which followed, could easily be accounted for without any poisoning having taken place.

VI. ABSTRACT OF A SCIENTIFIC DEPOSITION MADE BY ORFILA BEFORE THE ASSIZE AT PUY, AUGUST 22, 1843.

The method adopted in the chemical investigation, and the care with which it was conducted, were fully approved from this quarter, also the detection of the lead in the body and in the vomited matters was not called into doubt by Orfila; but he, likewise, objected to the inferences drawn from the facts ascertained, owing to the circumstances that the combination in which the lead might have existed remained unknown, and that all of the combinations of lead are not poisonous, and that it was not proved that the reagents employed, especially the potassa, which sometimes is contaminated with lead, were perfectly free from an admixture of the latter metal.

A portion of potassa employed in the legal investigation ought first to be taken by the hands of the tribunal. That, however, the lead found in the legal investigation was to be attributed to an insoluble, and hence not poisonous combination, Orfila did not consider as probable, as similar lead combinations do not usually occur out of the laboratory.

It was conceded by Orfila that a considerable dose of a poisonous lead preparation, if internally administered, may produce symptoms similar to those by which Pouchon suffered previous to his death. That, however, the lead discovered by the legal investigation may have originated from the enemas administered to Pouchon eighteen months previous to his death, he deemed improbable, because a great portion of the enema was immediately after evacuated again with the fecal matters, and that portion of the lead salt which might have entered into combination with the mucous membrane of the rectum, or have been absorbed, must have been eliminated again from the organism, especially with the urine, in such a manner, that after 15 or 20 days, nothing could have remained, and that the least of all we might account by this circumstance for the lead found in the vomited matters.

VII. REPORT SENT TO THE PRESIDENT OF THE ASSIZES AT THE HAUTE-LOIRE, BY ORFILA, DUPAQSUIER, PORRAL, AND JUL. BARSE, DATED AUGUST 24, 1843.

The above experimenters were required to institute a fresh investigation to determine:

1. Whether the preparation of lead found in the body of Pouchon and in the vomited matters was soluble or not?

2. Whether the potassa, employed on a former occasion by Reynaud, Porral, and Barse, was free from lead or not?

To satisfy this summons they assembled on the 22nd of August in the laboratory of the pharmaceutical department of the Hôtel Dieu at Puy, and commenced the investigation of the remainder of Pouchon's body left from the former investigation, which were under the care of the authorities, observing thereby the formalities prescribed by the law. It, however, appeared that in the meantime the organic substance had become decayed and sour to such an extent as not to warrant any one in saying whether the lead was originally present in the soluble acid state of combination, in which it was then found. With regard to the second question, viz., whether the reagents employed were pure, it was ascertained that the *potassa à l'alcohol*, used by Porral, Reynaud, and Barse in their legal investigation, contained lead, indeed, but it could not be determined with certainty, whether the lead was originally present in the potassa, or only brought to it by the water employed, which was likewise found to be contaminated with lead.

For these reasons, and to remove all

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thence conveyed into the circulating liquid mass, that afterwards, however, it is excreted again by the urine, saliva, and intestinal mucus, that, moreover, a portion of the lead salt unites with the mucous membrane, producing white spots, which, in consequence of the absorption after three or four days, disappear again, in such a manner, that no trace can afterwards be perceived; that, however, after several days, and even months, lead may be detected, if the said mucous membranes are destroyed with boiling nitric acid. This was based on experiments stated in Orfila's *Toxicology* (4th edit. vol. i. p. 694). This fact is analagous to that known of nitrate of silver, viz., that epileptical individuals, to whom the latter salt was internally administered, contracted in their cuticle and other organs a silver combination, which could never be removed. But the matter vomited by Pouchon, shortly before his death, and in which lead was likewise found, could not have originated from the enemas of acetate of lead administered eighteen months back.

doubts, the commission most carefully, chemically examined No. 4, viz., the *scrapings* taken from that spot of *the floor of the room* which was situated between the trunk and bedstead of Pouchon, the place, therefore, in which he vomited. These scrapings were a mixture of fecal matters and pieces of straw and leaves; it was carbonised over an open fire in a porcelain capsule, and almost incinerated. An empyreumatic odor was thereby exhaled, which indicated the presence of an animal matter, and differed from the odor perceived on the carbonisation of the scrapings taken from the bedstead. The carbonaceous ashes obtained in that manner were extracted by pure hydrochloric acid, and by pure freshly-distilled water, in which no vestige of lead was found. Into this liquid, after being filtered through asbestos, a current of washed sulphuretted hydrogen was directed, which instantly threw down a copious precipitate of sulphuret of lead. One portion of this precipitate was put to the document, and another portion of it was employed in the experiment to ascertain the nature of the lead with complete certainty. These experiments, in which no potassa was employed, proved it beyond all doubt that lead was contained in both the said scrapings, of the floor as well of the beadstead, which MM. Regnaud, Porral, and Barse found already on a previous occasion to contain lead.

From these, Orfila concludes that it may be presumed, if not with certainty, at least with the highest degree of probability, that Pouchon died in consequence of poisoning.

VIII. CONTINUATION OF THE LEGAL INVESTIGATION AND CONVICTION OF THE DEFENDANTS.

The criminal process at the assizes at Puy terminated the 25th of August, when the accused were sentenced to death. Appellation was laid in, and the Court of Cassation, on account of an error which occurred in the examination of witnesses, annulled the sentence pronounced by the assizes, charging the assizes at Riom with the continuation of the investigation. New, violent, and extensive discussions were then raised, MM. Rognetta, Flandin, and Danger (the latter as the well-known passionate opponent of Orfila in the famous case of Madame Lafarge), besides Dupasquier, having been added to the persons already appointed for the defence of the accused. We do not intend to fatigue our readers with the circumstantial debates, as the main features of the litigation and contradiction have already appeared from the above elucidations, and all the other pathological and chemical subtleties are of no real value; on the contrary, they rather create injury by lessening the confidence placed in scientific investigations, and even cause them to be looked upon with suspicion. We must not, therefore, wonder if the general-procurator, a laïe in chemistry, before the assembled assizes at Riom cried out,—"La chimi est une science conjecturale; on dit seulement la consulter!" that is, a conjectural science, based on mere suppositions of an ambiguous nature, which may well be consulted; but is unable to demonstrate with certainty. The general-procurator might, perhaps, have reproached with this pathology, but not chemistry, which on many occasions furnished the tribunals with most decisive proofs, when it was asked, whether arsenic, or any other poison which admits of positive demonstration, was present or not.

In conclusion, we intend to point out that the defence of the accused, besides other arguments, was founded likewise on the circumstance, that, in the post mortem examination, only symptoms were detected of a former ulceration, but neither of a late inflammation nor of a fresh lesion in the stomach and intestinal tube, which, however, ought to have been the case if a poisonous lead salt had been taken in a dose sufficient to cause death. Against these, Orfila quoted a case of poisoning, which he published in the first edition of his *Toxicology*, in which 48 grammes (1½ ounces) of sugar of lead, became rapidly fatal, without leaving behind any inflammation of the intestines. He observed that Pouchon had suffered for many years from a complaint of the stomach, which might have caused a small dose of lead salt to become a fatal poison, though the same could not have done any injury to a healthy man.

When the debates were closed, the accused were found *guilty* of the crime of murder by poisoning, and sentenced to hard labor for life.

From this remarkable process we have to deduce many, though long-known truths, and impress them upon our mind:—

1. That, in legal investigations, we are to proceed with the most possible circumspection, precaution, and candor.

2. That, previous to commencing the chemical investigation of the corpus delicti, we are never to omit carefully to record the proofs obtained of the purity of the reagents employed.

3. That, previous to giving a professional opinion, we should fully and with the greatest sincerity consider what may be stated with certainty, and distinguish it from the hypotheses which are only capable of a greater or less degree of probability.

4. Finally, when the scientific and practical, and proofs of certainty or probability, are once pronounced, the sophistical and subtle arguments broached against them, by the advisers of the defendants, should not mislead either experimenters or judges.

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POISONING CAUSED WITH BITTER ALMONDS.*

BY DR. ROSSI.

A HEALTHY boy, three years of age, one morning, some time after having partaken of his breakfast, which consisted of an infusion of oak-tree glands with milk and white bread, informed his father that he suffered from colical pains. The parent took the child on his arm, but on being called for, intended to put the patient on the ground, but it being unable to stand on its feet, he was obliged to lay it down on the floor. The mother on entering, found the boy in this situation, who, while laying, vomited matters exhaling a strong odor. On raising him, his eyes were seen to be closed: he was senseless and cold.

On being called in by his parents, who could not in any way account for this sudden indisposition, I found the child on a sofa, in the following state. The face entirely pale, lips, nose, and ears, white and cold; the upper and lower parts of the body likewise cold and flaccid; the abdomen was not distended, but soft, and scarcely of the normal temperature. It was stated by the mother, that the boy had a copious stool the same morning. The pulse was scarcely perceptible, the heart's movements were feebly felt, the breathing was scarcely perceptible, the mouth was firmly closed, and in no way to be opened. The mother, who in the meantime, took the child in her lap, repeatedly called it, but it seemed not to hear; she then shook it, whereat matters were vomited exhaling a very strong odor of prussic acid.

From this I was induced to administer to the patient a strong emetic, by which masticated almonds were ejected repeatedly; this, in combination with the prussic odor, admitted of no doubt that bitter almonds had been taken. After this the child recovered its senses to a certain degree, and some hours having passed, the warmth of the body was gradually restored, and full consciousness returned; a motion of a soft consistence was evacuated, but not preserved for my inspection. An appropriate treatment having been continued for some days, the child was perfectly cured. It was then ascertained, that the child had been in a neighbouring shop, and had consumed a quantity of bitter almonds.

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* *Medicinische Zeitung*, December 18, 1845.

ON THE CAUSE OF THE COLORATION OF THE FECES AFTER THE USE OF THE MINERAL WATER OF MARIENBAD.*

BY BARON BERZELIUS.

(*From a letter to Prof. Kersten, in Friberg.*)

"Stockholm, 20th Aug., 1845.

"I AM much obliged to you for the promised communication of the examination of the waters of Marienbad. What you write on the circumstance of the feces becoming black during the use of the springs Ferdinandsquelle and Kreuz Brunnens, interested me uncommonly. This is, likewise, the case after the use of every ferruginous water, whether it contains sulphates or not, it is the same with preparations of iron in general.

"It never entered my mind to suspect that this coloration is to be derived from sulphuret of iron, but I always believed that it might be attributed to a suboxide—oxide of iron, which is, likewise, black. It is, however, quite natural that, as rather a considerable amount of sulphuretted hydrogen is usually produced during the process of digestion, the oxide of iron present in the intestinal tube is reduced to sulphuret of iron in the corresponding stage, no matter whether sulphates have been administered or not."

It appears from the foregoing, that Berzelius, to whom I have communicated my chemical experiments relative to ascertaining the cause of the green or black coloration of the stools during the use of the mineral waters at Marienbad, fully approves of my views explanatory of this phenomenon, viz.:—that they are produced by *sulphuret of iron*. Berzelius, however, renders this assertion of a more universal application, ascribing the green or black coloration of stools to *all* ferruginous mineral waters, be sulphates contained therein or not, and of preparations of iron, to the formation of sulphuret of iron in the intestinal tube, which cannot be well controverted. The colorations of stools, produced by ferruginous water, which, at the same time contain sulphates, might mostly be more intense than those which take place after the use of mineral waters

* Walther and Ammon's *Journal für Chirurgie*, vol. iv., 1845.

devoid of sulphates, because, in the first case, by decomposition of the sulphates, during digestion, decidedly a greater amount of sulphuretted hydrogen than usually is formed in the stomach.

In conclusion, I remark that these colorations originate from the simple and not the double sulphuret of iron, as it was, probably in consequence of an error in writing, stated in my first communication on this subject.

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POLYGONUM ARICULARE, L. IN CHRONIC DIARRHŒA.*

BY DR. C. HERZOG.

Who would ever have thought that the common vogel-knöterich or wegetritt, that well-known, almost tasteless, parasitical plant of universal occurrence would act a part in materia medica? In former times this annual plant was sometimes employed externally as a moderately astringent remedy in wounds and ulcers, and internally in diarrhœa and piles. It has indeed some acridity in its taste. *Polygonum aviculare* has since become of pharmaceutical interest in another respect; in the middle of the last century, after the *polygala amara* got renown in Vienna, as a remedy in mucosity of the breast and incipient phthisis, in its stead, for a long time was derived from Frankfort the vogel-knöterich; wherefore this plant became a subject for discussion in pharmaceutical works, to direct attention to mistakes and sophistication.

Some time ago the polygonum aviculare (Herba centumnodü, Hb. polygoni s. sanguinariæ of the ancients) was recommended by Dr. Baugeris in intermittent fevers. It is stated, that it is employed in Algiers as a domestic medicine in the above disease, and taken in the form of a decoction. Dierbach says, in his latest discoveries on materia medica (vol. ii., p. 184), that the administration of this plant is no novelty, it being directed already by Dioscorides to boil the polygonum with wine, and take it one hour previous to the fever paroxisms; and Plinius likewise spoke highly of the virtue of this plant in tertian and quartan fevers.

Lately Leverat Perreton directed attention to polygonum aviculare, recommending it again in chronic diarrhœa and dysentry. It is stated that, as a concentrated decoction, it was efficient in several cases in which the usual remedies were of no avail. (*Journal de Chirurgie*, No. 323, 1843.)

On this occasion I must remark that the polygonum aviculare contains no tannic acid, though the plant possesses somewhat an acrid taste; its watery infusion being mixed with a decoction of isinglass produces no turbidness, and with a solution of iron it shows only a brownish turbidness; the same is the case with chloride of iron. An extract prepared, at a gentle heat, with diluted acetic acid, and almost neutralised with ammonia, behaved in the same manner; this acid infusion, on being treated with tannic acid, threw down a flaky precipitate, which, however, seems to be attributed only to vegetable albumen. If the acetic infusion is neutralised with ammonia, the liquid becomes of a fine yellow color, and by evaporation a brown extract is obtained, which, besides an herbaceous saline taste, has no peculiarly remarkable flavor.

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* Buchner's *Repertorium*, vol. xxxvi., p. 3.

PHYSIOLOGICAL AND THERAPEUTICAL EXPERIMENTS ON THE BITTER PRINCIPLE OF DIGITALIS.*

BY M. HOMOLLE.

FIRST EXPERIMENT ON A YOUNG RABBIT, THE PULSATIONS OF WHOSE HEART HAD PREVIOUSLY BEEN, AND FOUND TO BE, 148 DOUBLE BEATS PER MINUTE.

On the 27th June, 1841, at twenty minutes past eight in the morning, 0 grammes 0·5 centigrammes of the impurified bitter principle of digitalis, just as it was obtained from the first jet, were introduced into the subcutaneous cellular tissue of the internal part of the right thigh of the rabbit; a point of suture was made to prevent the substance from issuing out. No inconvenience appeared immediately, but, at the end of nine hours, a tendency to repose, anxiety, inappetence, and general muscular tremor were observed. The beats of the heart were reduced to 124, then to 102.

On the 28th, at 8½ a.m., 108 beats were counted, and 10 centigrammes of the same product were introduced under the skin of the inner part of the left thigh. At noon, 102 beats were counted and the poisonous action appeared less decided than on the previous evening; but at three o'clock it was powerfully developed, the ears were pricked, the hair slightly bristled; 112 strong and vibrating pulsations.

At five o'clock, in the same state, the same character of the heart's pulsations, which had increased to 144; 120 inspirations were counted. Before the experiment from 60 to 64 had been counted.

* *Journal de Pharmacie.*

On the 29th, in the morning, the beats had increased to 198, and were strong and vibrating; the absorption of the bitter matter was completed, and the little wounds had cicatrized without any trace of suppuration.

The administration of the bitter principle was continued, but internally; thus, on the 29th and 30th June, successively, it took 15, 20, and 30 centigrammes, and each day after taking it, the beats of the heart decreased from 24 to 30, resuming their frequency by the next morning, but the general effects were much less marked, and the bitter principle was completely digested. The stools became liquid on the third day.

We do not here speak of the diuretic action; it was not possible to take account of the urine passed; from the 2nd to the 6th July the administration of this agent was suspended, and the beats of the heart remained almost uniformly at 186. Then, on the 7th and 8th, the bitter principle was again given in the dose of 10 centigrammes, and on each day a diminution of from 30 to 40 beats was observed.

It is not without some reserve that we present the details relative to the action on the circulation which appears to flow from this experiment: the difficulty of counting the beats being capable of giving rise to error; it is, however, impossible for this cause of error to completely destroy the value of such decided results. But there are some points on which we should here dwell:—

1st. The complete absorption of the better principle by the vessels, without any trace of inflammation or suppuration.

2nd. The complete absence of vomiting.

3rd. The poisonous effects obtained by the subcutaneous absorption of a dose amounting to one-fifth of that administered internally, the assimilating power of the stomach being sufficient for digesting an eminently poisonous matter. Can we not here perceive an example of that providential prescience, which gives to the herbivora the faculty of assimilating substances poisonous to other species? Finally, this absence of inflammation after the subcutaneous application of our bitter principle, will strike us still more when we see that a smaller dose caused in a dog inflammatory symptoms, of frightful intensity.

SECOND EXPERIMENT ON A YOUNG TERRIER, COMMENCED JULY 11, 1841.

On the 11th, 12th, 13th, and 14th of July, the average number of the heart's beats was 148.

On the 14th, at 9, a.m., 5 centigrammes of bitter principle were administered.

At 11 o'clock, there were two vomitings of slimy stringy matters. The pulse did not appear to be much modified.

On the 15th, at 8 o'clock in the morning, 5 centigrammes were administered. Besides the vomitings which occurred three times, there was a sanguinolent motion, and the beats increased to 154.

On the 16th, the same dose was administered, with the same results.

On the 17th, at half-past 9, a.m., 0·5 centigrammes of bitter principle were introduced under the skin of the inner part of the right thigh. The dog played immediately after, and did not appear either fatigued or suffering.

At half-past 12, 144 pulsations were counted, stronger than in the morning.

At 2, p.m., 162 pulsations.

At 3, p.m., 184 tremulous, irregular pulsations difficult to count. The dog lay in a corner, refused to eat or drink, appeared to walk with difficulty, and had hiccups. The urine was very abundant, and this fact is the more remarkable from the dog having refused to drink for several hours.

At 4 o'clock, 186 more distinct beats were counted. The animal had a suffering and stricken appearance, with difficulty kept his legs, and particularly dragged the right posterior leg. It was affected, if moved ever so little, by a general muscular tremor, always refusing food and drink, still passed urine, and had a semi-fluid motion.

At 9, p.m., 174 weaker but more simultaneous beats were counted; the dog refused to rise, and suffered itself to be dragged along.

On the 18th, at 9, a.m., 198 weaker beats.

The animal had neither eaten nor drank; it refused milk. The eyes were bleared. The hair dirty and dull. Considerable emaciation; the abdominal parietes extraordinarily retracted; the right thigh presented a considerable phlegmonous swelling.

Since the previous evening, there had been neither motion nor urine.

At 3, p.m., 192 beats, same state.

At 4 o'clock, the general state somewhat ameliorated (urine); the swelling of the thigh had not diminished; the dog still refused food; he drank a little milk.

On the 19th, the dog was more lively; he ate a little meat at noon. The emaciation continued, and the swelling of the thigh remained the same.

On the 20th, the small incision made in the skin of the thigh was replaced by a large opening with clear, livid and sphacelated edges;

an abundance of bloody pus flowed out, and the swelling appeared to diminish.

During the following days, the health of the animal continued to improve. The beats of the heart were between 160 and 168.

On the 28th, the wound was cicatrized; 168 beats were counted. 1 centigramme of the bitter principle was given to the dog in meat; no appreciable alteration.

On the 29th, 156 strong and regular pulsations; 1 centigramme of the same product was administered.

At half-past 2, the beats of the heart were tumultuous, irregular, and vibratory; there had been neither stools nor urine since the morning. 1 centigramme was again taken.

At half-past 3, there was a glairy vomiting; urine; depressed appearance.

On the 30th, at 9 o'clock, 180 pulsations. 1 centigramme of bitter principle was administered.

At noon, 168 pulsations.

At 8, p.m., 1 centigramme was again administered.

On the 31st, the beats of the heart were irregular and intermittent. 1 centigramme was administered at half-past 8, and 2 centigrammes at 3.

On the 1st August, 2 centigrammes were given to the dog at 9 a.m. At noon, the beats of the heart were tumultuous, and so irregular as to number and power, that it was impossible to count them accurately. The number was calculated at from 150 to 160.

In this experiment, the bitter principle presented a local inflammatory action sufficiently intense for determining the formation of a gangrenous phlegmon, instead of its application under the skin.

Administered with the food, it caused vomitings and sanguinolent stools, in the dose of 5 centigrammes. The vomitings took place after the administration of 2 centigrammes. The diuretic action appeared to manifest itself on the day of the application of the bitter principle under the skin; the rest of the time we did not perceive it.

The influence exerted on the circulation presented the following characters: considerable increase in the frequency of the beats of the heart during the development of the phlegmon; in other circumstances, the beats became at first fuller, vibratory, and sometimes less frequent, then tumultuous, irregular and intermittent.

The developement of the phlegmon and the febrile state which it determined would be sufficient, independently of the poisonous symptoms observed, to explain the considerably-increased frequency of the heart's beats; and in later experiments, in which the perturbed action supervened so promptly after the administration of doses of 1 centigramme and 2 centigrammes, we were led to consider these doses as too high for obtaining pure sedative effects.

THIRD EXPERIMENT ON OURSELF.

On the 12th July, 1841, the pulse having presented on the preceding days an average of 64 beats per minute, all the functions being performed in the normal manner, we applied, at 11, a.m., on the surface of a blister, placed on the previous evening on the inner part of the left arm, 1 centigramme of bitter principle; the same product as that used in the foregoing experiments. Immediately, slight smarting and sensation of heat which did not continue. 60 regular and full pulsations.

At noon, *redness*, painful numbness of the arm, slight cephalalgia, 64 pulsations.

At half-past 2, the head-ache continued; yawning; general muscular feebleness. 70 pulsations.

At 5 o'clock it had dissipated. Appetite for dinner.

At 7 o'clock, 72 regular pulsations; a motion and urine; no appreciable symptom.

At 9 o'clock, 64 pulsations; the surface of the blister appeared brown and rather violet, and as if slightly scarified. 1 centigramme of bitter matter was applied without producing any immediate smarting.

At 10 o'clock, 60 pulsations, but less regular, and occasionally a pulsation appeared to be wanting; cephalalgia, redness, muscular weakness. The night was good, and sleep undisturbed.

On the 13th July, at 7, a.m., 64 pulsations; the surface of the blister was covered with mucous pus, and 2 centigrammes of bitter principle were applied.

At 8 o'clock, from 64 to 68 pulsations, slight cephalalgia; transient redness.

At half-past 10, very general weakness.

At half-past 11, 64 pulsations; the weakness increased, the legs trembled and seemed to bend; the urine appeared more scanty; appetite.

At 1 o'clock, 66 regular pulsations; smarting of the blister.

At 2 o'clock, cephalalgia very great weakness, as at the end of a violent emotion or syncope.

At 5 o'clock, 57 pulsations; the motions of the arms were painful; the vesicated surface was swelled, violet, with an inflammatory aureola, of erysipelatous appearance of several centimetres in extent; good appetite.

At 6 o'clock, 66 regular, depressible pulsations.

At a quarter-past 8, 68 to 62, soft pulsa-

tions, very irregular as to strength and number; in every 30 pulsations one was completely wanting, and in every five, one was much weaker than the rest; tendency to shivering.

At 9 o'clock, 64 pulsations, still irregular; urine more scanty.

At 9 o'clock, 57 still irregular pulsations; the blister was rubbed without any bitter substance being applied to it; in bed, 60 pulsations were counted; the sleep was disturbed and often interrupted; an unusual and painful heat in the hands and feet.

On the 14th of July, at 8, p.m., 68 regular pulsations; 2 centigrammes of bitter substance were applied to the blistered surface; smarting, with pricking sensation.

At 11 o'clock, cephalalgia, dazzling of the eyes, slight dimness of sight; 60 pulsations; appetite.

At 2 o'clock muscular weakness, which walking somewhat diminished; 60 irregular pulsations; the arm was very painful.

At 6 o'clock, after dinner, fatigue, weakness, return of cephalalgia, and dazzling of the eyes.

At half-past 8 o'clock, 60 unequal, irregular intermittent pulsations; very great fatigue, lassitude, not accounted for by the day's occupations; urine more rare, motion difficult.

The pain and numbness increasing as well the extent of the inflammatory areola, induced us to suspend the application of the substance, and, twenty-four hours afterwards the pulse had returned to its ordinary number. The blister was closed on the fourth day.

The bitter principle employed by the endermic method determined in the doses of 1 and 2 centigrammes, inflammatory symptoms which forced us to conclude the experiment on the fourth day. The dose of 1 centigramme was always sufficient for producing poisonous symptoms, such as cephalalgia, dazzling of the eyes, dimness of sight, general weakness, shivering and yawning; sleep was somewhat modified; the appetite seemed rather to have increased than to have diminished, and the venal secretion appeared to be relaxed.

The influence exerted on the circulation is manifested by the inequality, irregularity, and intermittence of the pulse, without any considerable diminishing of frequency being observed, although the pulse fell momentarily to 57.

EXPERIMENT IV. ON OURSELF.

On the 28th of January, 1841, in perfect health, the pulse oscillating between 64 and 72, at 8 o'clock in the morning. I took internally 0 grammes 00·5 of the bitter principle (the same product as was used in the foregoing experiments); no appreciable effect.

At 2 o'clock, 5 milligrammes more.

At 5 o'clock, 58 pulsations; appetite; with slight sensation of faintness.

At 9 o'clock, 60 pulsations.

At 10 o'clock, 5 milligrammes more.

On the 29th July, 6 o'clock in the morning, 60 pulsations; 5 milligrammes more; cephalagia; dazzling of the eyes.

At noon, 57 pulsations.

At 2 o'clock 58 equal, full, regular pulsations; 5 milligrammes more.

The same sensation of frontal cephalalgia and of faintness in the epigastric region, dazzling of the eyes, and weakness of the legs.

At 5 o'clock, 58 pulsations; diminished appetite; immediately after dinner, 64 pulsations; in the evening, the pulse returned to 58.

At 10 o'clock, the same sensation of fatigue and of muscular weakness; 5 milligrammes more; sleep, good.

On the 30th of July, at 6 o'clock in the morning, 56 normal pulsations; 5 milligrammes more; sensation of faintness.

At ½ past 10 o'clock, 54 pulsations; nausea and dazzling of the eyes; 5 milligrammes more.

At 2 o'clock 52 pulsations; no motions; urine normal.

At 5 o'clock, 50 pulsations before dinner, 52 after; want of appetite; I again took 5 milligrammes.

Cephalalgia, dazzling of the eyes, impaired vision, nausea, gases developed in the intestines, slight abdominal pain.

At 7 o'clock, 56 pulsations; during the whole night, very great uneasiness, almost continual nausea, painful heat, fatigue, cessation of the medicine, want of sleep, abundant urine, no thirst.

On the 31st of July, at 6 o'clock, 54 pulsations; exercise raised the pulse to 66, but rest speedily reduced it to 54; very great uneasiness; limpid and abundant urine; and diarrhœal stool.

At 9 o'clock, 60 intermitted but regular pulsations; disorder of the stomach; intestinal gases, causing rumblings; gaseous eructation; nausea; yawning; dazzling of the eyes, resembling flashes of lightning; appetite returned at noon.

At 2 o'clock, better; still no thirst; lassitude and inclination to sleep.

At 8 o'clock in the evening, 60 intermittent pulsations; disorder of the stomach; sleep was frequently interrupted during the night.

On the 1st of August, at 7 o'clock in the morning, 60 irregular pulsations, difficult to count; after rising, the pulse increased to 72. Frontal cephalagia, yawning and disorder of the stomach; this state continued through the day; urine abundant and causing heat in passing; after dinner, better. At 7 o'clock, 54 pulsations; night good.

On the 2nd, on waking 60 regular pulsations; return of the normal state.

The following days, the pulse varied from 64 to 68.

In this experiment, the bitter principle of digitalis, taken during the three consecutive days, in the dose of 5 milligrammes, repeated five times each day, at intervals of 4 or 5 hours, produced appreciable effects on the circulation besides the poisonous symptoms above described. The diminution of the pulse was progressive, and the third day it fell to 50 pulsations. This diminution was followed by irregularity and intermission; then, after the cessation of the treatment, this diminution of the pulse remained two whole days, progressively rising to the natural number.

The urinary secretion was evidently influenced, but this influence consisted in a diminution during the experiment, followed by a considerable increase after its cessation.

Finally, it appears to us from this experiment necessary to employ still smaller doses.

A person, to whom the regimen was necessary from the state of his health, on the one hand, and, on the other hand, whose skill in experimenting enabled him to give these experiments all the care and precision desirable, was kind enough to try several series of experiments, of which we here give a summary.

The first experiment, in November, 1841, demonstrated that the combination of the bitter principle with tannine, produced poisonous phenomena analogous to those produced by the isolated bitter principle.

The numerous causes which may induce the pulse to vary in frequency, being of a nature to throw doubt on the effects which we were disposed to attribute to the bitter principle of digitalis on the circulation, this person began by studying its different influences, then he took, in order to avoid causes of error, the precautions which we shall point out.

The minimum of pulse frequently being attained in the horizontal position, it became necessary to adopt the position in counting the beats. Some time elapsed since the meal, and the nature of the latter might likewise cause considerable variations in this frequency; the diet adopted was of extreme regularity with respect to the hour at which it was taken, and the nature and quantity of the aliments. The pulse was counted at the same hours five time a-day, and after at least ten hours' repose in the horizontal position. Each series of experiments presented three phases. The first devoted to the determination of the number of beats of the pulse before the experiment.

The second appropriated to the number of beats of the pulse during the experiment.

The third devoted to the determination of the number of beats after the experiment.

The experiments were made with pure bitter principle, but not treated by ether.

FIRST SERIES OF EXPERIMENTS.

After 18 days' observation of the normal pulse, whose average was found to be 60 pulsations $\frac{6}{10}$ths per minute, the active principle was taken in pills of 2 milligrammes each, one at 11 o'clock in the morning, and one at 11 o'clock at night, for 8 days. On the 9th day, the dose having been raised to 6 milligrammes, there was, on the next morning, nausea, efforts at vomiting, colic, and a diarrhœal motion, rendering it necessary to suspend the use of the bitter principle. Nevertheless, these morbid phenomena did not disappear for eight days.

The general average of the number of pulsations during the 9 days of the administration of the medicinal agent, was found to be 52$\frac{3}{10}$ths, and the minimum observed on the 9th day was 42.

The following are the principal phenomena observed during the 9 days' experimenting:—

During the first day and most of the following ones, a sensation of general fullness and hard arterial pulsations. From the third day to the end *tiraillement* and general prostration.

On the fifth and sixth days it was considered that the urinary secretions were slightly increased.

During 7 days after desisting from the use of digitaline, the pulse was continually observed, and the average frequency was about 58, the minimum being 52.

It was scarcely possible to retain any doubt relative to the sedative effect of the bitter principle under consideration. A minute dose, 4 milligrammes, which could never be exceeded without causing symptoms of poisoning, produced a marked relaxation of the circulation.

These experiments suggested these questions, which, moreover, are in accordance with the spirit and the letter of the programme issued by the Société de Pharmacie.

Is the isolated principle with which we experimented in a physiological point of view, the only one to which digitalis owes its pro-

perties? Has the entire plant a more marked action on the circulation than our product? To solve these questions the same person made, in the same circumstances and with the same precautions, the following comparative experiments, substituting for our bitter principle powdered digitalis of good quality.

For seven days before the experiment, the average of the pulsations was 57¼.

Powdered digitalis, in pills of 10 centigrammes, was taken for 4 days in the dose of 10 centigrammes, morning and evening. On the fifth and sixth days three pills of 10 centigrammes were taken during the day. On the seventh and eighth days, the amount was raised to 4 doses at intervals of two or three hours.

The average of the pulsations during this period was 55¼. During the first four days the action on the pulse was scarcely perceptible, and there were no more marked phenomena; but on the fifth day, the dose being raised to 30 centigrammes, there was some nausea and colic; then, on the following days, the sensation of plenitude, twitchings in the stomach, and rumbling in the intestines, accompanied by a more marked diminution of the pulse.

The action on the urine did not appear more manifest than in the previous experiment.

The average of the pulsations during the six days after the cessation of the ditigalis was 52⅛, and much lower, as is evident, than before the experiment. We would first observe, that the dose of digitalis taken the first four days appear to have been insufficient, and then we thought, from this fact and some others in our possession, that when the dose of digitalis was not too high, its sedative action remained some time after the cessation of its employment; and if this effect was not observed in the experiments with pure digitaline, we were led to believe that it was owing to the dose of 6 milligrammes of pure matter, which was proportionally much higher than that of 40 centrigrammes of powdered digitalis.

We should also remark that the phenomena observed after the employment of the digitalis appeared more especially to affect the digestive organs, and that its tolerance appeared to us still less than that of the isolated bitter principle.

We yet regard, moreover, these two series of observations as of a nature to demonstrate that the active principle isolated by us possesses indeed a similar action to that of the plant from which we extracted it, and which makes us think that this principle possesses an action on the system at least a hundred times as great as that of the powder of the dry plant.

Some experiments tried since in morbid cases confirm the results already in our possession. Not to extend this part of our work too much, we will confine ourselves to giving a rapid summary of two or three.

In a very severe case in which pleurisy with pericarditis was complicated with anasarca, with scanty urine loaded with albumen and coloring matter of the blood. When the cough, difficult respiration, wiry pulse, tumultuous disorder of the beats of the heart, which could not be counted, seemed to cause death to be regarded as imminent, the administration of digitaline, in the dose of 2 milligrammes repeated three times until evening, was followed by the evacuation, in the course of the night, of three pints of limpid urine; at the same time, the difficult respiration ceased, and the pulse, which could not be counted on the previous evening, presented 120 regular pulsations. Four pills of 2 milligrammes of digitaline were given during the day. The urine continued in the same abundance, and next day the pulse gave 96 full, regular pulsations, with few intermissions. At the same time appetite returned, and the diarrhœa and thirst ceased. Continuation of the same treatment, the same results, and next day the pulse fell to 54 regular pulsations, but with intermissions, one pulsation being deficient in 15 or 16. The doses of digitaline were diminished, and at the end of 8 days the repugnance of the patient compelled us to suspend its employment. But the symptoms did not return, and the patient is now quite well.

In another case of simple pleuritic effusion, digitaline was given with success, and appeared to hasten the reabsorption of the effusion.

Finally, we will mention a case in which the employment of our bitter principle, prescribed for nervous palpitations, had a very marked diuretic action, since the patient, uninformed on this subject, first acquainted us with it; this diuretic action continued as long as the bitter principle was employed.

THE COMPOSITION OF BONE.*

A VERY lengthened series of experiments on this subject, by Dr. Stark, seems to afford somewhat unexpected results. That gentleman says—

"From these experiments we see how very nearly the same is the proportion of earthy

* *Edinburgh Medical and Surgical Journal.*

and of animal matters in the bones over the whole animal kingdom. They seem to demonstrate that wherever a true bone occurs, that bone contains the same average amount of earthy and of animal matters. We see that there is no difference in this respect between the true bones which surround the mouth of the sturgeon, and the bones of birds or of the mammalia. Yet the sturgeon is one of those fishes so low in the scale of creation, that its skeleton consists of simple cartilage. These experiments, therefore, demonstrate the fallacy of the statement, that the bones contain a larger amount of earthy matter the higher we ascend in the scale of organization."

CASE OF POISONING BY STRAMONIUM.*

Mr. Spence describes, in the *Boston Medical and Surgical Journal*, the cases of three females who had taken infusion of stramonium leaves (half an ounce to a pint of water) in mistake for horehound. He found them lying in bed, stupid, unable to articulate, with a certain peculiar wildness of countenance, and flushed face; pupils dilated and insensible; conjunctivæ highly injected; lips and tongue parched; no vomiting; breathing at times stertorous, and labored; hands cold, with a trembling and slightly convulsive movement; great rigidity of the muscles of the neck and back; at times active efforts at utterance. Stimulants were administered in two cases, with apparent benefit. The third case was fatal. The writer lays stress on the absence of vomiting, a symptom said by writers on materia medica to be always present.

ON THE MORTALITY IN PRISONS; WITH THE DISEASES MOST FREQUENTLY FATAL TO PRISONERS.†

BY WILLIAM BALY, M.D.

PHYSICIAN TO MILLBANK PRISON, AND LECTURER ON FORENSIC MEDICINE AT ST. BARTHOLOMEW'S HOSPITAL.

This paper contains the results of an inquiry into the rate of mortality, and the nature of the more prevalent diseases in the Millbank Penitentiary and other penal establishments during the last fifteen or twenty years.

The annual rate of mortality in the different prisons of England, calculated from the average number of prisoners and the number of deaths, exclusive of the deaths from Asiatic cholera, has ranged between 15¾ per thousand to nearly 39 per thousand; and in Switzerland from 25 to 35 per thousand. In France, the mortality, including deaths from Asiatic cholera, has ranged in the hulks from 39½ to 55½ per thousand; and in the *Maisons de Force et de Correction*, from 30½ to nearly 87 per thousand.

The annual rate of mortality amongst persons at liberty in the different countries and cities in which these countries are situated, and at the periods of life of prisoners, has varied very little from 15 per thousand. The excess of mortality has been much greater in some prisons than in others, but the amount of this excess of mortality is no measure of the degree in which the health of the prisoners is affected by the discipline, diet, and general arrangements to which they are subjected, since there are many circumstances, independent of all systems of prison discipline and internal prison arrangements, which greatly affect the number of deaths occurring in prisons. The most important of these circumstances are—First, The extent to which the practice is carried of granting pardons to convicts in failing health; second, the degree of predisposition to disease of the class of persons forming the population of the prison; third, the length of confinement which the prisoners undergo; and fourth, their liability to endemic and epidemic diseases, owing to the situation of the prison.

The high rate of mortality which prisoners suffer is really the effect of their punishment, and is not owing to the unhealthiness of the class whence criminals are, for the most part, derived. This is proved by the increased mortality which attends an increased duration of imprisonment; and by the result of comparing the rates of mortality in English prisons with the rate of mortality of the population of Liverpool, the most unhealthy town in England. The mortality of persons of the ages of fifteen to seventy, in Liverpool, during 1845, was 18 per thousand living. But the annual ratio of deaths amongst the prisoners in the county prisons of England has been nearly 23 per thousand; amongst those confined in the Millbank Penitentiary, at all periods of imprisonment, nearly 31 per thousand; and amongst those passing through the third year of their confinement in that institution, more than 52 per thousand.

In America, France, and Switzerland, as

* *Lancet*, April 26, 1845.

† Read before the Royal Medical and Chirurgical Society, Feb. 25, 1845.

well as in England, the proportion of deaths occurring annually has been much greater among criminals in prison than amongst persons of a corresponding class of society out of prison. The diseases to which this increased rate of mortality has been chiefly due in the Millbank Penitentiary, and all prisons wherein criminals are confined for long periods, are the various forms of tubercular scrofula, and especially tubercular phthisis.

No other class of diseases has produced uniformly in all prisons, a higher ratio of deaths than the same class of diseases causes among persons at liberty; whilst the mortality from many diseases is less in prisons than amongst the free population. Even where endemic diseases have been prevalent, owing to the unhealthy site of the prison, the great excess of mortality has still been caused by tubercular diseases.

The causes which have rendered tubercular disease so frequent and so fatal in prisons, are, in the author's opinion—First, deficient ventilation; second, cold; third, sedentary occupations and want of active bodily exercise; fourth, a listless, if not dejected, state of mind; and fifth, the poorness of the diet.

The diet in the Millbank Penitentiary and in the American prisons has been more abundant than that of the agricultural laborer. But in many other prisons the allowance of food has been very scanty.

In respect of the diet, and also of the ventilation and warming, a great improvement has been recently made in the prisons of England, and it is to be expected, after the lapse of a few years, that a great amelioration will be found to have taken place in the health of prisons.

Dr. Webster, without expressing his concurrence in all the opinions now promulgated, agreed with the author respecting the effects produced upon prisoners when only confined for a short period, who often left the prison in an improved state of health, compared with their previous condition. This fact had been generally observed in gaols, and he would, in confirmation of that opinion, refer to the results met with during the last two years at the City Bridewell, as he could speak on the subject from personal knowledge. The fellows, of course, knew that the persons committed to this prison were dissolute and depraved characters, who had been exposed to the inclemencies of the weather, and often to the want of food; nevertheless, most of them leave the prison in better condition than at their committal. The period of confinement varies from a few days to three months, the average length being thirty days. To prove that the health of the City Bridewell prisoners is not deteriorated by their confinement, he (Dr. W.) would remark, that during 1843, when the committals were upwards of 1,000, only sixteen infirmary cases occurred, and one death; whilst, in 1844, when the numbers committed amounted to nearly 1,150, there was only one death during the year, and twenty-four infirmary cases, most of them being trifling complaints. Properly speaking, even the single death recorded ought not to be ascribed to the prison discipline, since the patient only remained fourteen days in Bridewell, and was an old vagrant, who laboured under fever at his committal, and had suffered much from misery and destitution. Long confinement often injures the health of prisoners, but short periods frequently prove advantageous, as shown by Dr. Baly's observations. He (Dr. W.) also agreed with the author as to the frequency of phthisis and bowel complaints; indeed, at the Penitentiary, consumption seemed lately to have been the most fatal disease amongst the inmates. The parliamentary report for 1844 stated that of the eleven deaths met with in that prison during the previous year, seven were from phthisis; at the same time, amongst the fourteen criminals pardoned on account of impaired bodily health, the majority were affected with pectoral disease, seven being phthisis, and one pleurisy. Again, in 1832, epidemic dysentery was very prevalent in the Penitentiary, when nine deaths occurred from that disease. He made these remarks on purpose to ascertain whether the author coincided with him that the mode of ventilating and warming prisons usually employed, materially contributed to increase pectoral diseases amongst the inmates. The breathing of hot dry air irritates the mucous membrane of the lungs, produces cough, and, according to his (Dr. W.'s) opinion, in addition to the mental depression of prisoners, this dry and overheated atmosphere tended to bring on consumption, especially in those predisposed to that complaint. Besides local causes, the food consumed had, he thought, much influence in the production of bowel complaints, particularly the frequent use of liquid food, such as peas soup; perhaps Dr. Baly would state his views to the society on these points. Although the author, in the abstract of his paper, had only alluded to the bodily diseases affecting the Penitentiary inmates, there was another question, of equal importance, to which he (Dr. W.) would wish to direct attention, in order to learn from Dr. Baly how far the mental condition of the criminals was affected by the discipline and se-

clusion of the prison, when they were placed in solitary confinement. This was an important subject, and one which had recently occupied the attention of philanthropists and medical men in this country, as also in France and America. Dr. Baly might feel unwilling to reply to such questions, as he held a public appointment under government; but he (Dr. W.) being differently situated, considered it would not be out of place to mention, in a medical society like the present, a few facts illustrative of the influence which solitary confinement produced, from the records of the prison to which Dr. Baly is attached. In 1839, according to parliamentary reports, three insane patients were sent from the Penitentiary, to an asylum; five in 1840; or, taking the eighteen months prior to July, 1841, when the solitary system was strictly enforced, fifteen persons became insane; whereas, during the subsequent eighteen months—that is, when an important modification took place in the prison discipline—five insane criminals were removed, and only two during the year embraced in the report of 1844. This remarkable diminution in the number of Penitentiary inmates transferred to a lunatic asylum is conclusive on the subject, as it occurred subsequent to the period when the prohibition to intercourse was limited to three months after admission, the prisoners being then permitted to converse with two or three other prisoners during the hours of exercise, attention being, however, paid to the age, the disposition, and offences of the criminals thus allowed to associate together. Stronger evidence cannot be adduced respecting the effects which solitary confinement exerts upon the minds of ignorant or depraved individuals, than the facts just stated. He (Dr. W.) entertained strong opinions upon this question, and considered no punishment so severe to a living human being as long continued seclusion from the company of his fellow-creatures, especially to uneducated individuals, often devoid, not only of any good principles to fall back upon during their solitude, but who were, instead, frequently the slaves of their own evil passions.

Dr. Baly observed that his paper had reference simply to the effect of imprisonment on the body, and he therefore should refrain from answering Dr. Webster's question in reference to the effect of solitary confinement on the mind. In regard to the influence of heated air in the production of phthisis in the Penitentiary, cold appeared to have the most injurious influence on the prisoners. He could not trace the production of phthisis to hot air. Facts were, however, he believed, on record, to show that hot air was injurious, although, as yet, he was aware of no information from any prison which warranted a belief that it caused phthisis. With respect to diarrhœa and dysentery, he felt convinced, when these diseases were prevalent for any lengthened period, that they were not produced by the kind of food which had been taken, but owed their origin to some peculiarity of the locality in which the prison was situated. Liquid and poor diet, though they would not originate, nevertheless, aggravated these diseases. He had, in his paper, mentioned mental depression as one of the causes of bodily disease, and this depression, of course, would be increased by solitary confinement.

Dr. Webster did not refer the occurrence of phthisis to the influence of hot air, but it might produce other chest affections, as bronchitis, which would terminate in phthisis.

Dr. James Johnson referred to the curious fact stated in the paper, that the mortality in prisons was always lessened after the fourth year, and inquired if any cause could be assigned for this circumstance?

Dr. Baly observed that it was a fact that the mortality in the Penitentiary was less in the fifth than the fourth year. It had been noticed, also, in the Eastern Penitentiary of America, that this circumstance obtained: and in this institution, also, there was less mortality in the fourth than in the third year. He explained this by the fact that those prisoners who were liable to be affected with scrofula, fell a victim to that disease before the termination of the fourth year. Those who survived that period were not liable to be thus diseased.

Dr. Cursham inquired how long a time it required for scrofula to develop itself in those prisoners in whom there was no trace of the disease previous to their admission into the Penitentiary?

Dr. Baly replied that scrofula began to develop itself in the second six months' imprisonment, and increased in severity during the next eighteen months. He possessed only a limited number of facts in regard to this question, but it would appear that the disease gradually declined after the second year of its existence.

Mr. Bransby Cooper inquired what was meant by scrofula in the paper of Dr. Baly. The term was so indefinite, that he wished to know in what way it was employed.

Dr. Baly, in the abstract, had referred tuberculous scrofula to imprisonment. He considered this disease to consist of a deposit of tuberculous matter in the lymphatic glands, the serous membranes, or any organ of the body.

Mr. Bossy, in reference to the inquiry of Mr. B. Cooper, remarked, that Dr. Baly

had correctly described the scrofula developed by imprisonment as tubercular cachexy. It showed itself by paleness, weakness, and general debility of the frame; and these were so marked, that he (Mr. B.) could, in examining a number of men, easily determine which of them had been subjected to imprisonment. Dr. Webster had remarked, that short imprisonment appeared to have a beneficial influence upon health, and this was apparently the case, for from want of exercise, and the use of fluid diet, there would be an increase of fat. He had found, however, that the improvement in health was not real, for, upon putting these persons to labor, they were incapable of muscular exertion. There was, moreover, a loss of weight in them. There were the persons in whom, if imprisonment were continued, tubercular disease would develop itself. With respect to the prevalence of tubercular disease generally, the mortality from this cause in the hulks had been increased by a circumstance which should be mentioned. On the carrying of the Emancipation Act in 1834, the black convicts of the West Indies were sent to this country on their way to Van Diemen's Land. They arrived chiefly in the autumnal season, and phthisis developed itself in great numbers as the effect of confinement and cold. This accounted for the large mortality for some time, from phthisis, in the hulks. The cruelty of this plan was represented to the Government, and was not persevered in. In addition to this, also, it might be stated, that pardon was not granted to prisoners in the hulks on account of ill health. He differed with Dr. Baly respecting the influence of prison food in the production of diarrhœa, and gave two or three illustrations in point. In one of these he traced the prevalence of this disease to soup containing a quantity of barley-husks; and, in another instance, to the bread used by the prisoners having been made of flour, a quantity of which had been ground from wheat damaged by the sea, and which had began to vegetate. In both these instances the bowel complaint prevailed during the time the prisoners were partaking of the unwholesome diet, and disappeared on substituting more healthy food. He agreed with Dr. Webster that hot air produced catarrh, and chronic bronchitis, if not phthisis. In the Chelmsford prison, the prisoners were taken from a cell of high temperature to stand at the cold treadmill, and were again taken to the cell. Catarrh was frequently caused in consequence. This had also occurred in other prisons.

Dr. Webster expressed his satisfaction that Mr. Bossy's extensive experience respecting prisoners coincided with the opinions he had expressed relative to the influence which particular kinds of food produced on the health of the inmates of prisons. In addition to the facts Mr. B. had stated, he (Dr. W.) might mention, that in Bridewell, although situated in a confined locality of the city of London, near Fleet-ditch and Puddle-dock, bowel complaints, or any serious disease, had not been prevalent, notwithstanding the prisoners were not allowed to take regular out-door exercise. Generally speaking, the men were employed on the treadmill, and the women in picking coire, but their food was of good quality, and the bread excellent; indeed, it could not be purer, as the corn was ground in the prison, and baked at the House of Occupations, with yeast also made on the premises. He (Dr. W.) was likewise gratified to hear that Mr. Bossy entertained similar opinions with himself respecting the ventilation of prisons, and the injurious influence which the breathing of hot dry air had in producing pectoral disease amongst the inmates. Having stated, in the previous remarks he (Dr. W.) had taken the liberty of offering to the society, that a large proportion of the prisoners recently pardoned at the Penitentiary were discharged on account of being affected with phthisical symptoms, he would now ask Dr. Baly whether any of these patients had subsequently recovered; for, if such were the case, the fact would show that their residence in prison tended materially to produce consumption. With reference to the lunatics sent to Bethlehem Hospital from the Penitentiary, it must be satisfactory to Dr. B. to hear, that, during last year, four individuals formerly under his care in that prison had been cured, and removed from the hospital by warrants of the Secretary of State.

Dr. Baly replied, that in a great number of cases of phthisis at the Penitentiary, apparently hopeless, the disease was immediately checked on the release of the prisoners, many of whom entirely recovered. A favorable change took place almost immediately they were informed of their probable change. These cases showed, in a remarkable manner, the influence of mind on the progress of disease. Mr. Bossy had, in some degree, misunderstood him with reference to the causes of diarrhœa and dysentery, which he admitted might occasionally be produced by impure food. But when diarrhœa and dysentery were prevalent for several years, they did not then arise from diet, but were always dependant on the locality of the prison The causes mentioned by Mr. Bossy as productive of bowel complaints could not be always

prevalent. Facts which he (Dr. B.) had referred to in his paper proved that his view was correct, for not only were these diseases found to be more prevalent at particular seasons, as in spring and autumn, but when they were epidemic in the surrounding neighbourhood. In these instances the diseases were owing to miasma.

Dr. Gregory suggested that as Dr. Baly was about to furnish other papers on the subject of disease in prisons to the Society, much benefit would be derived, particularly in respect to age, if he directed his attention to the acquirement of the tubercular diathesis in a number of persons under circumstances directly the reverse to those of prisoners. He thought soldiers offered this opposite condition. He had been struck with the origin of phthisis in the recruits of several regiments, particularly the Foot Guards. It was a curious fact, that many of these men, even though carefully examined by the stethoscope, and pronounced healthy, at the end of a few months fell victims to tubercular disease. The announcement of the result of investigation into these cases in Dr. Baly's paper would increase its value and enhance our knowledge of the subject.

Dr. Baly was obliged for the hint thrown out by Dr. Gregory. He was well acquainted with the prevalence of consumption among the Foot Guards, who suffered from this disease to twice the extent, as did the cavalry regiments. He did not agree with Dr. Gregory as to their being placed under circumstances the reverse of prisoners; on the contrary, they were subjected to several of the same injurious influences. Nothing, for instance, could be worse than the ventilation of the barracks, particularly those in Portman-street and in the Tower. There was no ventilation whatever in the rooms they slept in, and these were their sitting-rooms also. They mounted guard frequently, and were therefore frequently exposed to cold, as were prisoners. There was another cause which also acted on both these classes, and this was, the listless state of mind which resulted from their condition, and the inactive state of their bodies, both having a tendency to the production of tubercular disease. To these may be added, perhaps, their dissolute habits.

Mr. Cooper regarded the depressed condition of mind of the recruits, conjoined with their severe exercise in drilling, as a further cause of phthisis in these men.

Mr. Propert considered that the well-known indulgence of recruits—who were generally fresh from agricultural pursuits—in drinking, and their contraction of venereal disease, must have an influence in the destruction of their health.

Mr. Macilwain considered Dr. Baly's paper important in reference to the causation of disease and its amelioration. He inquired whether in those prisons in which diarrhœa was prevalent, as the result of locality, the peculiarity was in the soil, or in the ordinary matters of drainage. He made some remarks on the influence of ventilation on the function of the lungs.

Dr. Baly regarded the diarrhœa as the effect, not of imperfect drainage, but of a moist state of ground for some extent round the prison. The Wakefield House of Correction, in which diarrhœa was more generally prevalent than in any other prison of England, was situated in a hollow valley of clay, which was often flooded in winter, and in summer was not quite dry. The subsoil was clayey, the surface of a rich vegetable character.

THE EPILEPTIC PATIENTS IN PARIS.*

We present to our readers the following history of the epileptic patients in Paris, to which allusion has been so often made in the course of Tawell's case.

In the year 1830, hydrocyanic syrup was prescribed to seven epileptic patients, (in one of the hospitals of Paris.) This syrup, which the physician intended to contain $\frac{1}{180}$th part of hydrocyanic acid, according to Magendie's formula, was prepared, at the Pharmacie Centrale, with nine parts of sugar syrup and one part of medicinal acid, in conformity with the formula of the ancient codex. Every one of the seven patients, having taken 11 grammes and 50 centigrammes of this syrup, accordingly swallowed 1 gramme and 15 centigrammes of medicinal acid. This dose is enormous, and such as the strongest constitution could not resist. The formula of the codex was evidently monstrous, and it became a matter of urgent necessity to replace it by that which the majority of medical practitioners at Paris were in the habit of using, and which admitted only $\frac{1}{180}$th part of prussic acid into the composition of hydrocyanic syrup. This substitution has been effected since, and there is no fear now that such deplorable mistakes will ever happen again.

Seven minutes after the administration of the hydrocyanic syrup, the seven patients were found stretched on their beds in a state of perfect unconsciousness; all of them had been attacked by convulsions. The respiration was loud and hurried, the mouth foamed,

* *Lancet*, April 12, 1845.

the body was covered with sweat, the pulse was frequent. This general excitement was soon succeeded by relaxation and sinking, which increased rapidly, though gradually, and finally terminated in death. The respiratory movements decreased in frequency and extent; the pulse became every minute slower and weaker; the sweat and the extremities became cold, and death supervened. In some of the patients, the face and the teguments of the skull were strongly ingested, whilst in others they were very pale; the pupils were generally rather dilated. It does not appear that there was any vomiting. One of the patients only made violent efforts to vomit, shortly before death. It was intended to give a very hot foot-bath to the patients, but the majority of them expired before the application of this remedy. One of them, however, lived longer than the rest; his legs were immersed in hot water; soon after he was suddenly seized with very violent general convulsions, under the influence of which he threw his legs out of the bath by an extremely sudden movement. He evidently felt the heat of the water, since the expression of his face, one moment before the invasion of the convulsions, denoted great and acute pain, whilst acceleration of the respiration was observed at the same time. The face, the conjunctivæ, and the whole head, were most strongly ingested. A vein was opened largely; black and very fluid blood flowed from it; the flowing ceased as soon as the walls of the vein collapsed, (upon loosening the ligature.) The patient expired whilst efforts were being made to restore the flowing of the blood.

The first of the seven patients died after the lapse of from fifteen to twenty minutes; the last, after the lapse of forty-five minutes. MM. Adelon, Marc, and Marjolin dissected the bodies, and found the following pathological alterations in every one of them, varying simply in distinctness and intensity:—Manifest inflammation of the mucous membrane of the stomach and of the small intestine, with a remarkable development of the mucous crypts of this membrane; slight ingestion of the subperitoneal cellular tissue of the stomach and small intestine; the spleen softened, and in some instances reduced to a pappy tissue; the veins of the liver filled with a pretty large quantity of black and fluid bood; the veins of a dark-violet color, slightly softened, gorged with blood, and readily admitting of the removal of their external membrane. The heart of a pretty firm tissue, and quite empty; the large arteries empty, the large veins, on the contrary, gorged with very fluid blood, of black color; the blood fluid in every part of the body where it was found, and without showing the slightest traces of coagulation; the mucous membrane of the larynx, trachea, and bronchial tubes, of a dark red, which did not disappear upon washing the parts; the bronchial tubes filled with a frothy and sanguinolent liquid; the membranes of the brain ingested; the sinus of the dura mater gorged with a pretty considerable quantity of black and fluid blood; the tissue of the brain rather softer than in the natural state; in every other respect healthy, as well as likewise the spinal cord; the mucous membrane of the bladder white; that of the pharynx and œsophagus likewise white. No part exhaled the peculiar odor of prussic acid; no signs of putrefaction were observed; all the bodies were stiff.

ERGOT OF RYE.*

Dr. Crowne considered that the effects of the ergot were exceedingly uncertain, but there could be no doubt that in many cases its effects were very marked. He stated, that to rupture the membranes was occasionally a very difficult task, and he would therefore recommend a trial of the ergot previous to the attempt; he considered the effects of this remedy to cease immediately after the expulsion of the child; he had found that where small doses had no effect, larger were of no service. He (Dr. C.) was satisfied that it exerted no poisonous influence upon the child, but that the incessant uterine action caused such continued pressure upon the placenta and funis as to cut off the circulation and so destroy the fœtus; he had frequently administered this remedy in amenorrhœa without any effect; he administered it in half-drachm doses, infused in hot water, and its effects were usually manifested in fifteen or twenty minutes; he further stated, that sloughing had been imputed to the use of this remedy.

The President had given two ounces of ergot without any uterine action being produced, consequently he ruptured the membranes; the child was born alive.

A member had seen after-pains very severe in cases where the ergot had been given.

Mr. Dunn had employed this remedy in numerous cases of labour where no physical obstruction to the passage of the head existed; he considered that it lessened suffering, but was occasionally injurious to the child; had found after-pains more severe subsequent to its use. Was in the habit of

* Westminster Medical Society, February 15, 1845.

giving it in half-drachm doses, in boiling water, to which he added one drachm of brandy.

Mr. Chowne did not think the ergot necessary in the practice of midwifery.

Mr. Harding believed it had no specific effect on the gravid uterus at the full period, and was quite convinced it was powerless in the early months.

Mr. Curtis considered that ergot had no specific effect on the uterus during parturition, and therefore did not employ it. It appeared to act beneficially on hæmorrhages of a passive kind. In uterine hæmorrhages after parturition he had found it of service. He was in the habit of using a tincture of the ergot.

TO DESTROY RATS.*

I FREQUENTLY observe inquiries from your correspondents as to the best means of destroying these pernicious and destructive vermin. Let them try the following nostrum—the secret of one who professed the art, and was eminently distinguished, and supported too, until, at length, in his old age, he conceived and executed the happy idea of publishing his renowned secret, by subscription, in the form of a sealed pamphlet. This publication, of some sixteen pages octavo, was sold extensively for one guinea and a half per copy, and, to the best of my remembrance, it contained but very few recipes of any value, except the one I subjoin. I have mislaid the pamphlet, and therefore quote the chief receipt from memory; but have no doubt of its efficacy, from having seen it tried repeatedly. The poor man has, undoubtedly, been "gathered to his fathers" long since; his important secret can, therefore, no longer concern him in any way. The book, if I remember rightly, was published by Seeley, some 25 years ago. 1 quart of the best oatmeal; 2 ozs. of finely-powdered loaf-sugar; 6 drops of oil of rhodium; 6 drops of oil of caraway; 6 drops of oil of aniseed; musk, ¼ of a grain. Mix well together on a sheet of clean paper, with a knife, taking every possible care to break the small lumps formed by the drops of oil in the oatmeal; also not to touch the mixture with the hands. Keep it in a closely covered pot for use. Choose a very quiet, retired place, much frequented by rats; place on six or eight pieces of clean board, about two table-spoonsful of the feed, for at least four nights successively, taking care to remove all that the rats have not consumed every morning; in so doing, disturb the rats as little as possible, and on no account whatever attempt to strike or kill them, even should they be found tame enough to be caught by the hand. In the course of four or five days, they will become tame, and will scarcely leave the feed on the approach of any person. They will scent each other, and will probably congregate from all parts of the neighborhood. Continue to feed them nightly, until they are found to eat freely; then, about the sixth night, mix a small teaspoonful of arsenic with the powder; and after leaving it all night, remove what may remain, and burn it. The publication stated that a rat has no gall, for which reason the smallest quantity of the poison, if taken by them, will be found to insure their destruction. They will retreat to their haunts, where, feeling an intolerable thirst, most of them will drink water and die. Few will remain to tell the tale, if these directions are attended to; but care must be taken not to use too much arsenic, on account of its extreme acidity, or their natural instinct will be sure to deter them from swallowing any part of the poison.

* *Agricultural Gazette.*

SECRETION AND PROPERTIES OF BILE.*

A SERIES of experiments by Schwann has led to the distinct conclusion of the bile being indispensable to life. They consisted in removing a portion of the common bile-duct, and establishing an external fistulous opening into the gall-bladder, so that the bile might be naturally secreted, but be discharged externally, and not permitted to enter the intestine. Their general result was, that of eighteen dogs thus operated on, ten died of the immediate consequences of the operation (by peritonitis and other affections, aggravated, probably, by the want of bile); and of the remaining eight, two recovered, and six died. In the six which died, death was the result of nothing but the removal of the bile; after the third day, they daily lost weight, and had all the signs of inanition—*e. g.*, emaciation, muscular debility, uncertain gait, falling of the hair. They lived from seven to sixty-four days after the operation; and the inanition was the greater the longer they survived. Young dogs appeared to die rather sooner than old ones. Licking the bile as it flowed from the fistula, and swallowing it, had no influence on the consequences of the operation. In the two dogs that recovered, the importance of the bile was equally well shown; for in these it

* *British and Foreign Review.*

was found, when they were killed, that the passage for the bile into the intestine had been restored, and the period of its restoration was distinctly marked by their weight (which had previously been regularly decreasing) being augmented, and continuing to increase till it amounted to what it was before the operation; and also by the fistulous opening into the gall-bladder healing, and the discharge of bile ceasing.

EFFECTS OF TOBACCO.*

THE following is extracted from the proceedings at a late meeting of the Medical Society of London:—

Dr. Clutterbuck had observed that workmen in tobacco were not injured in the manner mentioned by the last speaker, although tobacco was of so irritating a nature. He thought this remarkable, and inquired the reason.

Dr. Alison confirmed from observation the remarks of Dr. Clutterbuck.

Dr. Willshire replied, that tobacco acted on the nervous system, and did not irritate mechanically. This he thought a sufficient answer to the question of Dr. Clutterbuck.

Mr. Pilcher detailed the particulars of the case of a medical student whom he had been requested to see. This gentleman suffered under all the symptoms of phthisis. There was mucopurulent expectoration, night sweats, &c. The mucous membrane of the throat, epiglottis, and the neighboring parts, was coated with a brown fur. The patient had been an immoderate snuff-taker: he was told to discontinue the snuff; he did so, and recovered. Mr. Pilcher detailed also a similar case in which the discontinuance of the noxious habit was followed by recovery.

THE HYDRATED PEROXIDE OF IRON IN POISONING BY ARSENIC.†

BY W. ALLISON, ESQ. EAST RETFORD.

ABOUT half-past five, p.m., Dec. 2nd, 1844, I was desired to visit M. J——, aged thirteen, residing three miles off, a girl of violent temper, who, having been severely reproved by her father on account of misconduct, had swallowed arsenic. Before four o'clock, a large teaspoonful, and before five o'clock, about the same quantity, was taken, each dose mixed in water; and she had suffered violent spasmodic pain, according to the messenger's (her brother's) account, "writhing," "twisting about," "moaning," and making strange faces." On arriving at the house with a stomach-pump, castor oil, magnesia, and hydrated peroxide of iron, she was found vomiting, and more free from pain, but complaining of a sensation of burning heat in the throat and stomach, with a feeble pulse and a cold skin. After a little resistance on her part, and having both coaxed her to swallow a heaped tablespoonful of the iron mixed in water, and threatened to overpower her and to use the stomach-pump, the iron was swallowed. About ten minutes afterwards, when thin gruel containing the magnesia was just cool enough to inject into the stomach, effectual vomiting occurred. A second quantity of iron was swallowed, and on my going up stairs a second time with the liquid for injecting, free vomiting again led to the administration of a similar dose of iron: about ten drachms had then been swallowed. After waiting half an hour, the girl drank an ounce of castor oil, which soon returned with a part of the iron.

Dec. 3rd.— Several motions, evidently containing the preparation of iron.

4th.— No motion during the night; pulse more natural; pains slight. The opening mixture was repeated.

10th.—Recovering; aperients necessary, and she begged for pills. Blue pill, and compound extract of colocynth.

A question naturally arises—"Did the girl actually swallow the specified quantity of arsenic?"

No one saw her drink it; but the parents told me that they had no doubt about it. the father said her temper would lead her to any length. The mother found a mug with some milky liquid at the bottom, in which she had no doubt the girl had mixed arsenic, from a packet labelled, "Arsenic, Poison," which had been procured for the purpose of mixing amongst seed-wheat.

The arsenic had been taken from a hole made through the paper, and was not to be found. The girl did not say what she had done, until she confessed to a favorite brother at the time when she was in violent pain, at half-past four o'clock. The girl was but thirteen, and was unlikely to know the effects of arsenic, which she described,—burning pain in the throat and stomach; the pain at times much more violent, &c. To-day (Jan. 28, 1845) I met with her, and in conversation begged, for my own instruction, she would tell me whether or not she really did swallow arsenic, and what quantity, as I wanted to know how much might be swallowed without producing death; to

* *Lancet.* † *Ibid.*

which she unhesitatingly replied, "To the best of my knowledge, I really did swallow, at twice, altogether, more than half a tablespoonful, expecting it would very soon kill me." I once related a case in which a girl was seen drinking thirty grains of the red nitric oxide of mercury, from which she recovered, as I thought, from the use of the stomach-pump.

The evidence and symptoms are tolerably conclusive that arsenic (half a tablespoonful) was swallowed. The stomach-pump was indirectly useful, for the sight of it produced vomiting more than once.

As one large teaspoonful was retained in the stomach more than two hours, it is probable that the iron had some chemical effect upon the arsenic whilst passing through the alimentary canal.

SPECIFIC GRAVITY OF URINE.*

ON A VERY READY AND EASY MANNER OF ESTIMATING THE QUANTITY OF SOLID MATTER HELD IN SOLUTION IN ANY GIVEN QUANTITY OF URINE OF A GIVEN SPECIFIC GRAVITY (AT 60° FAHR.)

At a time when the morbid conditions of the urine are attracting much attention, and when it is believed that important inferences may be deduced from ascertaining the amount of solid matter excreted from the kidneys in a given time, as compared with the amount so discharged in health, it seems a matter of some importance to call the attention of the profession to a very simple and accurate method of arriving at this result. My purpose, in this letter, is to call attention to the Table constructed by Dr. Henry, in which he gives the solid contents of a wine pint of urine, of specific gravity varying from 1,020 to 1,050—a table, I may remark, which has received the sanction of Dr. Prout, since he copies it at page 27 of his work on the *Nature and Treatment of Stomach and Urinary Diseases;* and also of Dr. G. O. Rees, who not only quotes it at page 147 of his book on the *Analysis of the Blood and Urine*, but adds, that he "has had occasion to test its correctness, and can recommend it as calculated to afford most satisfactory results."

Now, if this table be examined, it will be found that there is an increase of solid contents in each wine pint of urine, corresponding to the increase of one degree of specific gravity of exactly 19·2 grains; or of 1·2 grains of solid matter in each ounce for every increase of one degree of specific gravity. Hence a very simple process will give us the amount of solid contents in urine of any given specific gravity, say of 1,025; for we have only to multiply the constant quantity 1·2 grains by 25 (making 30 grains), to give us the solid contents of one ounce, whose specific gravity is 1,025; and this again by the number of ounces voided in any given time, say 40 ounces (making 1,200 grains), to give us the whole quantity of solid matter dissolved in the urine passed in that time. This rule will be found on trial to give results so nearly coinciding with those of the table (the error is constantly just 1·6 grain in excess in the wine pint), it is so easy both to recollect and to apply, and so much more convenient than reference to a table not always at hand, that I shall make no apology for trespassing on your pages, with the view of bringing it under the notice of your readers.

I remain, Sir,
Your obedient servant,
ALGERNON FRAMPTON, M.D.
Physician to the London Hospital

* *From the Medical Gazette.*

PHOTOGENIC MANIPULATION. By GEORGE THOMAS FISHER. Parts 3 and 4. London: George Knight and Sons.

The author describes, in the little work before us, the various modes of practising the art of Photography, including Calotype, Chrysotype, Cyanotype, Chromatype, Energiatype, Anthotype, and Amphitype. The explanations are simple and well expressed. We recommend our readers to avail themselves of the useful information contained in the pages of this work.

TO CORRESPONDENTS.

"S. W."—The recent convictions of chemists and druggists for selling spirits of wine for other than *medical* purposes has not escaped our attention: the particular case referred to was one of undeniable hardship, and the course pursued to ensure conviction anything but creditable to the parties concerned.

NOTA BENE.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of the Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th; Advertisements on or before the 27th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE DOUBLE SALTS OF TARTARIC AND RACEMIC ACIDS WITH POTASSA AND SODA, ATTENDED WITH SOME REMARKS ON THE CHARACTERS OF MONO AND POLYBASIC ACIDS.*

BY DR. R. FRESENIUS.

In my treatise on the composition and behavior of the racemates,† I stated that I had not succeeded in producing a double racemate of soda and potassa. By this fact, and the circumstance that racemic acid is unable to form either acid salts with bases of the baryta series, or double salts with the same bases and alkalies, I was induced to assume that racemic acid is monobasic, and that this property constitutes an essential difference between this and tartaric acid, the latter being bibasic.

Since then Mitscherlich obtained‡ a double racemate of soda and potassa, and this chemist,§ as well as Löwig,‖ believes that racemic acid, like tartaric acid, must be considered as bibasic.

Having been supplied, by the kindness of Professor Liebig, with a fresh quantity of racemic acid, which greatly exceeded that I possessed on the occasion of my preceding labor, I was enabled to repeat on a larger scale my former experiments relative to the salt in question, and to correct them.

In the following are communicated, first, my new experiments, and then questions put, which, when answered, will indicate the mono or bibasic constitution of the racemic acid.

There are three views prevailing, varying with reference to the amount of water contained in the double tartrate of soda and potassa:* to settle the question I have undertaken a minute investigation of this salt also, and the results are added to the present treatise.

I. RACEMATE OF SODA AND POTASSA.

Mitscherlich† states, that this salt can only be obtained from a solution, in which an excess of potassa salt is found. This assertion has not been confirmed by my experiments.

a. Of two solutions in water of racemic acid I saturated exactly the one with carbonate of soda, and the other with carbonate of potassa, and after having mixed the two liquids, and exposed them to the heat of the sun, I allowed them to evaporate.

b. The acid racemate of potassa was heated with water to ebullition, then neutralised by additions of carbonate of soda, the solution evaporated, and allowed gradually to cool.

c. A solution prepared in the manner of *b*, was committed to spontaneous evaporation.

d. To a solution, prepared in the manner of *a*, an excess of a neutral potassa salt was added, and then committed to spontaneous evaporation.

e. Contained a solution prepared like *a*, with an addition of an excess of a neutral soda salt. It was exposed to the air, and allowed to evaporate.

f. Similar to the foreging, with an addition of carbonate of soda. It was crystallised by evaporation and refrigeration.

In all the above cases I obtained distinct and regular crystals of the double salt. These manifoldly modified experiments I instituted chiefly with the view to ascertain why, in my former experiments instituted as in *b*, I had not obtained the double salt. I, however, have not succeeded in settling this point, for I obtained the double salt also with a little racemic acid left from my former

* Liebig's *Annalen der Chemie und Pharmacie*, February, 1845.

† *Ibid.*, vol. xli.

‡ Poggendorff's *Annal.* lvii., p. 484.

§ Mitscherlich's *Lehrbuch der Chemie*, 4th edit. ii., p. 157.

‖ Löwig's *Repertorium der Org. Chemie*, 1842.

* Berz's *Jahrb.* xxxiii., p. 327.

† Mitscherlich's *Lehrb. der Chem.*, 4th edit., vol. ii., p. 159.

labor, employing it in two experiments instituted like *a* and *b*.

To obviate any doubts, whether the freshly-prepared acid, which exhibited the reactions of racemic acid, was really such, I determined its amount of water at 212°F.

1·3322 grammes dried over sulphuric acid, lost at 212°F. 0·1386 water, which is, in 100 parts:

	Calculated.	Found.
4 C = 300·0		
2 H = 25·0		
5 O = 500·0		
HO = 112·5		
Ag. = 112·5	10·46	10·40
1050·0		

The double racemate of soda and potassa offers considerable, hard, colorless, transparent, rhombic tables or columns of the one-one membrous system. According to the determination of Professor Müller they are commonly formed by the predominant terminal surface *c*, and the octahedral surfaces *o* and *o'*. The edge between *o* and *o'*, as well as the acute (?) edge between *c* and *o'*, are truncated, the first by the surface *a*. The crystals do not effloresce on exposure to air, at a mean temperature; in the summer heat, however, slightly on the surface. They are easily soluble in water, one part of the anhydrous salt requires, at 43°F., 2·11 parts, or one part of the crystallised salt requires 1·32 parts of water (8·7535 solution left 2·8105 grammes salt dried at 356°F.). In hot water it is dissolved in any proportion. It may be repeatedly recrystallised without undergoing decomposition. At from 194° to 212°F. thec rystals fused, offering a tenacious limpid liquid, which between 248° and 302°F., under a vivid boiling, gradually become turbid, and are afterwards converted into a white solid mass; this latter state remains unchanged until the heat is raised to 392°F., when it becomes brown, and on gently calcining it exhales the known odor of burned sugar, and being strongly swollen it is converted into a carboniferous double carbonate of soda and potassa. The salt dried at from 356° to 374°F. attracts humidity from the air, after being dissolved it continues to exhibit all the reactions of a racemic salt.

In the following analyses air-dried crystals were employed, the powder of which was pressed between paper.

I. 2·8475 grammes (of the salt prepared according to *b*) at 365°F., lost 0·7140 water.

II. 1·3666 grammes (prepared according to *c*) at 365°F., lost 0·3485 water.

III. 1·6561 grammes (prepared according to *d*) at 365°F., lost 0·4225 water; gave 0·7805 chloride of sodium + chloride of potassium, gave 1·4279 chloride of potassium and platinum.

IV. 0·8547 grammes (of the remainder left from the racemic acid employed in the first labor and prepared according to *b*) lost 0·2157 water.

Hence the following composition results:

	Calculated.	In 100 parts; Found.			
		I.	II.	III.	IV.
$2\dot{R}$ = 1650·00	46·73				
NaO = 390·89	11·07			11·07	
KO = 589·91	16·71			16·66	
8Ag = 900·00	25·49	25·07	25·50	25·51	25·24
3530·80	100·00				

On heating the double racemate of soda and potassa at 212°F., for whatever length of time, the 25·49 per cent. water cannot be completely driven off. In one experiment after having applied heat by a water-bath for seven hours only 22·41 per cent. water were removed; that this circumstance, however, is owing to the fused state of the mass and not to a more intimate combination with the last atoms of water is immediately seen, if the saline powder is put into a water-bath together with an excess of pure fine sand, when after boiling for two hours the whole water is expelled.

On recrystallising one portion of the double racemate of soda and potassa, at a temperature less than 45°F., I obtained once the salt with 6 equivalents of water.

The crystals obtained in this way, likewise belonged to the one-and-one membrous system, and by their appearance and behavior, could not be distinguished from those described before.

1·7328 grammes of the salt treated as above, lost at 356°F., 0·3437 water.

1·0087 grammes gave on combustion with chromate of lead, 0·3540 water, 0·6692 carbonic acid.

Hence the following composition results:

	In 100 parts. Calculated.	Found.
8 C = 600·00	18·15	18·09
10 H = 125·00	3·77	3·82
16 O = 1600·00		
NaO = 390·89		
KO = 589·91		
3105·80		

Or the formula—

	In 100 parts. Calculated.	Found.
$2\dot{R}$ = 1650·00	49·92	
NaO = 390·89	11·83	
KO = 589·91	17·84	
6Ag = 675·00	20·41	19·83
3305·80		

II. DOUBLE TARTRATE OF SODA AND POTASSA.

The crystals of this double tartrate belong to the system 1-and-1 axis. As I will show in the sequel, that its amount of water is 8 equivalents, and thus in this point equal to the above described double racemate, it appears from this instance, what has been shown in many other cases by de la Provostaye,* that the combinations of the racemic and tartaric acids at an equal amount of water, are not isomorphous.

The anhydrous double racemate of potassa and soda, dissolves at 43°F., in 2·62 parts of of water (12·6804 solution at from 410° to 420°F., left 3·4994 dry residue,) it is therefore somewhat less soluble than the corresponding double racemate.

The crystallised tartarus natronatus requires 1·70 parts. By heating the crystals at from 158° to 176°F., they fuse, offering a clear limpid liquid, which boils at 248°F. At from 338° to 356°F., the mass becomes tenacious, and the vesicles enlarged, after some time it flows clear and tranquil. At from 374° to 383°F, boiling again, which ceases only at from 410° to 428°F. The mass then obtained is tenacious and clear, which after cooling, still remains clear and attracts humidity from the air. At 428°F., it becomes brown, and on gently calcining, under the same phenomena, as the double racemate it passes over into the double carbonate of potassa and soda. Besides other characters, the circumstance that the double tartrate after the water is driven off, exhibits a clear, limpid mass, whereas the racemate then appears to be white and turbid, makes it easy to distinguish the two salts.

With reference to the composition of the crystallised tartrate of soda and potassa, it was formerly believed that it contained 10 equivalents = 29·7 per cent. water.* This assertion was founded on an analysis made by Schulze, which according to our modern notions is highly inaccurate.† In more recent times the amount of water in question, was determined by Mitscherlich and Schaffgotsch, to be 8 equivalents or 25·49 per cent.‡ and finally Dumas and Piria§ estimated it to 7 equivalents or 23·03 per cent.

My experiments perfectly confirm the results obtained by Mitscherlich and Schaffgotsch, and accounts for the circumstance by which Dumas and Piria were led into a mistake. The experiments were instituted with well formed crystals of the perfectly pure double salt, which after being completely air dried, were rubbed into powder, then repeatedly pressed between filtering paper.

I. 1·2729 grammes on being burned with chromate of lead, gave 0·4959 water and 0·7953 carbonic acid.

II. 0·9400 grammes treated in the same manner, gave 0·3682 water and 0·5883 carbonic acid.

III. 1·3523 grammes lost at 420°F., 0·3419 water, without being in any way decomposed.

IV. 1·2797 grammes at the same temperature, 0·3185.

V. 4·154 grammes on being carbonised, treated with diluted hydrochloric acid, evaporated and calcined, gave 0·5447 chloride of sodium + chloride of potassium.

From this the following composition results:

	In 100 parts. Calculated.	Found. I.	II.
8 C = 600·00	16·99	17·04	17·07
12 H = 150·00	4·28	4·32	4·35
18 O = 1800·00	50·95		
NaO = 390·89	11·07		
KO = 589·91	16·71		
3530·80	100·00		

Or the formula:—In 100 parts.

	Calculated.	Found. III.	IV.
$\dot{T}$ = 1650·00	46·73		
NaO = 390·89	11·07		
KO = 589·91	16·71		
8Ag = 900·00	25·49	25·28	24·89
3530·80			

* *Annal. de Chim. et de Phys.*, iii., 129.

* Compare Berzelius, *Lehrb. der Chemie.* Geiger's *Handbuch der Pharmacie.* Döbereiner, *Pharm. Chemie.* Dumas, *Traité de Chim., &c.*

† Gmelin, *Handb. der Theoret. Chemie.*, 1822, vol. ii., p. 941.

‡ Berzelius, *Jahresb.*, xxiii., p. 327.

§ *Annal. de Chim. et de Phys.*, v., 353.

From analysis V, it appears that the atomic weight is = 3529·5.

Dumas and Piria, on combustion, obtained almost exactly the same numbers as the aforesaid, viz., 17·1 per cent. C, and 4·3 per cent. H, on drying 23·0 per cent. water. The formula T, NaO, K O + 7 Ag. requires 17·55 C, and 4·02 H. The numbers found agree, therefore, better with the above than with the latter, though the carbon in the analysis of Dumas and Piria is by all means a little too high, which, for the rest in combinations, so rich in water, may be accounted for by the circumstance, that the carbonic acid was not completely dry. By the fact that the said chemists, on drying, found only 23 per cent. water, it appears that they applied a heat of 356°F.; because, on being heated to that degree, the double salt loses only 7 equivalents water = 23·03 per cent., as will appear from the following experiments:—

1·3523 grammes of air-dried salt at from 356° to 365°F. 0·3101 water = 22·93 per cent.

1·2797 grammes at from 365° to 374°F. lost 0·3018 water = 23·58 per cent.

It appears from these, that the last equivalent of water is more pertinaciously retained than the others, and any doubt relative to this fact is removed by *intimately* mixing the double salt with sand, and heating it a long time in a water-bath, by which 7 equivalents water are driven off, where the last remains combined.

It is now to be discussed whether the racemic acid is more correctly to be considered as a mono than a bibasic acid. This question depends on the decision given upon another, namely, that by which characters of the mono or polybasic nature of an acid is to be distinguished.

I will endeavor to show in the sequel that, according to the latest experience, it is indispensably necessary, and demanded by the genius of the age, to come to a general understanding relative to that matter.

Liebig, by whom the above division of the organic acids was made, describes the character of the monobasic acid in the following terms:—*

"The acid salts of the monobasic acids, if put together with soluble bases, are resolved in two or more neutral salts, which may be separated by crystallisation, in case they possess an unequal degree of solubility, and are not isomorphous."

As the practical character of a bibasic acid, Liebig quotes in the same place:—

"The acid salts of the bibasic acids contains only one atom acid, whence it comes that no separation takes place, if they are neutralised with other soluble bases."

As three basic acids, Liebig designates those in which it may be assumed that three equivalents of water enter as hydrate, each of which may be displaced by one equivalent of any basis employed.

To these definitions is added the circumstances of falling into powder at a higher temperature; it appears, however, from the above words that these characters are of a less general application.

From Liebig's original treatise,* and labors undertaken by others with the same mind, it, moreover, appears, that double salts of an acid with a basis of the magnesian series, and an alkali may be found with a monobasic as well as with a polybasic acid, as they originate by displacing the hydrating water; and, finally, that acid salts of bases of the baryta or magnesian series, and the double salts of bases of the baryta series with alkalies, induce to consider the acid as of a bi or polybasic nature.

In addition to these characters, Hagen,† pointed out the circumstance, that one atom of a salt cannot be combined with half atoms of water, and considers this as a character of a bibasic acid. Fehling considers the succinic-hyposulphurous acid as tetrabasic,‡ and the succinic acid as tribasic,§ the first, because the formation of the succinic hyposulphurous acid may be best accounted for by this assertion; the latter, because the succinic acid forms a basic lead salt, the formula of which does not well agree with the preparation of the acid as of a monobasic‖ constitution, as otherwise the bisuccinamid would deviate from the constitution of other amids, and because by treating succinic ether with oxide of lead, water is separated. Cahours entertains the same views of succinic acid, because in succinic ether one equivalent of hydrogen cannot be displaced by chlorine.

I am far from giving my decision on the validity of all these characters or adducing corroborating proofs. I confine myself to having directed attention that these characters partially contradict one another, and partially lead to consequences which have neither been made, proposed, nor are admissible. The oxides of potassium and ammonium are considered as isomorphous, but not those of potassium and soda; the oxides of sodium

* *Handbuch der Pharmacie*, von Jeiger, bearb. v. Liebig ii., p. 607.

* Liebig's *Annalen*, vol. xxvi., p. 113.
† Liebig's *Annalen*, vol. xxxvi., p. 257.
‡ *Ibid.*, p. 283.
§ *Ibid.*, vol. xlix., p. 202.
‖ *Ibid.*, vol. xlvii., p. 221.

and ammonium, therefore, are likewise not isomorphous. Why do we admit the existence of a double salt of soda and potassa to serve as a proof, whereas to the double salt of soda and oxide of ammonium we attribute no power to prove? Why then do we not assume the equivalent of sulphuric acid as double, when its acid soda salt on being saturated with ammonia yields not two distinct salts, but one well crystallised double salt?*

If the racemic acid, as it forms crystallised double salts so well with soda and potassa, is bibasic like tartaric acid, if, therefore, in the potassa salt one atom of it be contained only, why is it divided, if the acid potassa salt is neutralised with baryta, lime, or strontian, why does it not form any acid salts with these bases and those of the magnesian series? If the first be true, the second must as well take place.

If half an atom of water speaks against the monobasic nature of an acid, how is it that we do not double the atomic weight of the acetic acid, its combination with strontian, on crystallising above 59°F., yielding the formula $\overline{A}$, SrO+2½Ag.†, with the inability of racemic acid, in other combinations, to act the part of a bibasic acid? How is it with sulphuric acid, the first hydrate of which is $SO^3+\frac{1}{2}HO$?

If the arguments adduced by Fehling and Cahours in favor of the tribasic constitution of succinic acid are well founded, and if this acid, therefore, be really tribasic, how does it happen, that it forms neither acid salt with baryta or magnesia, nor double salts with the alkalies, or with these and the bases of the baryta series? How is it that among all the salts investigated by Döpping and Fehling not one was found, in which one equivalent acid with the oxides of three different radicals is combined, a state which is considered as the prototype of a tribasic acid? The common phosphoric acid, for instance, frequently shows, as in the microcosmic salt, (PO^5, NaO, NH^4O, HO).

It is easily conceived, that it would not be difficult to add to the number of these questions. I, however, believe, that those quoted already are more than sufficient to prove, that the division in question may only then be properly arranged, and we all may agree upon that score, when its principles, and the practical characters of the acids of each class will be more clearly and unequivocally defined than it has been done hitherto. Nothing will then be easier than to decide upon the question, whether the racemic acid is monobasic or bibasic? —nt—

* To remove all doubts on the nature of this salt, I prepared and analysed it, and can only corroborate the statements given by Riffault (*Annal. de Chim. et de Phys*, vol. xx., p. 432) relative to the physical properties and composition of that substance. The analytical results which I obtained are the following:—

1. 1·2707 grammes of air-dried salt lost, without decomposition, 0·2657 water, left on calcining 0·520 sulphate of soda.

2. 0·993 grammes lost, without decomposition, 0·207 water, left on calcining 0·404 sulphate of soda.

3. 0·300 grammes gave 0·740 grammes of sulphate of baryta. Hence results the following composition in 100:—

		I.	II.	III.
NaO	= 390·89	18·01	17·93	17·82
NH^4O	= 326·97	15·07		
$2SO^3$	=1002·32	46·18	.. ..	46·00
4HO	= 450·00	20·74	20·90	20·84
	2170·18	100·00		

† $\overline{A}$ means acetic acid.

ON THE PHOSPHORESCENCE OF THE DIAMOND.*

BY P. RIESS.

The remarkable property of the red light to render the phosphorescence excited by luminous stones less intense, has not, as far as I am aware of, been examined hitherto with reference to the diamond.† And yet it appears to me that this investigation is of much interest, as relative to the diamond we cannot think, as we might have done in the case of other luminous stones, that the light might have produced a chemical change.

Eight years ago I had an opportunity of examining 63 diamonds, which were highly phosphorescent; they became luminous, after having been exposed to the rays of the sun for half a minute, in a dark room for several minutes with a shining yellow light, which gradually becoming less intense offered a bluish lustre, which for 15 or 20 minutes remained perceivable to an acute eye.

To shorten the experiments, I inspected

* Poggendorff's *Annalen der Physik und Chemie*, Feb. 1845.

† This property was discovered, in 1775, in the red of the spectrum, by Wilson, corroborated, in 1805, by Ritter, and, 1816, by Leebeck, observed in the rays of the sun colored by red glass. Ritter, Beiträge, Z. *Kentniss der Galvanismus*, vol. ii., p. 282, and Göthe, *Zur Farbenichre*, vol. ii., p. 706.

the luminous diamond through a coloured (blue) glass, and declared it as being extinguished, when it could no more be seen in that manner. A diamond being free and exposed for half a minute to the rays of the sun, continued to be luminous for 8 minutes, and was shortly after extinguished; covered by a blue glass, and exposed to the sun for 10 seconds, it was perfectly luminous for 7 minutes. This diamond was exposed to the sun, being first covered by a blue glass for 10 seconds, and, then by a red glass for 50 seconds, it appeared to be extinguished, though only one minute and a half had elapsed since it had been excited by the blue light. A diamond, which became highly luminous after having been exposed to blue rays, was exposed to the red ones, and now (2 minutes after the first excitation) it became completely extinguished. The time of the radiation of the various lights, when it amounted to more than 10 seconds, had no influence on their action. A diamond covered by a blue glass, and exposed to the sun for 37 minutes, became not more strongly luminous than in the former experiments, and, after having been exposed to the red rays for two minutes, it immediately became extinguished. All other experiments gave similar results: it is, however, to be remarked, that if a diamond, after being extinguished by a red light, be inspected by the naked eye, it continues shining with almost the same intensity as is perceived in a diamond exposed to blue rays after the lapse of 12 minutes. Whether the phosphorescence of the diamond, when excited by heat, is likewise weakened by the red rays, I have not ascertained; probably because, as I have remarked already, this phosphorescence, compared with that induced by radiation, was only feeble.

The phosphorescing diamond becomes luminous, like other luminous stones, by the blue light as well as the direct rays of the sun; and this phosphorescence is in a high degree reduced by the radiation with the red rays of the sun. —nt—

CHEMICAL ANALYSIS OF A CELTIC WEAPON.*

BY DR. R. FRESENIUS.

LAST year, on digging a new bed for the Wieseck (a rivulet falling into the Lahn), near Giessen, an old weapon was found, which Counsellor Nebel, of Giessen, recognised as an old weapon of the Celts, and known by the name of the cleaving wedge (axe). As we are without information respecting the metallurgical knowledge of the ancient inhabitants of Germany, if we wish to obtain any notion about this matter, we must let the products which we find speak for themselves. It was, then, not devoid of interest to ascertain the chemical composition of the said cleaving axe; conforming myself, therefore, to the wish of Professor Liebig, I undertook its analysis.

The weapon was covered with a very thin layer of oxide, hence it had a tarnished brownish appearance; whereas, on being recently cut, it offered a fine gold-yellow color. It was found cast in a state completely free from vesicles. On being filed and sawed through, it offered an alloy of the hardness of the common red cast (rothguss).

At the qualitative analysis were found, as principal constituents, copper and tin, and besides, in small quantities, nickel, iron, and lead, and inconsiderable traces of zinc and manganese. Quantitatively to determine the constituent parts, the tin was separated from the other metals by treating with nitric acid, the copper and lead thrown down from the solution by sulphuretted hydrogen, and, after having evaporated the filtered mass, the iron and nickel precipitated by potassa. The precipitated sulphurets of copper and lead were then boiled with feeble nitric acid, the solution treated with carbonate of ammonia and ammonia in excess, the precipitated cupriferous carbonate of lead dissolved in nitric acid, and the lead precipitated by sulphuric acid. The ammoniacal solution of copper was united with the filtered mass, and the oxide of copper precipitated with potassa. The iron and nickel I separated by the method of Fuchs, with carbonate of baryta.

From 2·2716 grammes of the alloy were in that manner obtained:

2·6360 oxide of copper, 0·1944 oxide of zinc, 0·0230 sulphate of lead, 0·0094 oxide of iron, 0·0090 oxide of nickel. The quantities of manganese and zinc were not sufficient to be weighed.

Hence follows the following composition of the alloy:—

	In 100 parts.
Copper	91·99*
Tin	6·73
Lead	0·69
Iron	0·28
Nickel	0·31
Manganese, Zinc	traces.
	10·000

*Liebig's *Annalen der Chemie und Pharmacie*, i., 1845.

* These numbers are calculated from the loss, which was directly found to be 92·65.

From these numbers it appears that this alloy has completely the same composition as gun-metal, which generally contains 90 copper and 10 tin.

With reference to the amount of nickel and iron, it is very remarkable how well the ancient armorers knew how to hit the point, as it appears from the experiments of Dussaussoy that a slight addition of iron materially improves the quality of the bronze, in as far as smaller objects may then be cast of it. By this expedient it is rendered less liable to form vesicles, acquires more hardness and tenacity—properties which are especially to be kept in view in the manufacture of weapons intended to hit and thrust.

—nt—

ON THE PROTOXIDE OF NICKEL.*

BY F. A. GENTH.

(Abridged from a Letter directed to Professor Liebig.)

On investigating all the products obtained, on smelting the copper schist at Riechelsdorf, the two first copper discs obtained at the production of the black copper (schwarz kupfer), which on account of their impure condition were melted again, seemed particularly worthy to be noticed.

The first of these discs consists of metallic copper, the surface and cavities of which are strewed over with a layer of small almost microscopical crystals, which I considered to be oxide of copper, as it has precisely the appearance of some naturally occurring red copper ore.

On dissolving the copper in nitric acid these crystals remained with some slacks.

Under a lens they were recognised as distinct regular octahedrons, without any further combination. The color is grayish-black with a red tint, the powder browned verging into gray. They are opaque and possess a metallic lustre; and not magnetical. Their hardness is between that of the calc spar and that of the fluor spar; they are easily cleft; specific gravity = 5·745.

They are insolable in nitric and hydrochloric acids, difficultly soluble in boiling sulphuric acid. They are not altered by fusing with carbonate of potassa, but are easily reduced by borax, and the phosphor-salt, communicating to the glasses thus formed the characteristic color of nickel. By the acid sulphate of potassa they are dissolved on fusing. The crystals fused with the acid sulphate of potassa after cooling, offered a citron yellow mass, which was easily dissolved in water, yielding a green liquid, from which soon after crystals separated of the appearance of sulphate of potassa and protoxide of nickel. The solution gave all the reactions of the pure protoxide of nickel. The cobalt was then tested by your method, with cyanide of potassium, but in one gramme of these crystals was so little cobalt found, that it scarcely could be demonstrated by the blow-pipe on treating it with a borax bead.

It now remained only to examine in which combination the nickel was separated. I found, by a provisionary examination, that I had to deal with an oxidated body. To be enabled to make an exact analysis, I first prepared a chemically pure material, by reducing the crystals to powder, passing them through linen, and afterwards fusing with carbonate of soda; to remove all slacks, the fused mass was repeatedly boiled, the crystals treated again with concentrated nitric acid, washed with water, and dried.

By dry hydrogen, the purity of which I previously ascertained, at a strong red heat 5·5780 grammes of these crystals were reduced. These gave 4·3905 nickel. Hence 1·1875 oxygen.

This gave the proportion—

1·1875 : 4·3905 = 100 : 369·7263.

From which it results, that these crystals are the chemically pure protoxide of nickel, and that the atomic weight given by Berzelius is fully correct, as the very inconsiderable deviation lies within the unavoidable errors of calculation.

The volume of its atoms is almost as great as that of the isomorphous magnesia.

$\frac{469 \cdot 7263}{5 \cdot 745} = 81 \cdot 76$.

It is interesting, that this protoxide of nickel crystalises with the regular octahedron, unlike oxide of zinc, which crystallises with the rhomboëdrical system, showing the direction of the lamina to be towards the sides and terminal surfaces of the hexaëdric prisms.

—nt—

* Liebig's *Annalen der Chemie und Pharmacie*, i., 1845.

PROVISIONARY NOTICE ON WOOD, SULPHURIC ACID, AND THE VOLATILE FAT ACID OF OIL OF COCOA-NUTS.*

BY DR. H. FEHLING.

(Abridged from a Letter addressed to Professor Liebig.)

The action of sulphuric acid on fecula, wood fibre, &c., occupied my mind long

* Liebig's *Annalen der Chemie und Pharmacie*, i., 1845.

ago; it was near to assume, that the "catalytic power" acts in this case in the same manner as in that of formation of ether from sulphuric acid and alcohol. Braconnot's acide vegeto-sulphurique seemed to confirm this assertion, and this acid was to be examined closer, and to be ascertained whether a similar acid may be produced from amylon and sulphuric acid. I prepared this kind of salts in the spring, by putting together linen, purified, and again fecula with from 1 to 1½ parts of hydrated sulphuric acid, saturating afterwards the diluted liquid with carbonate of baryta, oxide of lead or lime, and evaporating the filtered mass under the air pump. These salts remained 5 or 6 months under the air pump ere I had time to investigate them; they were all easily soluble, and not crystallisable: a more minute examination showed, that all of them, agreeing in that respect with ether of sulphuric acid, contain for 2 equivalents SO^3—1 equivalent BaO,* CaO, or PbO; at the same time, however, yielding various amounts of carbon, hydrogen, and oxygen, partially 70, 90, and more equivalents of carbon, by which it appears to me, that they contain gum or sugar; the concentrated watery solution of the baryta salt of fecula, on being treated with alcohol gave a considerable precipitate, in which most of the salt was contained, as in this precipitate much baryta was found, whereas in the liquid only traces were discovered. The same precipitate contained still much gum, which may most conveniently be separated by alcohol and sulphuric acid. I lately found in one of the most recent numbers of Erdmann's *Journal*, that a French chemist had more closely investigated the vegeto-sulphuric acid; I, therefore, would request you to receive in your journal a short notice on the salts formed by fecula and sulphuric acid, if you do not deem it disqualified for this purpose. I am still occupied with this combination, but am slowly progressing, as all the evaporations must be operated under the air-pump, and especially any elevation of temperature is avoided.

I have shortly likewise investigated the volatile fat acid of the cocoa-nut oil; the baryta salt is $C^{16}H^{15}O^3 + BaO$ that is, according to Lerch, caprylate of baryta; I possess now a few half ounces of the pure baryta salt, and will separate from it the acid, to investigate it, and to prepare from it the ether, which has a very sweet odor. On this, as well as on some other fat acids, I will communicate to you a complete notice for your annals.

—nt—

* For instance, 0·588 on fusing with nitre and dissolved in the acid, gave 0·043 SO^3, BaO; the acid filtrated mass, on being precipitated with chloride of barium, gave 0·042 SO^3, BaO; 0·458 substance, 0·587 CO^2 and 0·251 HO.

This gives in 100 parts—

C	35·24
H	6·07
BaO	4·80
SO^3	5·02

Calculated in 100 parts—

$C^{90}H^{90}O^{90} + 2SO^3 + BaO$.

C	35·8
H	5·9
BaO	5·0
$2SO^3$	5·3

CHEMICAL COMPOSITION OF CUBAN.*

BY S. H. SCHEIDHAUER.

Of the cuban (*vide* Professor A. Breithaupt's description of the new hexaëdrical spaltbarer kies [cleaveable pyrites] in Poggendorff's *Annalen*, vol. lix. p., 325), I made two almost completely concordant analyses, hence I do not hesitate to give the mean of the result as follows:—

Sulphur	34·78
Iron	42·51
Copper	22·96
Lead	trace
	100·25

By dividing the corresponding atomic weights with the numbers found, the following quotients are obtained:—

For sulphur	0·172
,, iron	0·128
,, copper	0·058

hence the atoms of sulphur, iron, and copper are respectively = 172 : 128 : 58, which approaches to the proportion 3 : 2 : 1, which leads to the formula $Fe^3S + Cu\,S$.

And it hence results from calculation—

S	35·97
Fe	40·44
Cu	23·38

values which do not much deviate from those found by experience.

NOTE BY A. BREITHAUPT.

If, according to more recent determinations, the atomic number of iron is taken as = 350, and that of sulphur as = 200, Sulphur, iron, copper are = 1739 : 1214 : 580;

* Poggendorff's *Annalen der Physik und Chemie*, Feb., 1845.

and by calculation we obtain for $Fe^2 S + CuS$—

Sulphur	35·38
Iron.....	41·28
Copper	23·34

which more nearly approaches to the found values. I, however, venture to add, that in the latter is still some sulphur wanting, and a little iron too much. By the resemblance of cuban with the copper pyrites, and with the magnetical pyrites, with both minerals it is associated, it becomes probable that the copper is contained therein as CuS, and that, perhaps, the formula $Fe^2 S^2 + 2 Cu^2 S + 6 F S$ be more correct, though it appears to be less simple.

For the rest the above formula $2 Fe S + Cu$ is likewise given for the copper pyrites (*vide* Rammelsberg's *Handwörterbuch*, vol. i., p. 364); but there a mistake occurred, as this formula does not agree with the numbers given in that place, which ought to have been the same as the above.

—nt—

THE CHEMICAL COMPOSITION OF THE KYROSITE.*

BY C. H. SCHEIDHAUR.

Of the kyrosite† described in Poggendorff's *Annalen*, vol. lviii., p. 282, by Professor Breithaupt, I have made four analyses, and as the values obtained show some variations, I will give them all, one of which only contains all determinations:—

Sulphur		52·21	53·05	
Iron	45·01	46·00	45·60	45·90
Copper ..	2·07	..	1·41	1·60
Arsenic ..	0·90	..	0·93	0·95

As the exterior appearance of this mineral differs so much from the pyrites, *its amount on copper and arsenic, however inconsiderable, must be looked upon as essential.* I did not yet venture to assign to it a formula.

NOTE BY BREITHAUPT.

The arithmetical mean of the above analysis gives—

Sulphur....	52.63
Iron	45·63
Copper	1·69
Arsenic	0·93

The number of atoms of sulphur, iron, copper, and arsenic, are in the proportion of—

261 : 130 : 4 : 2,

and though from this a formula so simple as might have been desired does not result, we fully agree with Mr. Scheidhaur's assertion. The most simple formula might be—

$32 FeS^2 + CuS$,

in which $Fe S^2$ is replaced by $Fe As^2$; because—

$129 FeS^2 + (4 CuS + FeAs^2)$

is not well admissible. It is certain, that the kyrosite being possessed of a particularly vivid metallic lustre, and a peculiarly yellowish color is by no means a mixture, and the less so, as it occurs in crystallised pure masses of the size of a fist, without being accompanied by another mineral endowed with a metallic lustre. Among matters associated with this mineral I remarked, though very rarely, a coating of kupferindig (Breithaupt) CuS, which circumstance seems to prove, that in the kyrosite is contained CuS and Cu^2S.

—nt—

* Poggendorff's *Annalen der Physik und Chemie*, Feb. 1845.

† Though I have expressly pointed out that the kyrosite is not the same as Werner's white copper ore (weisskupfererz) yet these bodies have already been confounded.

ON THE COMPOSITION OF THE CRYSTALLISABLE HYDRATES OF BARYTA AND STRONTIA.*

BY M. FILHOL.

Baryta and strontia form, in combining with water, each two well-defined combinations; the first of these combinations, formed of 1 equivalent of base and 1 equivalent of water, is perfectly known to all chemists; it is not the same with the crystallisable combination which is obtained by allowing to cool a solution of baryta or caustic strontia saturated by the aid of heat.

Crystallisable hydrate of baryta contains, according to Thénard and Berzelius, 9 equivalents of water to 1 of anhydrous baryta; the latter chemist says, that the crystals of hydrate of baryta contain so much water, that the quantity which may be obtained forms more than double the weight of the anhydrous baryta which furnished them; this does not agree with the preceding results. Wishing to be positively certain of the composition of the crystallisable hydrates of baryta and strontia, I prepared a very large quantity of each of them, and I analysed them with the greatest care. I now detail in succession the analysis of each of them.

100 parts of hydrate of baryta, dried between the folds of filtering paper, at the ordinary temperature, furnished me in six successive analyses the following quantities of sulphate of baryta:—

* *Journal de Pharmacie*, April, 1845.

1.	78·32	which corresponds to baryta	51·45
2.	78·40	,, ,,	51·48
3.	78·27	,, ,,	51·40
4.	78·35	,, ,,	51·45
5.	78·28	,, ,,	51·40
6.	78·34	,, ,,	51·45
Mean	78·32		51·43

These crystals were therefore formed of—

Water	48·57
Baryta	51·43
	100·00

By supposing that this hydrate is formed of 8 equivalents of water to 1 of anhydrous baryta, the following numbers are arrived at :—

Water	48·64
Baryta	51·36

This hydrate is therefore formed of Ba O + 8 HO.

These crystals fuse in their water of crystallisation at a moderate temperature; fusion commences at 212°F.; heated further, they lose their water of crystallisation. Their density, taken at 60°F., is 2·188; Berzelius states their density to be 4, which is certainly an error; this latter density is that of the hydrate with 2 atoms of water. The crystallisable hydrate of strontia is formed, according to most chemists, of 12 equivalents of water to 1 of strontia, or of

Water	67·62
Strontia....	32·38
	100·00

I have made several analyses of these crystals; my analyses are far from agreeing with the foregoing.

100 parts of crystallised hydrate of strontia furnished, on the average, 69·75 sulphate of strontia, which corresponds to

Strontia....	39·17
Water	60·83
	100·00

If the formula Sr O + 9 HO be admitted, we find that these crystals should be formed of—

Strontia....	39·01
Water	60·99
	100·00

The crystallisable hydrate of strontia contains 9 equivalents of water to 1 of strontia; its density is 1·911 at the temperature of 60°F. It will doubtless be astonishing to observe that baryta and strontia, which resemble one another in so many respects, give 2 crystallisable hydrates, whose composition is not analogous; but I would observe, that differences of the same kind are found in the quantities of water which other anhydrous compounds of baryta or strontia take to form hydrates; thus the crystallised chloride of barium contains 4 atoms of water, whilst that of strontium contains 12. The nitrate of baryta is anhydrous, whilst the nitrate of strontia takes water of crystallisation: in general, the hydrated compounds of strontia contain more water than the corresponding compounds of baryta.

OBSERVATION OF AN ACID RAIN.*

BY M. DUCROS, OF NIMES.

At the end of May, or at the commencement of June, 1842, a storm occurred in our town, accompanied by violent claps of thunder; a considerable quantity of hail fell: this storm ravaged the woods and vineyards in the direction of Nimes to Saint Geniez de Melgoin. The quantity of hail which fell in a short time was sufficiently great to cover the court of the house which I inhabit, as well as the terrace which crowns it, with a layer of at least 4 or 5 centimetres. The size of the hailstones scarcely exceeded that of the smallest ordinary peas, and their form was oovid; they appeared to be formed of several layers, the first of which was less transparent than the others. At the commencement of the storm chance led me to put in my mouth several hailstones which I had collected in my hand, and the peculiarly pungent taste, which I at first mistook for the impression of cold on the tongue, suggested to me the idea of ascertaining whether this hail did not contain some peculiar principle.

I then put on my terrace a glass flask, surmounted by a funnel, which I at first well washed; my flask, when full, was stopped with its glass-stopper, and, after the hailstones were completely melted, at the ordinary temperature, which was effected only after 6 or 7 hours, the liquid was transparent, colorless, and inodorous. A flask which, filled with distilled water, weighed 507 grammes 45 centigrammes, weighed, when filled with this water, 570 grammes 95 centigrammes; the taste of this water was agreeable, and produced in the mouth a sensation of freshness; the flask in which the hailstones had been collected, and in which the liquefaction was operated, when it was emptied, had evidently lost its transparence, as if it had been soiled with a fatty body. I cannot better compare this effect than to that produced by orange-flower water in vessels in which it has been contained.

* *Journal de Pharmacie*, April, 1845.

Tested by several reagents, this water presented the following phenomena:—It reddened litmus paper, tincture of litmus and syrup of violets, and restored to yellow turmeric paper which had been turned brown by potassa; and this reaction did not disappear after 24 hours' exposure to the air. Exposed in a capsule to prolonged ebullition, and when the evaporation was very far advanced, the acid reaction was neither increased nor diminished in intensity, and no action was manifested during the ebullition. An excess of tartaric acid caused no apparent turbidness; however, 24 hours after, on the sides of the vessels in which I operated, there were deposited granulations sensible to the touch and visible by placing the glass between the sky and the light. Nitrate of silver caused no sensible change. Sulphate of iron, after some hours' contact in a closed flask, deposited some yellowish striæ. Oxalic acid and quadoxalate of potassa produced no perceptible phenomenon. Chloride of barium did not produce any precipitate, nor even apparent turbidness.

A certain quantity of this water was introduced into a medicine phial to which I had adapted a bent tube, the phial and the tube were filled; the extremity of the tube was fixed under an eprovette, resting on a pierced cork, in a small earthenware pan, containing, as well as the tube eprovette, distilled water, to serve as a hydropneumatic trough. The phial and the tube adapted to it contained 126 grammes of liquid. The apparatus was arranged; I heated the phial and obtained in the eprovette two or three bubbles of gas, which I passed into a small tube over mercury; I added a few drops of lime-water, which underwent no change.

From the above experiments, I draw the following conclusions:—

1. That this liquid contains a free acid, since it reddened litmus paper and tincture of litmus, and restores to yellow paper turned brown by an alkali.

2. That this acid is not carbonic acid, since the colored papers and tincture of litmus did not change after 24 hours' exposure to the air.

3. That this acid is volatile, since, by long-sustained ebullition, the liquid retains its acid reaction without becoming more intense by evaporation.

4. That this acid is not hydrosulphuric acid, since it does not give any sulphurous odor.

5. That this volatile acid is not hydrochloric acid, since nitrate of silver did not cause any precipitate.

6. That among the volatile or gaseous products free oxygen might be found, since the sulphate of iron allowed some yellowish striæ to be precipitated; but, as it is known, that the air dissolved in water is very rich in oxygen, on account of this gas being of much greater solubility than nitrogen, I considered that this precipitation was due to the presence of a little air.

7. That the gaseous product contained in the eprovette was not carbonic acid, since lime-water was not rendered turbid.

8. That the fixed principle was not a salt of lime, since there was no precipitation by oxalic acid, nor by quadoxalate of potassa.

That it was not a hydrochlorate, since nitrate of silver produced no change.

That it was not a sulphate, since chloride of barium caused no turbidness.

But that this principle might contain a salt of potassa, although it did not give a very apparent indication by tartaric acid.

After having endeavored to establish, by some inductive experiments, what might be the constituent principles of this water, to obtain these principles, I operated in the following manner:—

A certain quantity of this water was submitted to distillation in a glass retort, to which I had adapted a glass receiver, both previously well washed with distilled water; after having retained in the receiver at least 99 per cent. of the liquid employed, I dismounted the apparatus.

In order to obtain the almost dry matter, which remained in the retort, I poured into it a little distilled water, well washed the retort with it, and poured it into a watch-glass, serving as a capsule, and placed the capsule on a hot iron plate, in order to complete the evaporation of liquid, with the view of being able to obtain a crystallisation, if it should take place. When only a small layer of liquid remained, I put my capsule under a bell-glass and left it; after 24 hours, I examined the product in the capsule which had been reduced to dryness; there then remained only a small crystalline deposit; it appeared to me to present the prismatic form. I collected this small crystalline mass on a paper and dried it perfectly; in this state it weighed 15 centigrammes. Its taste was cool, pungent, and slightly bitter; thrown on a burning coal it fused quickly and rendered combustion much more vivid.

Another portion of this salt was put in a small watch-glass; I poured on it concentrated sulphuric acid, which, without heat occasioned no reaction; but, on raising the temperature a little, there was a perceptible disengagement of white vapors of a pungent acid odor, characterising nitric acid. Although the quantity of matter was not suffi-

cient for making other experiments. I have not the slightest doubt concerning the nature of this salt which I consider to be nitrate of potassa.

With respect to the distilled water, whose reaction was acid, I saturated it with potassa, by means of a small glass-rod, which I dipped in the alkaline solution, and with which I afterwards stirred the liquid; when there was no longer any acid reaction, I distilled in the retort which had been used for the first distillation, and which had been well washed with distilled water, and nearly to the same point as the first time. I operated in the same manner, also, in order to complete the evaporation to a suitable degree.

After 24 hours' repose under a bell-glass, I also obtained crystals similar to the first, but rather more abundant and of a better determined form, and which gave all the essential characters of nitrate of potassa.

The above facts leave no doubt in my mind that nitric acid was accidentally formed during the storm and formation of the meteor, and I found this opinion on the fact verified by experiment, that air, exposed to a current of electric sparks may be converted into nitric acid, provided always that it is in contact with moisture, nitric acid not being able to exist alone; it is known that the most concentrated nitric acid is represented by 1 proportion of real acid and 1 proportion of water. The air, therefore, being charged with a very great quantity of electricity, is it not probable that those powerful discharges which took place during the storm, combined the constituent principles of the air in such a manner as to form nitric acid, as in the experiment of the laboratory?

With regard to the nitrate of potassa found in combination, might it not proceed from the infinitely attenuated molecules raised by the winds which prevailed before and during the storm, and which, thus suspended, were attacked by the nitric acid formed by the electricity? Such is my opinion. Men versed in science will be able to appreciate its justice; my intention is only to communicate a fact which appears to me new and curious.*

PREPARATION OF PHOSPHORUS FROM URINE.

To the Editors of The Chemist.

GENTLEMEN,

SOME time since, with a view to preparing phosphorus from urine, I evaporated a considerable quantity of the same to dryness, and the salt thus obtained being intimately triturated with charcoal in fine powder was subjected to distillation in a cast-iron retort. A large quantity of gaseous matters was disengaged, consisting principally of carbonic acid, together with a vapor having a strong empyreumatic smell. The retort was kept at a white heat for, I dare say, half an hour, with the beak immersed in water; still no phosphorus was obtained. Subsequently, I treated calcined bones in the manner recommended in the *Penny Cyclopædia*, with sulphuric acid and water, and obtained about half an ounce of phosphoric acid, which was also mixed with charcoal, and distilled as before. In this instance, as in the former one, a large quantity of gaseous products came away, accompanied by a vapor of precisely similar smell.

The conditions detailed in the *Cyclopædia* I observed with the strictest accuracy, except that I made use of an iron retort instead of one of earthenware. Perhaps this circumstance might account for my failure. If any of your correspondents can set me right, I shall feel grateful for it.

Wexford. F. S. DAVIS.

REPORT ON THE MEMOIRS OF MM. SIMON AND HARDY RELATIVE TO THE CULTURE AND PRODUCTS OF THE PAPAVER SOMNIFERUM IN THE CLIMATE OF ALGIERS.*

BY MM. DE MIRBEL, BOUSSINGAULT, AND PAYEN (REPORTER).

MM. DE MIRBEL, Boussingault, and myself have been charged with the duty of reporting on Memoirs by MM. Hardy and Simon, relative to the culture and products of the *papaver somniferum* in the climate of Algiers; we now submit to the Academy the result of our investigation.

M. Hardy, in a very detailed memoir, gives an account of his new efforts to fulfil the intentions of M. C. Marechal, Minister of War, and to solve the question of the utility of the production of opium in Algiers.

With this view, the director of the central nursery, in continuing his experiments, proposed to cultivate a greater extent of land, in order better to ascertain the influence of seasons, of the period of sowing the seed, of agricultural care, &c.; wishing, also, to keep an account of the expenses and to deduct them from the value of the crop, M. Hardy

* The presence of nitric acid in hail is a fact long known in science; we have, however, inserted the observation of M. Ducros, as it confirms those previously made.

* *Comptes Rendus*, No. 14, April 7, 1845.

thought, and rightly, that it was necessary to determine the quality of the products.

This latter part of the operations was more particularly reserved for us; consequently, and in order to complete the basis of the calculations, we at once present our experiments and observations in this respect.

One of the samples of opium, collected by M. Hardy, had been extracted during the rain; we analysed it separately. 100 parts contained 89·1 of dry substance, and gave 4·67 of pure morphia. In taking account of the excess of moisture in this sample, in order to compare the amount of morphia with the products sent over last year, it was found that 92·4 of dried substance should have furnished 4·84, a number which does not nearly approach that of 5·02 found in the opium of 1844.

The other sample, which represented the quality of the greater part of the crop, extracted during more favorable weather, contained 9·5 of water per cent., and gave 4·94 of morphia; by reducing its proportion of water to 7·6, in order to establish the comparison between the product of 1843, it is found that it represents 5·10 of pure morphia per cent., that is to say rather more than the opium of the foregoing crop.

Thus the opium obtained this time in a more extended culture is evidently superior in quality to the products examined last year.

This result, therefore, appears to confirm the hopes which we had conceived relative to the possibility of obtaining in Algiers opium of good and uniform quality.

The second important product of the same crop consists in the oil which is easily extracted from the seeds after the opium has been collected.

Profiting by the assistance of M. Tripier, director of the Pharmacy of Algiers, M. Hardy proved that the poppy seed gave 45 per cent of oil, the greater part of which flowed out without heat and is comestible.

The seeds which have been sent to us gave 42·6 of oil, that is to say, 2·4 per cent. less than the quantity obtained by M. Tripier. Probably, the difference observed was owing to some alteration during the voyage. The sample of oil received from Algiers is like the oil of oeilette, and slightly more colored than those of the north of France.

The more extended culture made last year permitted the verification and completion of the instructions communicated, in 1843, by the care of M. Liataud.

The following is a summary of the favorable conditions deduced from the latest observations. It requires a soft light substantial soil, particularly well permeable to water. If it retained the latter, the root of the poppy, which is large, fleshy, and soft, would very soon be destroyed. The soils which appear preferable are those in which sand predominates nearly in the proportions of two-thirds to one-third of clay. This land should be amended the year before by transient manures, and prepared during the summer by deep digging, so as to render the earth as light as garden land.

It is also very useful for the plantation to be sheltered as much as possible from the west winds; the slightly inclined declivities which face the east, seem to present the most favorable exposure.

The seeds should be sown in the autumn; they appear immediately after the first rains. They are covered with leaves by a slow vegetation during the winter; and, in the spring, they give stems and capsules of a strength not to be obtained by seeds sown during or after the winter, forced as they are by conditions of climate as soon as those sown four or five months earlier.

It may happen in a rainy spring, like that of this year, for example, that these late-sown seeds may give very fine products, but this case is of rare occurrence.

The land should be divided into beds of 2 metres broad, with paths of 40 or 50 centimetres left free for the operations of weeding, gathering, &c.; the length of the beds is immaterial. From 2 kil. 500 grs. to 3 kil., may be sown in each hectare of land.

Sowing in a line does not answer for so fine a seed, because it is difficult to bury it at a proper depth; there are almost always vacant parts, the seeds which are too deep in the ground not being able to spring up.

As soon as the young plants have four or five leaves, they should be freed from weeds, and the superfluous plants removed so as to leave a distance of 0·20 to 0·25 centimetres between each. The most vigorous plants should be preserved. Afterwards, when necessary, the ground is dug up a second time, and care is taken that no other plant grow in the plantation. When the young poppies completely cover the ground with their foliage, the binette must not be introduced any more, in order to avoid bruising the tender leaves. From this period until that of the maturity of the capsules, nothing is done to the plantation, except plucking up with the hand any weeds which may show themselves here and there, and of which the soil should always be carefully purged.

An important precaution to be taken during the operations of weeding, is not to wound the principal roots or the pivots, with the instruments made use of; otherwise the plant would perish by the decay produced

by the effusion of its juices. The poppy will not bear transplanting; this means cannot therefore be employed for replenishing vacant places, as is the practice with some other cultures.

When it is perceived by their color turning yellow, and by the hardness produced by the accumulated juices, that the capsules have attained the proper maturity, the cultivator should hasten to put in requisition the workmen necessary for incising the capsules and collecting the opium; one hectare will require from 15 to 20 people for 12 or 15 days. Children should not be employed for this work; they cannot reach the capsules.

The product may be collected 20 hours after having incised the capsules, but this operation proceeds less quickly; by incising for three hours, the work of six or seven hours collecting is prepared. Thus the incising should be performed during the three hottest hours of the day, and the tears of opium which flowed out the night before collected during the intervals of morning and evening.

The instrument which appeared most suitable for making the incisions is a penknife with a convex blade; and, for collecting the opium, the blade of the carving-knife of the same form.

Taking account of the extraordinary and unfavorable variations of the temperature of this year, M. Hardy thus sets forth the culture of one hectare—

	francs.
Labor with the hoe, 96 days at 2 francs	192
Sowing, harrowing by hand, 44 days at 2 francs	88
Twice digging	118
Collecting opium, 229 days at 2 francs	458
Total for opium..	856
Collecting seed, 37 days at 2 francs	74
	930

PROBABLE PRODUCT OF ONE HECTARE.

By supposing one-third more for the produce of opium (for it must be admitted that the season will not be always so unfavorable as this year), M. Hardy arrived at the following results—

Opium, 13 killogrammes, 268 at 30 francs	698
Poppy-seed, 11 hectolitres, at 30 francs	330
690 bundles of stalks, at 10 centimes	69
Total produce per hectare	1097
Nett profit....	167

We have received from the Minister of War two samples of opium obtained from another culture by M. Simon, by means of peculiar processes. This opium was contained in poppy-heads cut in two.

No. 1 lost by desiccation 8·5 per cent., and gave 3·70 of pure morphia, representing 3·74 for the opium reduced to the proportion of water taken as a term of comparison.

No. 2 contained 6·45 of water; 3·86 of morphia was obtained, a quantity equivalent to 3·82, for the normal degree of humidity.

These results, verified by twice repeating the analysis, prove that the quality of the opium extracted by M. Simon is rather inferior to that of the samples from Algiers, collected in 1843 and 1844.

The smaller proportions of morphia might, indeed, be compensated, or more than that, by the greater quantities of opium collected; but not having received any intelligence concerning the new processes employed, nor concerning the products obtained, we cannot form any opinion in this regard.

The examination of the memoir and samples sent by M. Hardy, and of which we have just given a report, shows that, notwithstanding exceptional and unfavorable circumstances of temperature, the quality of the opium is sustained and even improved; it is therefore permitted to us to hope that better results may yet be obtained in the ordinary conditions, by continuing with the same care these interesting agricultural experiments.

Everything leads us to believe that we may thus be able to obtain the important result of furnishing the medical art with an agent whose useful properties will be guaranteed by the uniformity of its composition.

The efforts to be made in order to obtain so high an object appear to us very worthy of being encouraged by the approbation of the Academy.

INVESTIGATION OF SEVERAL NATURAL AND ARTIFICIAL COMBINATIONS OF PHOSPHORIC ACID.*

BY C. RAMMELSBERG.

(*Communicated to the Royal Academy of Sciences, at Berlin, 6th Jan.*, 1845.)

MANY very interesting papers on the composition and properties of alkaline and earthy phosphates have recently been published, whereas other combinations of the same acid were lost sight of, such is particularly the

* Poggendorff's *Annalen der Physik und Chemie*, Feb., 1845.

case with reference to alumina, magnesia, and iron. This is chiefly owing to the difficulty met with in analysing them, which not unfrequently requires much time. The first impulse to the following experiments was given by some rare minerals, being natural phosphates, the composition of which has not yet been fully ascertained. This is chiefly the case with reference to the *amblygonite*, *azulith*, and *wagnerite*, which are remarkable no less on account of their rare occurrence than by their constitutional parts.

WAGNERITE (PLEUROKLAS).

For the only investigation of this most rare substance we are indebted to Fuchs, who, in the year 1821, obtained the following result:—

Phosphoric acid	41·73
Magnesia	46·66
Oxide of iron	5·00
Oxide of manganese..	0·50
Hydrofluoric acid....	6·50
	100·39*

Fuchs resolved the mineral by sulphuric acid, and separated the sulphate of magnesia from the phosphate of magnesia by water. The latter was decomposed by boiling with potassa ley, whereas the phosphoric acid and the fluorine were not determined directly. On a minute examination of the whole process of the investigation alluded to, it soon becomes evident that the result is merely approximative, and a repetition of the analysis desirable.

As this substance is extremely scarce, I am highly obliged to Bergrath Haidinger for having supplied me, as he was frequently kind enough to do, with material sufficient for a complete investigation. On the specimen under investigation were found large opaque reddish crystals, and small transparent ones of a wine-yellow color of wagnerite in a green argillaceous mass, associated with quartz, calispar, and bitterspath.

The specific gravity was found by Fuchs =3·13. I have determined it to be in the transparent portions=3·068, and in the opaque ones=2·985 (temperature=11°4 R.)

Owing to the presence of fluorine, the analysis of the wagnerite is more difficult than it seemed to be at first, and becomes more complicated still, if it is correct what we learned by experiments, viz., that the phosphoric acid cannot be completely separated from the magnesia by fusing it one time only with carbonate of soda. Three analyses were made with most possible variations in the methods adopted, the particulars of which I will in a brief manner indicate in the following

1. *Resolution by sulphuric acid and carbonate of soda.* — The fine powder was covered by sulphuric acid in a platinum crucible, from which, owing to an admixture of silica (quartz), when still cold, fluosilicic gas was disengaged. This was then heated until the whole of the fluorine, and the greatest portion of the sulphuric acid were driven off, afterwards macerated in water, and mixed with four times its quantity of carbonate of soda, evaporated, and calcined for half an hour. The mass, after being fused, was washed with water, and from the obtained alkaline solution, after repeated evaporation, and addition of carbonate of ammonia and redissolution, the phosphoric acid was precipitated by chloride of calcium.

The lixiviated residue, after being dissolved in hydrochloric acid, gave a precipitate with ammonia, which contained oxide of iron, alumina, and magnesia, with phosphoric acid, wherefore it was to be fused again with carbonate of soda. The bases were separated by the known methods, and the lime and magnesia were similarly removed from the ammoniacal liquid.

2. *Resolution by sulphuric acid.*—The resolved mass was finally exposed to a temperature short of redness; until vapors of sulphuric acid appeared no longer. It was then extracted by water. According to a statement of Fuchs, by this treatment, sulphate of magnesia (oxide of iron) alone ought to have been dissolved, and by the determination of the sulphuric acid it could then be hoped to ascertain the equivalent of fluorine. But this is impossible, partly because the specimen always contains a little silica, which escapes as fluoride of silica, partly because water, besides sulphuric acid, likewise extracts phosphate of lime. By this circumstance the analysis is rendered complicated, and this method is, therefore, not to be recommended. What is insoluble in water was here likewise resolved by calcining with carbonate of soda, though by this operation the whole amount of phosphoric acid could not be extracted, and the precipitate obtained afterwards with ammonia, was to be digested, and boiled with ammonia for a long time.

3. *Resolution by a mixture of carbonate of soda and hydrate of potassa.* The result obtained by this method was the best, it was employed in the analysis of the most transparent portions of the mineral, which were the most recent and purest ones. The quantity of potassa employed was equal to that of the substance (3·84 grammes), and of carbonate of soda three times as much. The

* *Schweigg. Journal*, vol. xxxiii., p. 269.

mass was completely fused, and then treated with water, when it yielded an alkaline liquid containing phosphoric acid and fluorine, and in the residue was left the magnesia with the other bases.

a. The liquid was placed in a platina capsule and dried by a water-bath, covered with water, an inconsiderable residue of phosphate of magnesia filtered off, treated with hydrochloric acid until it became of a slightly acid reaction, free from carbonic acid by a very gentle heat, and then in close vessels thrown down by ammonia and chloride of calcium. The precipitate, consisting of phosphate of lime and fluoride of calcium, was resolved by sulphuric acid and alcohol, and by this the lime determined. The alcoholic liquid was somewhat evaporated, mixed with ammonia and chloride of calcium, and the phosphate of lime finally, by resolution, with sulphuric acid investigated relative to its amount of phosphoric acid.

b. Left on dissolving in hydrochloric acid some silica. The precipitate thrown down by ammonia in this case contained likewise some phosphoric acid, hence it was again calcined with carbonate of soda. It consisted of oxide of iron, lime, and magnesia, and one portion of the two latter remained in the ammoniacal filtered mass still to be determined.

These rather laborious processes, but for which no better and shorter one may be substituted, yielded the following numbers:

	I.	II.	III.
Silica	..	..	2·68
Phosphoric acid	41·89	40·23	39·56
Magnesia	42·04	38·49	45·07
Lime	1·65	4·40	2·32
Oxide of iron	2·72	3·31	4·47
Alumina	0·55	0·96	..
Fluorine	..	..	9·12
			103·22

The silica is in the state of quartz, only mixed with the wagnerite; it must, therefore, be subtracted; the same is the case with the alumina, and small quantities of the other bases, which being combined with carbonic acid are present as calc spar and bitter spar, and, notwithstanding the pains taken in picking out, the fragments could not be removed. Its quantity cannot be determined with accuracy; it is, however but inconsiderable. After subtracting in III., the analysis which succeeded best, 2·68 per ct. of silica are left.

Phosphoric acid	40·61
Magnesia	46·27
Oxide of iron	4·59
Lime	2·38
Fluorine	9·36
	103·21

It is seen that this result essentially differs from that given by Fuchs, with reference to its amount of fluorine only. On attempting to calculate, basing ourselves on analysis III, the quantities of protoxide of iron and lime, the last being for the most part replaced by magnesia, are found to be as 1 equivalent to 4·43 of the latter, the whole thus amounting to 50·70 per cent.

But 9·36 fluorine, to form Mg Fl require 6·34 Mg = 10·34 magnesia, and, therefore, the phosphate consists of—

		Oxygen.
Phosphoric acid	40·61	22·75
Magnesia	40·36	15·62

Taking into consideration that by the method of investigation the whole amount of phosphoric acid and fluorine will scarcely be found, and that in the magnesia a small quantity of the first named body may still be retained, I think no doubt can be left, that the phosphate of magnesia is to be considered as Mg^3OP^5, of which 1 atom is combined with 1 atom fluoride of magnesium, which represents the wagnerite.

$$Mg\,Fl + Mg^3OP^5$$

Its composition in 100 parts is as follows:—

Phosphoric acid	43·32
Magnesia	37·64
Magnesium	7·69
Fluorine	11·35
	100·00

Or, as the analysis must give it—

Phosphoric acid	43·32
Magnesia	50·18
Fluorine	11·35
	104·85

The formula $Mg\,Fl + Mg^4OP^5$ contains an uncommon saturating point, and requires only 38·5 phosphoric acid, hence less than it appeared by experiment, and 55·73 magnesia; it is, therefore, decidedly incorrect.

The formula proposed by myself was long ago considered by Kobell as answering to the Wagnerite,* and was later admitted likewise by Fuchs;† it could, however, be based only on a mere supposition, as the analysis of Fuchs gave an amount of fluorine totally different from the above.

The wagnerite is by no means a talc-spatile, if we are warranted in thus expressing ourselves, that is, it is not $MgOFlO^5 + 3MgP$, because then it is to contain 6 per cent. more phosphoric acid and only 4·33 per cent. fluorine.

* *Charakteristik der Mineralien*, i., p. 106.

† *Naturgeschichte des Mineralreichs, nachden Vorlesungen*, Von Fuchs. Kempten. 1842, p. 171.

Before leaving the analysis of the wagnerite, I will add some remarks on the determination of the fluorine. It was already stated in the foregoing, that the determination attempted by the resolution with sulphuric acid is not to be relied upon, as an admixture of silica is modifying the result. But even if the wagnerite is entirely free from silica, in this, as in all other cases, the sulphuric acid, could, á priori, not serve in the calculation of the amount of fluorine, as it cannot be ascertained whether the fluorine be neutral, and the said calculation being possible in the latter case only.

Fuchs obtained from 100 parts wagnerite, 46·66 sulphate of magnesia = 30·78 sulphuric acid, and these are equivalent to 14·36 fluorine.

On the other hand, after the extraction of the sulphate of magnesia, the residue left amounted to 78 per cent., in a manner that 22 per cent. fluoride of magnesium were present. If the salt is neutral = Mg Fl, it contains 13·11 fluorine.

It therefore appears, that the analysis of Fuchs, though merely approximative, yet nearly expresses the real amount of fluorine, and the 6·5 per cent. hydrofluoric acid could only be given by an error of calculation. This analysis might, at the same time, serve as a new proof of the skill of the highly deserving author, if such were wanted at all.

On the specimen of the wagnerite, communicated to me for analysis, were found several reddish, tarnished, and soft portions, which were probably crumbled wagnerite, chiefly consisting of silica. The investigation gave—

Silica	93·81
Phosphoric acid	1·87
Alumina and oxide of iron	1·41
Lime	2·58
Magnesia	1·49
	101·16

PHOSPHATE OF MAGNESIA.

According to Graham's* investigation, the crystalline salt obtained by mixing diluted solutions of sulphate of magnesia and phosphate of soda, 15 atoms of water, of which 8 atoms are driven off at 212°F. I have investigated the gelatinous precipitate, which is thrown down from concentrated solutions of these salts, and which is distinguished from the above crystallised salt by its less amount of water. After having been washed when cold, and dried in the air, then 1·558 allowed to stand for a long time with sulphuric acid, they lost 0·113, and on calcining again 0·451, together 0·564 = 36·2 per cent. The determination of the magnesia, by calcining the residue with carbonate of soda, &c., showed, that the salt was Mg^2OPO^5, and the amount almost exactly corresponding to 7 atoms, which lead to assume 35·85 per cent. This salt has, therefore, the same composition as the crystallised one which was dried at 212°F.

If the liquid, which was separated from the precipitated salt by filtration is allowed to stand, crystals are formed of $Mg^2OPO^5 + 15HO$, whereas it becomes turbid on being heated, and deposits a flaky body, which is probably the following tribasic combination.

Riffault states that, the phosphate of magnesia on being boiled with water, is resolved into an insoluble basic salt, and a soluble acid salt.

To examine this, a portion of the above described precipitate, after being washed, was repeatedly boiled with water, the whole filtered, and the residue washed when still hot. The filtered mass was acid, contained much phosphoric acid, but only very little magnesia, and no crystalline salt could be obtained from it. The phosphate of magnesia, therefore, on being boiled, is resolved into a basic salt and free acid.

The first, after being dried, forms a white powder, and is distinguished from the bibasic salt only by the circumstance that, on calcining with a silver salt, it is precipitated with a yellow color.

The substance employed in the analysis was air-dried. 1·087 lost on calcining 0·267, and the remainder on being calcined with carbonate of soda and washed, gave 0·37 magnesia. Hence the salt is equal $Mg^2P + 5H$ and contains—

	By experiment.	From calculation.
Magnesia	33·98	34.76
Phosphoric acid		40·02
Water	24·56	25·22
		100·*

(To be continued.)

GAS PIPETTE.†

BY. DR. ETTLING.

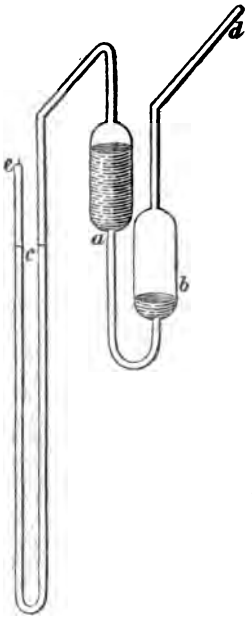
This instrument, which, according to the annexed diagram, may be manufactured by a glass-blower to any desired size, may be

* *Annalen der Pharmacie*, vol. xxix., p. 24.

* This matter has recently been examined with nearly the same result by Schaffner, (*Annalen der Chemie und Pharm.*, vol. l., p. 145,) but this chemist found in the last mentioned salt, 5 and not 7 atoms of water, of which 5 atoms may be driven off at 392°F.

† Liebig's *Annalen*, i., 1845.

welcome to chemists and persons who have much to deal with pneumatical operations. It renders the larger pneumatic troughs unnecessary, and offers the advantage to remove desired quantities of gases from graduated tubes or bells, &c., placed in cylinders, without bringing them in a horizontal position, and to translate gases into other vessels, requiring at the same time very little liquid to confine the gas. Previous to using the instrument, the cylinder *a* is filled with the liquid which is to confine the gas by dipping the branch *c* into the same liquid and sucking at the branch *d*; the aperture of the branch *e* is then introduced into the tube from which gas is to be taken, and sucked again at *d*, by this the liquid confining the gas passes from *a* into *b*, and is replaced by gas, which is confined in it, if the aperture *e* is immersed in the confining liquid, and left therein for any desired length of time. By blowing in air at *d*, the whole gas is expelled again, and whenever desired, especially if the opening *e* is somewhat straight, portions of it may be measured in bells, endiometers, &c. If the confining liquid be mercury, it is somewhat difficult to expel the gas, because a high column of mercury must then be depressed; it may, however, be diminished to half inch, if, on blowing in air, the tube filled with mercury, together with the branch of the pipette immersed in it, are raised until they are near the surface of the mercury.



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ON THE ATOMIC WEIGHT OF SIMPLE BODIES.*

CALCIUM.

In the latter volumes of these annals we have communicated the results obtained by Dumas,†, then Erdmann and Marchand,‡ and, finally, by Berzelius,§ relative to the determination of the atomic weight of calcium. Dumas, as well as Erdmann and Marchand, found, by experiment, the number 250. Whereas Berzelius gives as the mean of five severally instituted experiments 351·9424.

In consequence of Berzelius's critical remarks, Erdmann and Marchand|| have lately communicated the results of a new series of investigations; they first remove the objection, that the carbonate of lime prepared by themselves was impregnated with magnesia; the chloride of calcium employed, as is commonly the case with this body when an article of commerce, being a collateral produce of the preparation of ammonia, was, therefore, in consequence of the excess of lime with which it has been in contact, in its raw state, already completely free from magnesia.

Owing to the difficulties standing in their way on preparing lime completely free from chlorine and water, they resolved upon giving up the scheme of operating with artificially-prepared carbonate of lime, and employed the natural one.

As the purest double spar is never an absolutely pure carbonate of lime, they first endeavored to ascertain the precise quantity of the admixtures which yielded no carbonic acid. This quantity, which chiefly consisted of carbonate of protoxide of iron, and of carbonate of protoxide of manganese, amounted, as a mean of two experiments, to 0·040 per cent.

The impurities not being subtracted, in six experiments the following per centage of lime were obtained:—

I.	II.	III.	IV.	V.	VI.
55·997	56·022	56·031	56·032	56·044	56·042

As a mean of these experiments, the carbonate of lime is found to be composed of—

* Liebig's *Jahresbericht*, Giessen, 12th April, 1845.

† Leibig's *Annalen*, xliv., p. 216.

‡ *Ibid.*

§ *Ibid.*, p. 241.

|| *Journal für Practishe Chemie*, xxxi., p. 257.

Lime	56·028
Carbonic acid ..	43·972
	100·000

Hence the atomic weight of calcium, taking the carbonic acid to be 275, will be 250·39, which but little differs from 200, the number formerly obtained.

It may now be proper to mention a work published in 1841 by S. Baup.* To control the atomic weight of calcium, Baup analysed a number of very pure, well crystallised organic salts of lime.

It resulted that, if the atomic weight of carbon is admitted to be 75, the quantity of lime found leads to assume the atomic weight of lime to be 350, as it appears from the following table:—

Quininate of lime ..	9·396	9·384	349·543
Mateinate of lime ..	15·556	15·550	349·865
Fumarate of lime ..	26·923	26·921	349·970
Citraconate of lime..	25·000	24·999	349·986
Itaconate of lime....	30·107	30·085	349·744
Aconitate of lime ..	29·474	29·470	349·952
		Mean....	349·845

To determine the equivalent of carbon, Baup made similar experiments with a series of organic silver salts, which lead almost exactly to the number of 75.

IRON.

Erdmann and Marchand* have recently examined the numbers 348·72 obtained by Ivanberg and Norlin, and 349·523 by Berzelius.† Their method consisted in the reduction of the oxide of iron by hydrogen.

The oxide of iron employed in the first series of experiments was prepared from the oxalate of protoxide of iron. Harpsichord wires were heated with pure sulphur, the obtained sulphuret of iron dissolved in distilled sulphuric acid, and the sulphate of protoxide of iron repeatedly recrystallised, previous to its being treated with oxalic acid. The precipitated salt was, by calcining in shallow platinum vessels, finally converted into oxide of iron. In some experiments, previous to its being employed, it was moistened with nitric acid and calcined.

In the second series of experiments was employed an oxide prepared from the oxalate of protoxide of iron, which was obtained by reducing the acid oxalate of iron in the rays of the sun. The washed salt was calcined, moistened with nitric acid, and recalcined.

The hydrogen by which the reduction was operated was successively made to pass a solution of oxide of lead in potassa, a solution of corrosive, concentrated sulphuric acid, a solution of sulphate of silver, and, finally, dry caustic potassa. It was completely odorless, and burned with an invisible flame. The reduction was operated over a spirit lamp; the atomic weight of the oxide of iron, as well as of the iron was determined in vacuo.

FIRST SERIES.

35·2576 grammes oxide of iron gave	24·685	iron	=70·013	per cent.
24·2360 ,, ,,	16·956	,,	=69·962	,,
29·2576 ,, ,,	20·4752	,,	=69·979	,,
16·5201 ,, ,,	11·56975	,,	=70·030	,,
27·2189 ,, ,,	19·0470	,,	=67·977	,,

SECOND SERIES.

14·251 grammes oxide of iron gave	9·982	iron	=70·044	per cent.
9·155 ,, ,,	6·410	,,	=70·015	,,
10·527 ,, ,,	7·37475	,,	=70·055	,,

The mean of these numbers is 70·09 per cent. iron, by which the atomic weight of iron appears to be 350·1 or 28·008 H=1.

On comparing this number with that obtained by Svanberg and Norlin, and by Berzelius, we find it coinciding as near as possible. The mean of all the series of the experiments gives for the atomic weight of iron, the number 350 or 28 H=1.

COPPER.

The atomic weight of copper was determined by Berzelius, by reducing the oxide of copper with hydrogen, and found to be 395·695. Erdmann and Marchand have repeated this experiment on somewhat a larger scale. For this purpose they purified the sulphate of copper by recrystallising it from a solution of water impregnated with nitric acid, and then from pure water, until no reagent could detect any impurities, they dissolved the galvanoplastically precipitated cop-

* *Journ. für Pract. Chemie*, vol. xxxiii., p. 1.

† *Annalen der Chemie und Pharmacie*, vol. l., p. 452.

* *Bibl. Univers. de Genève*, June, 1842.

per in pure nitric acid, and resolved the nitrate of copper obtained in a platinum crucible over a spirit lamp.

In the first two experiments, the oxide of copper placed in a glass tube open at both ends, was calcined over a spirit lamp, a current of dry air being at the same time passed over it. The weighed oxide was then reduced over a spirit lamp by pure hydrogen, (this gas traversed an alkaline solution of oxide of lead, then sulphuric acid, and finally a tube containing hydrate of potassa). The water obtained was, after careful examination, found to be completely free from any amount of nitric acid. After having allowed to cool the copper in the hydrogen current, dry air was passed through it, the tube finally weighed, and the weights resulting reduced to their amount in vacuô, the specific gravity of the oxide of copper was then taken as 6·4, that of copper as 8·9.

The two last experiments differ from the first by a difference in the glass tube, which was drawn out into a point at one extremity and had a small stopcock attached to it at the other. After the calcination the point was melted off, the air withdrawn from the tube, and after having closed it by the cock, the weight ascertained.

The following numbers were obtained, bearing in mind that, the numbers obtained in the first two experiments are reduced to the vacuô. In the latter two, as has been remarked, the weight was determined in vacuô.

	Grammes oxide of copper.	Grammes of copper.	Per cent.	Atomic weight.
I.	63·8962	51·0391	79·878	396·9
II.	65·159	52·0363	79·860	396·5
III.	60·2878	48·1540	79·873	396·8
IV.	46·2700	36·9449	79·845	396·2

A mean of all experiments gives the composition of oxide of copper as—

Copper	79·86
Oxygen	20·14
	100·00

which differs in the second decimal number only from that given by Berzelius. The atomic weight derived from this is 396·6, hence only by one unit greater than that of Berzelius. The equivalent of copper is, therefore, far from being a multiple of the equivalent of hydrogen.

They are in the proportion as 31·7 to 1.

MERCURY.

After an experiment instituted by Sefström, Berzelius calculated the atomic weight of mercury, to be 1265·823. Sefström found in three experiments, that 100 parts of mercury to be converted into oxide, require from 7·89, 7·9 to 7·97 parts of oxygen. To these numbers correspond the atomic weights, 1267·4, 1265·8, and 1254·7. Sefström having considered the number 7·9 as the most correct one, the above atomic weight was established, though the mean of these experiments gives 1259·7.

Erdmann and Marchand* repeated Sefström's experiment. They found that, if in the analysis of oxide of mercury, its decomposition is operated by applying heat, this induces an error. As on calcining the oxide of mercury, almost invariably a tenacious, syrupy and tarnished metal pass over into the recipient. Erdmann and Marchand attributed this phenomenon at first to an amount of nitric acid contained in the oxide employed; they afterwards, however, ascertained, that this is merely owing to a superficial oxidation which the mercury undergoes in the atmosphere of oxygen of the colder portions of the apparatus.

Marchand and Erdmann then endeavored to avoid this source of error in the following manner. They operated the solution in a tube of Bohemian glass, three feet in length, which at its anterior extremity was drawn out in a point from 9 to 10 inches long, and bent downwards. Into this tube was first introduced some reduced copper turnings, these were followed by a layer of calcined sugar-coal, five or six inches long, then came the oxide, previously heated by a spirit lamp in another tube, until completely resolved, and then by a dry current of air freed from any vestige of mercury. The tube prepared in that manner was then shaken, as is done in an organic analysis, and placed on a Liebig's combustion furnace. The posterior extremity was first connected by a caoutchouc tube with a chloride of calcium tube, to this was attached Liebig's bulbous apparatus, filled with sulphuric acid, and finally, the latter made to communicate with a large gasometer. The apparatus having been arranged in that manner, the anterior point was introduced into a receiver, destined to collect the mercury. The receiver consisted of a tube in which 4 contiguous bulbs had been blown out; behind the first bulb, the tube was bent upwards in an angle of 45°, and another glass tube 5 inches long, about three-quarters of an inch wide was soldered to it, which was filled with gold leaves.

* *Journal für Pract. Chemie.*, vol. xxxi., p. 392.

In this manner the mercurial vapors were completely prevented from escaping.

On operating, heat was applied from the anterior towards the posterior portion, and at the same time a dry current of carbonic acid passed through the apparatus. The whole oxygen resulting from the decomposition of the oxide was immediately employed in the oxidation of the sugar charcoal, and the mercury distilling in the current of carbonic acid arrived in the receiver endowed with a bright lustre. After the termination of the experiment, the carbonic acid and the water formed on burning the charcoal were removed by a dry current of air, the point of the tube cut off, and weighed together with the receiver.

In this manner five experiments have been executed, in which the following numbers were obtained, and reduced to the vacuous state :—

	Grammes oxide of mercury.	Grammes mercury.	Per cent.	Atomic weight.
I.	82·0079	75·9347	92·594	1250·3
II.	51·032	47·2538	92·596	1250·7
III.	84·4905	78·2501	92·604	1252·1
IV.	44·6283	41·3252	92·598	1251·1
V.	118·4066	109·6408	92·596	1250·6

The mean of all the experiments gives the composition of the oxide of mercury.

Mercury	92·597
Oxygen	7·403
	100·000

And the atomic weight of mercury = 1250·9, that is nearly 100 times the amount of the equivalents of hydrogen, because

12·5 : 1250·9 = 1 : 100·07.

On subtracting the third experiment, in which graphite was employed instead of the sugar charcoal, which could quickly enough convert the whole oxygen into carbonic acid, wherefore the metal distilled over turbid and tarnished; the atomic weight appears to be 1250·6, which still nearer approaches to the proportion of the two equivalents, namely, 1 : 100·04.

SULPHUR.

The atomic weight of sulphur was found to be, according to Berzelius, 201·165; this he ascertained by dissolving a weighed quantity of lead in pure nitric acid, and treating the solution with sulphuric acid, evaporating and weighing the sulphate of lead after calcination.

Erdmann and Marchand have also revised the equivalents of sulphate of lead. They first followed the method chalked out by Berzelius; they could not, however, succeed in obtaining with the sulphate of lead invariably the same weight, as this salt on continued calcination loses one portion of its sulphuric acid. This may easily be recognised if the calcined salt is treated with nitric acid. From the limpid nitrous solution, by an addition of diluted sulphuric acid, sulphate of lead will be thrown down.

Erdmann and Marchand preferred, therefore, to determine the equivalent of sulphur by employing cinnabar. The cinnabar of commerce, after sublimation, sometimes leaves behind traces of a ferruginous volatile substance; in addition to which, free sulphur may be admixed. The cinnabar employed in the experiment was then uninterruptedly heated in an air-bath until the sulphur was completely driven off, and then three times sublimed.

The decomposition was undertaken in an apparatus similar to that employed in the determination of the equivalent of mercury. But the cinnabar was carefully mixed with the metallic copper, which was done partly, previous to introducing it into the tube, in a mortar, and partly in the tube itself, by a steel wire shaped in the manner of a screw. The decomposition was brought about with the greatest ease.

The numbers obtained and reduced to the vacuum were the following :—

	Gr. cinnabar.	Gr. mercury.	Per ct.
I.	34·3568	29·6207	86·212
II.	24·8278	21·40295	86·205
III.	37·2177	32·08416	86·206
IV.	80·7641	69·6372	86·222

The mean of all the experiments gives the composition of the cinnabar—

Mercury	86·211
Sulphur	13·789
	100·000

If the atomic weight of the mercury after the above-stated determination is taken to be 1250·9, one equivalent of sulphur is 200·07. By the atomic weight 1250·6, which seems to be the most probable, the number of sulphur appears to be 200·026, or almost the double of oxygen, or 16 times the number of the equivalent of hydrogen.

—nt—

ROASTING OF MINERALS.*

Rousseau suggests, instead of reducing the sulphurets by roasting in the manner hitherto

* Liebig's *Jahresbericht*, April 12, 1845.

adopted, to expose them at a high temperature, by which the oxidation of the sulphur is not only completely and speedily brought about, but the great advantage likewise derived, that the metal remains in the state of an oxide, entirely free from sulphur. Sulphuret of copper, iron, and antimony, give up the whole of their sulphur as sulphurous acid. Oxide of antimony was, in this manner, already prepared by Ruolz, in the form of a beautifully white extremely fine powder.

—nt—

ON THE MANUFACTURE OF SULPHURIC ACID.*

ACCORDING to Peligot, the process of the production of sulphuric acid in the leaden chambers is as follows: — Nitrogen, on coming in contact with air, hyponitric acid is formed, which, in the presence of water, is resolved into nitric and nitrous acids; the nitric thus produced is, by means of sulphurous acid, converted into hyponitric acid, sulphuric acid being produced in the presence of a large quantity of water, the nitrous acid formed in that manner is converted into nitric acid, and oxide of nitrogen, which acts again on fresh quantities of oxygen and sulphurous acid.

He further states, that, at the common temperature, strong nitric acid may, by sulphurous acid, under the formation of sulphuric acid, be completely converted into oxide of nitrogen, whereas with diluted nitric acid this takes place between 140° and 176°F. It follows from this, that sulphuric acid produced by an excess of sulphurous acid must be free from all nitrogenous combinations; to confirm this by experiments, he tried to separate these combinations from sulphuric acid by directing into it a current of sulphurous acid. He could not succeed in this with the concentrated acid; but, diluted with an equal bulk of water, an acid was obtained from it, which contained no nitrogenous combinations.

Peligot maintains that the English sulphuric acid contains nitric acid, as it must be volatilised by evaporation; it is, however, probable that the combination analysed by Provostaye, consisting of nitric and sulphurous acids, is contained therein, as this may remain at 572°F.

According to Peligot, sulphuric acid ought always to take care to direct an excess of sulphurous acid into the leaden chambers. Payen states, that a great number of manufacturers follow the directions just mentioned, with great advantage, and obtain sulphuric acid free from nitrogenous combinations.

—nt—

AMOUNT OF WATER CONTAINED IN THE SULPHURIC ACID OF COMMERCE.*

ACCORDING to the investigations of Wittstein,† the amount of water in the English sulphuric acid of commerce, is at present greater than it used to be, its specific gravity is commonly 1·844, which answers to the formula: $2SO^3 + 3HO$, yet it is customary to consider this as the first hydrate, $SO^3 + HO$ (1·852 specific gravity). The acid entering the commerce under the name of the nordhausen sulphuric acid, instead of one-fourth now generally contains only one-ninth of anhydrous sulphuric acid.

—nt—

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

GUANO, &c.†

I FARM a stiff soil, unsuited for the turnip husbandry, but, by dint of care and attention, I contrive to raise pretty good crops at times; by ploughing in a large supply of manure in winter, the soil is greatly ameliorated, and of course more easily worked in spring. When I sow the turnip, I frequently have only to apply a proportion of foreign manure, as 3 cwt. of guano per acre. A friend of mine says that 2 cwt. of guano, and 2 cwt. of Dawson's carbon, are equal to 3 cwt. of guano, the ammonia being fixed by the carbon; is he theoretically correct? would it not be adviseable to mix the above quantity of guano with only 1 cwt. of carbon, and 1 ditto of gypsum.—[Carbon does not *fix* ammonia, properly speaking; it retards its evaporation indeed; but the am-

* Liebig's *Jahresbericht*, Giessen, 12th April, 1845.

† *Agricultural Gazette*, *April* 26, 1845.

* Liebig's *Jahresbericht*, April 12, 1845.

† Buchner's *Repertorium*, XXXV., p. 350.

monia, which is condensed within its pores, is still volatile, and will gradually escape. The *fixation* of ammonia consists in its assuming a non-volatile condition, by union with an acid. As, for instance, when carbonate of ammonia and sulphate of lime (gypsum) mutually decompose each other (both being in solution), and result in carbonate of lime and sulphate of ammonia. We should prefer the last-named mixture.]

There is often great difficulty in getting a braird of Swedish turnip on clay soils; but when once secured in good time after being sown, and early in the season, the crop is generally very good. I have tried washing and steeping the seeds in pure water, rolling down the drills with a light wooden roller, all very useful and beneficial in their way; but there is a great desideratum wanting in agriculture—a steep which will ensure a speedy germination, and rapid growth of the seed of the Swedish turnip in the early stages of its growth, for unless that is secured, and if the plants are long in being fit for singling, no amount of manure will ensure a good crop. The above remarks have reference to land that has been thoroughly drained, ploughed from 12 to 14 inches deep, thoroughly cleaned, beautifully worked, and liberally manured; and be it remarked, that in certain parts of the fields, where the land was naturally better adapted for the turnip husbandry, a great crop rewarded the outlay. The foregoing question is well worthy of your attention, and the evil is loudly complained of in all districts where farmers are obliged to cultivate this invaluable crop on land which, even after thorough drainage, and deep tillage, is but ill-fitted for the same.

SULPHATE OF LIME, OR GYPSUM.*

THIS substance, which is a compound of sulphuric acid and lime, is found in abundant quantities in a native state, both for the purpose of agriculture and the arts; and it has been recommended for the same purpose as the muriate of lime—viz., the fixing of ammonia; but is infinitely inferior for that purpose, in consequence of its difficult solubility—at the temperature of 60°, one part of gypsum only being soluble in 460 parts of water, to obtain which, however, the gypsum must be finely divided, and macerated for a great length of time. Its more obvious use in agriculture is, however, as a direct food to certain plants, either as supplying the sulphate of lime, or, by its decomposition, affording the sulphur necessary to the due development of certain plants, such as the crucifera, &c. The following table, extracted from *Sprengel's Analyses*, shows the quantity of lime and magnesia, sulphuric and phosphoric acids, in 100,000 parts in several of our ordinary crops:—

	Lime.	Magnesia.	Sulphuric Acid.	Phosphoric Acid.
Wheat	96	69	50	406
Barley	106	180	59	210
Oats	86	67	35	70
Potatoes	33	32	54	40
Cabbage	1822	202	774	436
Swedish Turnips	835	282	890	408
Wheat Straw	240	32	37	170
Barley ditto	554	76	118	160
Oat ditto	152	22	79	12
Red Clover	584	70	94	138
Beet	285	133	123	167
Turnips	127	22	41	73

The above table may not be strictly accurate, but I believe it approximates to the truth, and certainly agrees with my own practical observations respecting the exhausting powers of different crops on various soils. Cabbages are known to be one of the most scourging crops that are grown, and we see, from the above table, that both it and Swedes require a large amount of sulphate of lime; the next is red clover, and the application of gypsum to the clover crop, on lands sparingly endowed with this substance, has been so repeatedly treated of, that it is an act of supererogation to enlarge on its usefulness.

BONES AND SULPHURIC ACID.*

A LETTER from Mr. Spooner, of Southampton, was read, in which he stated that last year he had used bones and sulphuric acid as compost for turnips. He had prepared it by placing 1¼ bushels of finely-ground bones in a tub, with half their weight in acid, diluted with four times the quantity of cold water; after some hours a few bushels of fine mould and some coal-ashes were added, so as to make the whole amount to 15 bushels of compost. This compost was used in three days after its preparation, but, being damp, did not run so well through the drill as if time had been given for its drying. The mixture was applied at the rate of little more than two bushels per acre; and, as Mr.

* *Journal of the Highland Agricultural Society of Scotland.*

* *Agricultural Gazette*, April 26, 1845.

Spooner states, successfully rivalled 16 bushels of bones applied to an acre of superior quality.

Mr. Slaney remarked, that in Shropshire they mixed the acid and bones in a stone trough, and, having stirred them well together, continued to add water till the whole was in a proper state for use. Excellent crops were obtained when the mixture could be equally distributed; but no common farmer would be able to employ it to advantage upon an extended plan until some mode for such regular distribution could be devised.

Colonel Challoner had seen on the Duke of Richmond's estates, in Scotland, a most simple and excellent contrivance for effecting that purpose, namely, a vessel suspended like a compass, in such a manner, on a pivot, as to preserve its horizontal balance in every position of the wheels and axle supporting it; and so contrived that the mixture could be distributed from it exactly over the drills. The price, he believed, was from 8*l.* to 12*l.*

Mr. Thompson thought that the difficulty was not confined simply to the distribution of the mixture by means of the water-cart, however excellently it might be invented for the purpose required, but extended to the imperfect reduction of the bones when the application was made. He had found particles of bone remain in the spout of the rose after he had increased the apertures by degrees to a considerable extent; even when he had employed whaledust (or coarse sawdust from the large bones of the whale), instead of fragments of common bone, as the basis of his operations: this sawdust being found after a fortnight's maceration to be so imperfectly dissolved, as to impede the even distribution of the mixture by the rose of the water-cart. He had applied the preparation with success in the state of compost, at the rate of four bushels of bone per acre.

APPLICATION OF ELECTRICITY TO THE GROWTH OF PLANTS.*

It is now nearly two years since I sent to the *Gardeners' Chronicle* a paper upon glazed pots, in which I hinted at the wonderful effects of electricity upon vegetation. Since that time my attention has been directed towards the all-pervading fluid by many incidents, and I would before now have communicated to your columns my opinions thereon, had not your "We wish this correspondent had left out his speculations about electricity," &c., &c., (see page 541, 1842), prevented me; indeed, I consider from the rather ungracious manner in which you seem generally to receive communications touching upon this subject, that it is not in great favor with you. Now, why should we not try all things, and hold fast that which is true? I have long been of opinion that the right application of electricity to the growth of plants, is the only true way of arriving at perfection in their cultivation; for the more I study the subject the more I am convinced that my opinion is well-founded: indeed, I have long looked upon all manures as mere conductors of electricity to the roots, and that just in proportion as they possess this quality are they good or bad. About two years ago I mentioned to some scientific gentleman that I believed the day was not far distant when all such dirty and expensive appliances would be superseded by electricity, and that I looked forward to a time when all laborious operations would be performed by its agency. I need not say how much pleasure the experiment of Dr. Forster gives me. To see that others are putting to the test of practice, what we have long deemed true in theory, is certainly calculated to strengthen our opinions; the more especially as there was no communion between the parties. Three years ago I commenced to lay out a large garden (27 acres), all of which I intended to have cultivated by electricity, procuring the fluid by wires suspended in the neighboring woods and trees; but unforeseen circumstances put a stop to my operations, and it is now being finished in the old "use and won't" way. That electricity might be advantageously used in raising seeds and propagating cuttings does not, with me, rest upon theory; as far as I have been able to make experiments, they have been highly successful. I might here quote largely from Liebig and others, in support of the opinions advanced, although these writers do not themselves always perceive the *rationale* of what they write about; or if they perceive it are afraid from some cause or other openly to declare it. I hope that others may turn their attention to this subject, and communicate the result.

A READER FROM THE COMMENCEMENT.

[It is perfectly true that electricity *is* "out of favor" with us, as our correspondent puts it. Not that we undervalue the importance of this all-pervading force; for who that knows what it is can do that? But we do not like to see our columns filled with crude speculations about what is or is not possible. For experiments skilfully conducted we should be only too happy to find room as well as for theories founded upon them. But experiment must *precede* speculation; till that is done

* *Agricultural Gazette.*

we regard disquisitions about the application of electricity to cultivation premature, to say the least of them. And we may appeal to the foregoing letter as a justification of this view: for what kind of information does it really convey, except that the writer holds certain opinions about electricity, and that he intended to cultivate twenty-seven acres by means of it, but—did not?]

EFFECTS AND USE OF FLAX-WATER.*

I was often struck with astonishment after rain, in the months of September and October, to see the great number of large trout dead in the water-course to my mill, from the effects of the water which had been let off from the several pits in the neighborhood where flax had been soaked. I am glad to find that even the water in which it is steeped can be turned to a good account. Some of the members of the Belfast Society have been trying experiments, and find that it can be returned to the land in the shape of manure. I should think bog-earth thrown into such water would be, when taken out, valuable.

Having given an outline of the plan of operation by which I and others have been successful in cultivating flax, and having watched the mode of management pursued by others in Ireland who had year after year prime flax, I cannot imagine how men can be so prejudiced as to assert that the growing of flax is a lottery. I am aware that it is not every man who wants to do so can grow it of fine quality; he must have time to bring his ground into a proper state of cultivation. Added to this, as there are a great number of farmers uninformed of the value and variety in quality of flax, they must have Belgian teachers, or be guided by the Belgian system of management, before they can compete with our foreign neighbors; however, I do not despair of their success when knowing the result of several experiments made by gentlemen in Norfolk who have produced fibre equal to the best Belgian, and I have pleasure in informing them that I have been lately favored with letters from some of the most extensive flax spinners in Yorkshire and Lancashire, promising to encourage and give a preference to their own country flax the moment they can do so without actual loss. No doubt these gentlemen feel, as landholders should do, the hardship of being obliged to pay so largely for flax to a people that tax the yarns made from their own flax 40 to 60 per cent., although they must have it for their wants.

As England is second to no country in the world in her manufactures or mechanical skill, it is to be hoped that the owners of the soil will not be the only class to follow, or hesitate to lead in the march of improvement, and I trust they will see that flax may be profitably grown both for the feeding qualities of the seed and for the fibre, although not in the same ratio, and that there is a great chance (if flax is cultivated with success) of many of the Manchester cotton spinners following the example set them by the Belfast cotton spinners, whose mills have been, with two exceptions, all turned to flax spinning within the last 12 years. The first flax mill was built in 1828, in that town, out of the ruins of a burnt-down cotton mill, and at present there are above 30 flax mills in Belfast and the neighborhood, at full work, and five new ones about to commence work, not taking into account the several mills in the counties of Armagh, Derry, Dublin, and in the town of Drogheda. Whilst many thousands of hands are employed in the flax spinning factories, and in the weaving into linen what their neighboring farmers and landlords have produced; here is a connecting link that cannot be broken between agriculture and commerce. The Belfast flax spinners are the only protective society the farmers in the north of Ireland require. As statements without proof are often considered erroneous, I shall conclude my observations on this subject by quoting a proof, which I took from the *Times* newspaper in July, 1843. A deputation from the flax spinners in Ireland waited on ministers at Whitehall, and made the following statement, because they considered their trade was going to be much injured by a free importation of machinery to a country that advanced the duty from what it was, 10 per cent. on linens and yarns in 1840, to what it then was and now is, 40 to 60 per cent.:—

We (they say) only exported from Ireland Yarns in 1832 amounting to		£5,000
Ditto in 1841	,,	1,700,000
Ditto in 1842	,,	1,000,000
Ditto in 1843	,,	1,200,000

Here is an increase from 1832, from 5,000*l.* to 1,200,000*l.* sterling, exported in 1843 up to July; now if proof be wanted to convince landholders of the increased consumption of flax in Ireland, and the extraordinary benefit the northern district in that country must derive from growing as it now does what is spun, to meet an immense increase of exportation, such as the above

* *Agricultural Gazette.*

figures prove to have taken place these last 12 years, I hope I may be allowed to say, I have brought forward facts worth the serious and conclusive consideration of the landed proprietors of this kingdom.

J. H. Dickson,
29, Broad-street-buildings, London.

P.S. I prefer Riga seed; because, from Riga flax being coarse, and seldom worth more than 40*l.* to 50*l.* per ton, I am of opinion that the growers there place more value on the seed than they do on the fibre, and therefore allow the plant to become full ripe, and its seeds become then better for sowing.

BONES AND SULPHURIC ACID.*

Mr. Kendle favored the council with the following communication:—

"I perceive the trials which were made last year with bones and sulphuric acid, are creating more and more interest as the turnip season approaches; I have great pleasure, therefore, in forwarding you, and shall feel obliged by your laying before the council, an experiment I made upon an acre of fen-land for coleseed. I put two bushels of bone-dust, not ground very fine, into a tub, with 70 lbs. of acid, and added 140 lbs. of water; it was all well stirred together, remained in the tub 24 hours, and then mixed with cinder-ashes in sufficient quantity to make it drill. I tried it by the side of bones at the rate of 8 bushels to the acre, mixed also with ashes; the superiority of the former was most striking, more particularly during the two first months. I may add, I left a few drills without any manure, which of course were throughout the season conspicuously bad. In cost there was little saving, but perhaps I ought not to have given 2*d* a pound for the acid. I was so well pleased with the result of last year's trial, I am quite disposed to use them again; it is particularly of a cheap rate. I should not have troubled you with this communication, but not having seen an experiment upon the same description of land upon which bones are applied with greater success, I think, than almost any other, I thought perhaps it might be found acceptable. I shall be happy to answer any questions, or give any other information in my power upon the subject, should it be required.

"C. J. Kendle.

"Fordham, Downham Market,
"May 3, 1845."

* Agricultural Society of England. Meeting of May 14, 1845.

THE INDIA-RUBBER TREE.

A correspondent of an American paper, writing from the Brazils, gives the following interesting particulars of the process of tapping the india-rubber or caoutchouc tree, and of manufacturing the gum into shoes and other things:—

"The caoutchouc tree grows, in general, to the height of 40 or 50 feet, without branches; then branching, runs up 15 feet higher. The leaf is about six inches long, thin, and shaped like that of a peach tree. The trees show their working by the number of knots, or bunches, made by tapping; and a singular fact is, that, like a cow, when most tapped, they give most milk or sap. As the time of operating is early day, before sunrise we were on foot. The blacks are first sent through the forest, armed with a quantity of soft clay and a small pickaxe. On coming to one of the trees, a portion of the soft clay is formed into a cup and stuck to the trunk. The black then striking his pick over the cup, the sap oozes out slowly; a tree giving daily about a gill. The tapper continues in this way, tapping perhaps fifty trees; when he returns, and with a jar, passing over the same ground, empties his cups. So by seven o'clock the blacks came in with their jars ready for working. The sap at this stage resembles milk in appearance, and somewhat in taste. It is also frequently drunk with perfect safety. If left standing now, it will curdle like milk, disengaging a watery substance like whey. Shoemakers now arrange themselves to form the gum. Seated in the shade, with a large pan of milk on one side, and on the other a fagon, in which is burned a nut peculiar to this country, emitting a dense smoke, the operator having his last, or form, held by a long stick or handle, previously besmeared with soft clay (in order to slip off the shoe when finished) holds it over the pan, and pouring on the milk until it is covered, sets the coating in the smoke; then giving it a second coat, repeats the smoking; and so on with a third and fourth, until the shoe is of the required thickness, averaging from six to twelve coats. When finished, the shoes on the forms are placed in the sun the remainder of the day to drip. Next day, if required, they may be figured, being so soft that any impression will be indelibly received. The natives are very dexterous in this work. With a quill and a sharp-pointed stick they will produce finely lined leaves and flowers, such as you may have seen on the shoes, in an incredibly short space of time. After remaining on the forms two or three days, the shoes are cut open on the top, allowing the last to slip out. They are then tied together and slung on poles,

ready for the market. There peddars and Jews trade for them with the country people; and in lots of 1,000 or more they are again sold to the merchants, who have them stuffed with straw, and packed in boxes to export, in which state they are received in the United States. In the same manner any shape may be manufactured. Thus toys are made over clay forms. After drying, the clay is broken and extracted. Bottles, &c., in the same way. According as the gum grows older, it becomes darker in color and more tough. The number of caoutchouc trees in the province is countless. In some parts whole forests of them exist, and they are frequently cut down for firewood. Although the trees exist in Mexico and the East Indies, there appears to be no importation into the United States from these places. The reason, I suppose, must be the want of that prolificness found in them here. The caoutchouc tree may be worked all the year; but generally in the wet seasons they have rest, owing to the flooded state of the woods; and, the milk being watery, requires more to manufacture the same article than in the dry season."

ON TOP-DRESSING WHEAT.*

EXPERIMENT on the actual and comparative effects upon a wheat crop grown on pea-stubble, of Potter's guano, natural guano, and rape-dust, as a winter application; also on the effect of nitrate of soda as a top-dressing for spring-sick wheat.

Details. — Soil — very poor limestone, worth 12s. per acre. Prior crops — Swede turnips (manure); barley (rape-dust); peas (manure). At the opposite end of this field the experiment on peas was made in 1842; the soil at that end, however, is of much better qualities (*vide* samples) than this now experimented on.

Management. — Ribbed and sown with yellow chaff wheat, Nov. 10, and manure as follows:—

Section 1.				Section 2.
1. Potter's Guano.	2. Guano.	3. Nothing.	4. Rape-dust.	5. Rape-dust and Nitrate of Soda.

Section 1.	1.	½ acre	Potter's guano, ½ cwt.,	sown in the furrow with the seed,	Nov. 10.
	2.	do.	Natural guano, ½ cwt.	ditto	ditto.
	3.	do.	Nothing.		
	4.	do.	Rape-dust, 3 bushels.	ditto	ditto.
Section 2.	5.	do.	Rape-dust, 3 bushels.	ditto	ditto.
			Nitrate of soda, 2 stones, applied as a top-dressing upon a plot which was perishing at spring,	applied	May 16.

Observations.—During winter most all of the plots looked well—better, indeed, than the wheat upon clover lea—the fine tilth of the stubble land promoting the quick growth of the plant. At the end of April, however, symptoms of a falling off began to show themselves, and, by the middle of May, Nos. 1 and 4 looked very sickly and patchy; No. 5 was more backward, but still was more healthy in color than Nos. 1 or 4; No. 2 was as yet pretty good, the color being still fresh. At this time another rood was measured off (alongside No. 4), which looked so yellow and sickly, that it seemed scarcely a skeleton of a crop, and was, as an additional experiment, dressed with ¼ cwt. of nitrate of soda; this plot, be it remembered, was manured with rape-dust, same as No. 4, and the remainder of the field (with exception of Nos. 1, 2, and 3), at the time the seed was sown; the nitrate being now used as a medicine intended to stimulate the failing plant. The effect of this application was magical. In 10 days the yellow patches were lost, and, in a week more, the only trace to be found of them was in the increased luxuriance of the same places, the tillage being applied most liberally on the diseased spots. Seeing this, the sickly patches throughout the whole field were now top-dressed, not as an experiment, but for the benefit of the crop. The results were equally astonishing with those upon No. 5; the worst places in the field soon being distinguishable as the best. Indeed, those who ran could read, and I had, in no few cases, to explain how the marvel had been accomplished; few imagining that, a short time before, these were actually the worst parts of the field, the questions generally asked being, why they were now so decidedly the best. During this time, the plots Nos. 1, 3, and 4, had improved a little; No. 2 was, however, decidedly best; but No. 5 had made most progress. In five weeks from the application of the nitrate, the wheat was full 6

* *Agricultural Gazette*, May 17, 1845.

inches higher, much thicker, and more luxuriant in color than No. 4. All marks were, therefore, unnecessary; setting aside the distinction in the hue, there was a perfect ridge or step formed by the superior height of the straw in No. 5, which was a sufficient line of demarcation. The ears put forth on No. 5 were also much larger than those on the unnitrated portions.

A talented chemist, who viewed the crop at this time, called the experiment a perfect illustration of agricultural pharmacy. Although, however, a tonic or an aperient may act on the digestive organs so as, in many cases, to improve health—though a stimulant may excite the languid nerves or a bitter rally the sickly appetite, they cannot in all cases succeed, nor can they remove organic infirmities; so, in this case, when harvest came, many of the ears in No. 5 were blighted. On the unnitrated portions, especially No. 4, these were, however, still more numerous. At this time, in Section 1, No. 2 (guano) looked decidedly best, Nos. 1, 3, and 4, being all bad.

August 18.—The various plots were mown, and, after standing in the field till perfectly dry, were thrashed, with the following results—

1. gave 110 sheaves, yielding 415 lbs. straw and 254 lbs. grain, weighing 62½ lbs. per bush.
2. do. 114 do. do. 518 do. 308 do. do. 62½ do.
3. do. 132 do. do. 427 do. 244 do. do. 62½ do.
4. do. 124 do. do. 439 do. 230 do. do. 62 do.
5. do. 170 do. do. 523 do. 320 do. do. 62 do.

Here we have the following

Final Results per Imperial Acre.

No.	Manure.	Quantity.	Gross Produce. Grain.	Straw.	Cost of Manure.	*Increase or †Decrease. Grain.	Straw.
			bush.	st. lb.	£ s. d.	bush.	st. lb.
1	Potter's guano	2 cwt.	14·97	118 8	20 0	†0·64	3 6
2	Natural guano	2 cwt.	19·71	148 0	1 6 0	*4·10	26 0
3	Nothing	..	15·61	122 0	..	..	..
4	Rape-dust	12 bush.	14·83	125 0	1 7 9	†0·78	*3 0
5	Rape-dust and Nit. of soda	12 bush. 1 cwt.	20·64	149 6	Ex. cost. 1 1 9	*5·81	*24 6‡

Conclusions.—From the above we see—

1. That winter manuring is, generally speaking, injudicious for a wheat crop on a pea stubble, as it induces an early growth, but does not maintain it. The fine state of division in which the soil is, after a stubble crop, the consequent facility with which the plant finds the nutritive matter which is in the soil, and the free access which the atmosphere has to the roots of the plant (causing the ammonia in the soil to be so readily evolved), may, perhaps, explain the proud growth of the plant in its early stages; while this quick escape, or use, of the tillage in the soil—leaving little food for the summer feeding of the plant when it requires most, and when the very luxuriance of foliage calls for a greater supply than if it had not grown so quickly at first—may explain the gradual falling off in the crop, after the winds and rains of spring, and the sun of summer, have exercised their influence on the soil. Again, the natural aversion which the wheat plant has to a light porous soil, and the greater danger which, in such cases—and especially when it has put out such a flush of foliage at first—it suffers from winds, &c., causing damage to the root or the stem, may also have some effect in producing the results spoken of. These remarks, however, be it remembered, relate only to thin or light soils. The stiff texture of strong lands prevents the evils we speak of; and, even on light land, it is only upon stubble that the evil is common: for on clover lea the artificial compression caused by pasturing the crop, leaves the soil in such a mechanical condition, that it acts as a safe storehouse for the manure in it (being less penetrable by the atmosphere or rain), and as a

‡ Be it observed that this is the gain arising from the *extra* top-dressing, and is obtained by comparing No. 5 with No. 4, where no top-dressing was used, both having had the same rape-dust.

better support for the root and the stem. In such case, too, the less ready supply of manure, and the less free medium for the roots to spread in, prevent that overgrowth at first which requires afterwards such an extra supply of food, and such an extra mechanical support against winds and storms. At the same time, the roughness and firmness of soil, which prevent early waste of tillage, provide for and encourage its use at a later and more important season: thus the clods (which the farmer takes care to preserve in preparing the soil), after having endured the action of winter frosts and spring showers, gradually fall to pieces, and convey fresh soil and fresh food to the roots of the plant.

2. That guano is superior to rape-dust and artificial guano as a winter tillage on the pea-stubble wheat crop.

3. That nitrate of soda may be applied with the greatest success as a top-dressing for winter wheat, which may at spring not answer our expectations.

The whole field, as well as the experimental plot, was an example of the truth of this. In every spot where nitrate was applied, its results were, to the harvest, most palpably beneficial—in some cases to a greater extent than the preceding details show. This is one instance of experimental knowledge applied to immediate practical utility; and in evidence of its success, I may add that it is the opinion of some farmers of no mean standing, who saw the working of the experiment, that it justifies us in concluding, 1st, That in all cases it is injudicious to use manure with winter crops upon stubble until spring; and, 2nd, That, by the aid of nitrate of soda, the farmer has a perfect command of the straw (and hence a high probability of grain in proportion) of his crops.

AGRICULTURAL CHEMISTRY ASSOCIATION.*

At the meeting of this society on the 9th ultimo, Professor Johnston gave a report of his proceedings since the last meeting of the committee. He called the attention of the meeting to what had been done in the laboratory since the last meeting. At that time he took occasion to mention the slag from iron furnaces, which Mr. Houstoun, of Johnstone Castle, can get crushed for a shilling per ton. Its value depends on the silicate of lime it contains; its action is not so immediate nor so valuable as quick-lime, and it is of comparatively little value where that substance is abundant and cheap; but where it is close at hand, and lime is high in price, or has to be brought from a distance, it might be applied with advantage; such, for instance, is the case at Muirkirk. At the last meeting he felt it his duty to give his opinion on the Cornwall patent manure. This had called forth an advertisement, which had appeared in some of the Edinburgh papers, from the manufacturers of that substance. It was his duty, however, to give his opinion honestly and faithfully, when consulted as to what might or might not be of benefit to the members of this association, and no proceedings such as were threatened by those gentlemen should deter him from doing so. The attention of the meeting was then directed to the value of several limestones. In developing the agricultural resources of a county, the want of lime in many places is felt as a great evil: anything, therefore, that will remove this, must be of corresponding benefit. It was his duty, therefore, to make known the good qualities of any limestones sent for analysis by members of the association. This has already been done in several instances, as may be seen in the printed report of its proceedings, where there is mention of a limestone from Inverness. Mention was here made of several limestones from the neigborhood of Edinburgh, two of them from Mr. Greenhill, of Bruntisland, an analysis of one of which he was permitted to publish. The one is found immediately above the other, but though so close together they are different in quality, the lower containing more magnesia than the upper, and therefore not so good for agricultural purposes, though well adapted for building. The following analysis will show the composition of the upper one, which is the purest, and therefore best fitted for agricultural purposes. He mentioned that specimens of the limestone, and also of several others which were subsequently referred to, were on the table for the inspection of the members:—

Carbonate of lime.....	95·84
Carbonate of magnesia....	0·84
Alumina and oxide of iron..	0·46
Insoluble silicious matter..	2·68
	99·82

There has also been examined a limestone from Broxburn, which lies a few miles to the west of Edinburgh. This is not so pure as to serve for agricultural purposes, but it is valuable as a cement, as may be seen from the specimens submitted for inspection. Several iron-stones of the true black-band variety have also been examined, but these were not sufficiently interesting to occupy the meeting. Among other substances sent for examination with this view, was the first

* *Ayrshire Agriculturist*, May, 1845.

water in which barley is steeped for twenty-four hours by the maltster. This water is at present allowed to run off, but it has been found to contain a large quantity of solid matter (about 26 grains to the half-pint). This liquor, if mixed with urine, might be beneficial as a manure, but another and further examination will show its merits better. In the steeping of barley, as is well known, the grain is allowed to sprout; but its growth after it has gone a certain length is stopped, and the sprouts fall off. In the neighborhood of Durham these sprouts sell for about 1s. a basket. Mr. Houstoun sent a portion of these for analysis: this has been done, and some curious facts have resulted therefrom. It has been found that they leave 10 per cent. of ash, and 25 per cent. of it consists of phosphates which are so valuable in other manures. The sprouts also contain the elements of ammonia, and 15 parts of silica. This last is a curious fact, as the barley itself scarcely contains any. This silica is contained in the husk which sticks to the seed; and it appears, therefore, that the barley derives its silica from the plant—not like wheat, which takes its silica directly from the soil. Mr. Houstoun has made a contract with some maltsters in his neighborhood for this substance, and Professor Johnston recommended that others should do the same, as he was sure that it would remunerate the purchaser at a reasonable price. Another topic of great importance to the farmer is the action of sulphuric acid on bones; and the more important as guano in a few years may fail, and it will require to be replaced by some other substance, and this may be done by bones, of which there is always a constant supply. They being of great value to the farmer, will become of still greater when guano fails. Bone consists chiefly of two substances—first, gelatine, which can be extracted by boiling in water, and also by the process which was exhibited (that of steeping the bone in dilute spirit of salt). The former method is used for making glue or gelatine. If the bone be burned in the fire it sometimes retains its form, but becomes more white, and easily crumbles down to powder; this is the second substance, namely, the bone-earth, which consists chiefly of phosphate of lime, with a small quantity of phosphate of magnesia, carbonate of lime, and other matters. When either of these substances (the gelatine or bone-earth), are applied to the soil, they are found highly favorable to the growth of plants; but if the bones be boiled in water, and the resulting liquid applied, as is done about Manchester, it produces a decided improvement; and if the bone-earth be reduced to powder and applied alone, it also produces good effects, though not so apparent as when both are applied together. But when sulphuric acid is poured upon bones reduced to powder, certain substances are produced, as gelatine, sugar, or glycerine. These substances contain nitrogen in a form which may be converted into ammonia, and which by itself is capable of entering the roots of plants from its solubility. (The proportion in which sulphuric acid is added to bones is either half a pound of the former to a pound of the latter, or weight for weight.) By the action of sulphuric acid on bones, gypsum and biphosphate or superphosphate of lime, as it is sometimes called, are formed. These being soluble in water can be taken up by plants, and thus the materials of which they build their frame are placed within their reach. This superphosphate is of great consequence, as from some recent experiments which have been made with it, it has been found to produce very striking effects, such as the following:—Clover has been increased one-half; and its effects on wheat have been marked when used as a top-dressing. Wheat, without a top-dressing of the substance, yielded 29½ bushels; with a top-dressing 58 bushels, or nearly double. It has also been found to produce a greater effect than 3½ cwt. of Peruvian guano, the cost of each manure being the same (namely, 42s.); and Sir John Ogilvie has found it to answer better than guano for turnips. The Professor here stated, that his recommendation of these or any other manures, was not for the purpose of aiding any manure-manufacturer, but from a thorough conviction that they were likely to prove of advantage to the members of the association. By the action of sulphuric acid on bones (one of acid mixed with twice its bulk of water) effervescence is produced; this soon ceases, and a grayish pulpy mass is left, which only requires a little bone-dust or sawdust, which should be charred on a hot plate, to make it capable of sowing by the common drill. This in practice has been found to be of great advantage, as some gentlemen have tried 4 pints of it, others 2 pints, and found that it produced the same effect as 16 pints of the bones applied in the usual way. It was here urged on members of the association to try these experiments on portions of their land, before the want of guano might oblige them to do so on a large scale. Another method of using it is with a large quantity of water, and applying it in the liquid form, as the great advantage of applying manures in the liquid state is every day becoming

more apparent. It is more effective than applying the dry bones, as it is spread more completely over the land, and the roots of the plants do not require to spread out their fibres in search of food, as it is here brought to them in a state fit to be taken up. It is well known to practical men, that bone-earth is not found to answer in stiff or clayey soils so well as in open or turnip soils: the reason of this is, that in the former, where the bones are used in a crushed form, each particle of bone becomes covered with a coat of clay, which excludes the air, and prevents its decomposition: now this might be obviated by using them in the liquid form, which will give a greater benefit without this evil occurring.

In the last Doncaster Report, the action of bones is said to be greater when used along with rich compost than when they are mixed with farm-yard manure or used alone. This may be explained in the same manner as already done, as also many other effects of bones with which we are acquainted. Before concluding, a few observations were made on several samples of guano that had been sent in for analysis. There was one analysis, the Professor here stated, that had been sent to him for his opinion, by Mr. Stirling, of Kenmure, and from the manner in which it was reported, he had no hesitation in saying that such a guano had never been imported into this country; and that the analysis (as any one in the least acquainted with chemistry might see) was made by a person totally incompetent to do it. I am permitted to give a copy of the analysis. It is said to be that of a cargo from Ichaboe.

Urate of ammonia	.....	36·14
Muriate of ammonia	.....	9·08
Phosphate of ammonia	.....	22·70
Hyposulphate	.....	6·89
Oxalate and phosphate of lime	.....	33·34
Silica, &c	.....	5·00
Water	.....	6·00
Loss by analysis	.....	0·86

There was also another sample sent by a member of the association, and, though many guanoes had passed through the Professor's hands, still from the appearance of this one, he thought it a good one, as it was said to be Peruvian; but on analysing it he was greatly disappointed to find, that what he thought might be worth 7*l.* to 9*l.* a ton, was not worth 4*l.*; thus showing that even the most experienced may be deceived by the appearances presented. The following is a copy of the analysis which is allowed to be inserted in your paper:—

Water	14·75
Organic matter and ammoniacal salts	13·71
Common salt, with a little sulphate of soda	25·74
Phosphates of lime and magnesia	38·06
Carbonate of lime	5·17
Insoluble silicious matter	2·57
	100·00

The last sample of these guanoes which were exhibited was, from its dark colour, not likely to be a good one, but it was even worse than was anticipated, by the proof, containing as it does, about 20 per cent. of stones and sandy matter, as the analysis shows—

Water	24·47
Organic matter ammoniacal salts	20·61
Common salt, sulphates, phosphates of potash, and soda	9·34
Phosphates of lime and magnesia	22·67
Carbonate of lime	0·50
Insoluble silicious matter, consisting of—	
a large stones	9·52
b sandy matter	12·89
	100·00

The Professor stated it to be his object in mentioning these things, to put the public and the farmers of this country on their guard against such impositions, and to let them know that there was a source where they could apply to discover their frauds; and he further stated, that he was always ready to give his opinion upon these topics to members, when called upon to do so, free of charge.

Several very curious specimens were on the table, especially one from Mr. Balfour, of Balbirnie, of a deposit which had taken place in a wooden pipe used to convey water from a mine. A perfect stone pipe had been formed within the wooden one, and which could not be distinguished from wood, except by the touch and weight—the wooden pipe being so exactly represented by the deposit.

ON SOOT AS A MANURE.*

The soot of ovens and chimneys, which may be procured in the vicinity of large towns, is among the most effective manures; there is not required more than 450 or 500lbs. on the Magdeburg acre of land, in order to produce on all soils (especially the light ones) the most luxuriant vegetation. Soot is an-

* *Agricultural Gazette*, May 10, 1845.

other example demonstrative of the great utility plants derive from certain mineral substances, as we know, by analysis, that 100,000 parts consist of—

38,330 coal, resin, and empyneumatic oil.
30,200 humic acid (partly combined with ammonia).
0,200 acetate of ammonia.
0,360 chloride of calcium.
1,500 phosphate of lime.
5,000 gypsum.
5,650 acetate of lime.
14,660 carbonate of lime, and a little carbonate of magnesia.
4,100 acetate of potash.

100,000 parts.

The constituent parts of soot obviously depend on the component parts of the fuel; for which reason, in some of them, more carbonate of ammonia, sulphate of ammonia, and sulphate of potash are to be found, by which naturally their action as manures is much increased. The soot of coal is superior to that of wood, the former being richer in ammonia.

If 500lbs. of soot are put upon the Magdeburg acre of land, the soil will receive (according to the above analysis), of really fertilising matter, 20lbs. of acetate of potash, 73lbs. of carbonates of lime and magnesia, 28lbs. of acetate of lime, 25lbs. of gypsum, 8lbs. of phosphate of lime, 2lbs. of chloride calcium, 1lb. acetate of ammonia, and 150lbs. of humic acid and humate of ammonia. We may well assume, that of the above substances the salts of ammonia and potash, gypsum, and the acetate of lime will be the most effective, as there is too little of the other substances to possess any tangible influence. In Belgium they take for every measure of seed-corn 8 to 10 measures of soot.

As soot acts by its easily soluble substances, it is always to be used as a top manure, and it is strewed, therefore, in spring, over the winter crops, or harrowed in with the seed of the summer crop. Sickly winter crops will be thereby very soon improved; their color will change into dark green, which is owing to the ammoniacal salts. It destroys almost immediately [?] the moss of the meadows, as these plants cannot bear ammonia at all, as we have seen when speaking of the manure with urine. A top manure of soot over clover will yield a most striking result, which is owing to the gypsum it contains; but in order that it may act efficiently, wet weather is wanted—in dry weather it may even be hurtful, as the plants will then receive a too concentrated food. Its effects last but one or two years. The decompositions which it effects in the soil are of no consequence.

At some places, soot, previous to being used, is mixed with lime and earth. But before the lime-dust is added the soot and earth must be well mixed together, and left lying in a heap for 8 or 10 days. After the lime has been added, the mixture is to be left lying in heaps for 4 or 6 weeks; then it is well worked up and used as top manure. The proportion in which these substances are mixed together is 1,000 lbs. soot, 1,000 lbs. lime, and 1,000 lbs. earth; the latter, however, must be very rich in humus, or the lime will expel the ammonia of the soot.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

THE ANNIVERSARY MEETING OF THE PHARMACEUTICAL SOCIETY OF GREAT BRITAIN, MAY 20, 1845.

In spite of the attempt of the council to draw a large assemblage on this occasion, by holding out in their announcement that the members should enjoy the piquancy of a hash of medical reform, in addition to the usual fare provided, the meeting was very scantily attended. In short, not one-half of the council had the courage to meet the members, knowing, as they must do, that, from their gross mismanagement, nothing can save the society from its impending doom, or arouse the interest of the members from their apathy—an apathy evidenced by the enormous secessions both of members and associates, notwithstanding the reduced subscriptions, abolition of entrance fees, &c.

The proceedings of the meeting were commenced by the chairman, Mr. Morson, expressing his regret at the absence of their president; which regret was clearly not reciprocated by the audience. The secretary then read the council's report, which exhibited the common result of these hole-and-corner societies, viz., increased expenses, with decreased benefits to the members. It was moved by a gentleman that the report be adopted, without a com-

ment, and seconded by the Mayor of Maidenhead, who more than once kindly informed the meeting of his dignified title.

Mr. Harmer, in a very able speech, denounced the perversion of the funds of the society, and its general mismanagement. He wished to know why the benevolent fund was to be sacrificed to the increased salaries of the officers and the increased expenses of the journal. He would ask why the council continued to grant to Mr. Jacob Bell the monopoly of supplying a journal to the members at the frightful cost of upwards of £1,200 annually of the society's funds. If a journal was to flourish through the influences of the society, and that only, why should not the profits arising from the sale thereof go to the society, instead of the editor's pocket? He was for free trade in journalism, as he believed it calculated to give it a more scientific character. He, who had been one of the first projectors of the society, and a member of its council for some time, would gladly use his best exertions to support the society while it continued to promote the objects for which it was originally instituted; but he never would tacitly allow its resources to be prostituted, as they had been for the last two or three years, for personal aggrandisement. One of the first and best features of the society had been the establishment of the benevolent fund—to abandon which was to destroy one of the most valuable qualities that the society possessed, and sink it fearfully in its usefulness and in the estimation of its firmest supporters; he should, therefore, consider it his duty to move that £250 be voted to the benevolent fund.

Mr. Bagster seconded the motion.

Mr. Bell made a lengthened speech upon education *versus* benevolence, and talked considerably about humanity.

Mr. Pedler suggested that those members who were so anxious for the welfare of the benevolent fund, should prove their sincerity at once by voluntary donations. He should oppose Mr. Harmer's motion.

Mr. Herring thought a good dinner was the best means of raising subscriptions for charitable purposes. (Laughter.) He would move that a public dinner be got up, with a good supply of wine, which would wonderfully enlarge the bumps of benevolence in the members' heads. (Continued laughter.) Let them follow his advice; that would be taking the bull by the horns.

Here a scene of indescribable confusion ensued, which had been brewing some time. In fact, the grossest irregularity had pervaded the meeting from its commencement. Motion made upon motion, amendment after amendment submitted, and so much recrimination took place that the chairman found it utterly impossible to unravel the deranged proceedings. He consequently put the original motion, which was carried.

Mr. Waugh hoped, if there was any reporter present, he would refrain from publishing these disreputable altercations. They would excuse him speaking thus, but he had suffered in a similar way himself; and he would tell them, that "a fellow feeling makes us wondrous kind."

Mr. Burden regretted that the petition of the associates for the abolition of Sunday trading and late business hours had been so scurvily treated by the council. He could speak from personal experience of the tyrannical treatment in some of the largest and so-called respectable houses in London. In his own neighborhood, the West-end, young men were kept employed till 12 o'clock nightly, without relaxation during the whole week.

Mr. Squire would tell the meeting that it was upon his motion the petition was rejected by the council. He could not prevent prescriptions coming in at all hours, and when they came in they must be prepared. He would like to know who would dare to tell him when to shut up his shop. Every man must be master in his own house.

The list of nominations for the council, prepared by themselves, was then put to the meeting, and which (after some squabbling, in which several of the council declared, with becoming gravity, their weariness of office, and their assurance of continuing to serve only for the public good) was carried. The chairman then declared the meeting adjourned.

PREPARATION OF SULPHATE OF SODA FROM SEA WATER.*

BALARD† found that a solution of sulphate of magnesia, on being treated with chloride of sodium at a temperature of 50°F., deposits crystals of pure sulphate of soda. Based on this fact he suggests to take the advantage of the sulphate of magnesia occurring in sea water to produce the above effect. The water evaporated to a great extent in summer, by means of *gradirwerke*, is protected from rain, and kept until winter, when it is exposed in large, extremely shallow receptacles, about 3½ inches, (1 decimeter) deep. As soon as the sulphate of soda

* Liebig's *Jahresbericht*, April 12, 1845.

† *Journal de Pharmacie et Chimie*, t. vi., p. 406.

crystallises the mother ley is drawn off. Balard calculated that a *gradirwerke* of 200 hectares may yield at least 600,000 kilogrammes (1,200,000 pounds) of sulphate of soda, and he hopes to gain, by erecting an establishment of adequate dimensions, the quantity of 13,000,000 kilogrammes (26,000,000 pounds) of sulphur, annually consumed in France, which are lost in the manufacture of soda, as a double sulphate of lime and sulphuret of calcium, and to remove the necessity of deriving the sulphur from Etna.

According to the observations of Balard, on evaporation of the mother ley, a double sulphate of potassa and magnesia is obtained, of which one saline of 200 hectares may furnish at least 200,000 kilogrammes, corresponding to 90,000 kilogrammes (180,000 pounds) of pure sulphate of potassa. By this expedient the author believes, that he not only may provide for the whole amount of sulphur consumed in France, but be enabled to render sulphur an article of exportation in the said country. He, moreover, states, that the above recommended process was indeed brought into activity since last year, and will be carried on with full energy next summer. —nt—

DISCOLORATION OF FŒCES.

To the Editors of The Chemist.

GENTLEMEN,

Observing a paragraph in your last month's journal, by Baron Berzelius, on the coloration of fœces after the use of mineral waters, I am induced to lay before your readers the particulars of a similar occurrence which came under my observation in April last.

A physician in town prescribed for a gentleman a mixture containing the trisnitrate of bismuth, and after taking it for a few days, he called upon me to know the cause of his stools becoming quite black. Being unable to account for the sudden change of color, I informed him that it must be owing to the presence of sulphuretted hydrogen in the intestines, which being generated in considerable quantity, probably produced the unpleasant symptoms which he complained of, viz., tenderness over the region of the bowels, with flatus, lowness of spirits, and restless nights. I recommended him to name the circumstance to his medical attendant, which might give him an opportunity of ascertaining the primary cause.

At this period another gentleman was taking the bismuth, and I asked him if he observed any discoloration in his stools; when he informed me that no change had taken place of any consequence during the time of taking that medicine, which is a proof that sulphuretted hydrogen is not generated in the intestines of every individual, therefore bismuth may be used as a test for the presence of that pernicious gas and its compounds.

In warm climates it is usual to give large doses of calomel, in order to produce the desired effect; and in this country it is also given to some persons in considerable quantity, without producing any ordinary symptoms. I do not pretend to assert the actual cause, but I am induced to believe that it is owing to the fœces undergoing rapid putrefaction and generating sulphuretted hydrogen, on the formation of hydrosulphates*, which unites with the calomel, and converts it into the black sulphurate of mercury, a substance comparatively inert, thereby misleading the practitioner; however, I have had no means of proving the fact, but I trust this may meet the eye of some individual who may have an opportunity of examining the fœces after the use of calomel, which may lead to an important result.

I believe calomel is found to act with greater energy, by placing on the tongue small and repeated doses; this probably is owing to its being absorbed, whereas a larger dose enters the stomach, and being conveyed into the intestinal canal, is there converted into sulphuret of mercury.

If not trespassing upon too large a space in your valuable journal, and you deem the foregoing worthy the attention of your readers, you are at liberty to insert the same.

I remain,
Gentlemen,
Your obedient servant,
HENRY OSBORNE.

Southampton, May 15, 1845.

POISONING WITH PHOSPHORUS.†

To discover the phosphorus in cases of poisoning, Runkel ‡ allows the suspected matters to digest with bisulphuret of carbon; after filtering, he shakes them with alcohol, then on applying a gentle heat and evaporating the bisulphuret until only a few drops are left, he throws hot water over these, and obtains the phosphorus in the shape of oily drops, which solidify after some time.

—nt—

* Hydrosulphate of ammonia converts calomel instantly into the black sulphuret of mercury.

† Liebig's *Jahresbericht*, April 12, 1845.

‡ Buchner's *Repertorium*, XXXV., p. 117.

MEMOIR ON THE ETHEREAL TINCTURES.*

BY M. EMILE MOUCHON.

"Il est toujours utile d'essayer de frayer la route, quel même elle serait imparfaitement tracée."—ORFILA.

"Si tout le monde se mêle de faire des expériences et se croit en état d'en faire, peu di gens sont capables de les bien faire; il faut, pour cela, une tournure d'ésprit spéciale qui n'est pas donné à tous."—PARENT DUCHATELET.

FIRST PART.

THE subject of ethereal tinctures has, hitherto, been but little studied; but it must also be observed that these products have really acquired a certain degree of importance only for the last twenty-five or thirty years at most: they were scarcely known in the time of Beaumais; we find that this celebrated pharmacologist expresses himself but vaguely and uncertainly concerning them. With the exception of Cass, who published at the time an interesting memoir on this important subject, no author has undertaken any serious investigation, either to enlighten the opinion of men of science as to the real value of these medicines, considered as therapeutic agents, or with respect to the choice of the best modes of preparation to be followed for rendering them such as they ought to be, in order to fully justify their employment.

Ethereal tinctures in general, which I will also call *étherolés*, with MM. Henry and Guibourt, have never inspired me with great confidence, with the exception of some which I comprise among those of which the codex (French pharmacopœia) recommends the preparation by maceration. Chlorophylle, acting the greatest part in most of these ethereal preparations, and this substance being naturally insipid and inodorous, I could not find in it anything in favor of this group of medicines: the ether is the principal agent, and without it medicine would not have much to hope from the therapeutic action of the ethereal tinctures.

Let us see, however, what we should think of each of these agents in particular, before coming to the facts which I think calculated to elucidate the question. In making an appeal to the knowledge of our day, we shall perhaps find the means of establishing favorable distinctions for some of these agents, at the same time as we may be able to form a nearly accurate idea of the state of the question at the present time.

It is so much the more important to examine each tincture in particular, as by entering into considerations relative to each of these products, I shall have an opportunity of giving my own observations, which answer perfectly to the end which I have proposed to myself.

APPRECIATION OF ETHEREAL TINCTURES ACCORDING TO OUR PRESENT KNOWLEDGE.

Ethereal Tincture of Aconite.—M. Soubeiran says that no experiment has been made to appreciate the composition of this tincture, and he asks whether aconitine is in solution in it. Aconitine dissolving readily in sulphuric ether, it would seem that it should form a constituent part of this preparation; however, I can certify that I have fruitlessly endeavored to find this proximate principle in it. As a counter-proof, I should have looked for it in the extract of which I am about to speak, but I own that I have not carried my examinations so far. This ethereal tincture leaves, as a residue in the capsule in which it is spontaneously evaporated, $\frac{1}{16}$th of solid matter, of a dark-green, possessing a very strong odor and taste.

Alcoholic Extract of Aconite.—After having treated with rectified ether 60 grammes of powdered aconite, in order to collect 250 grammes of tincture. I acted on the powder with alcohol at 21° Cortier, until completely exhausted. From this second treatment resulted 250 grammes of alcoholic tincture, mixed with a certain quantity of ether, which I evaporated to a pilula consistence, after having filtered the boiling alcoholic tincture, in order to separate from it a matter under the form of a coagulum. The extractive mass, collected, weighed rather more than 10 grammes, and presented all the characters of the alcoholic extract of aconite.

Ethereal Tincture of Arnica.—Ether has a feeble action on arnica, it is with difficulty that 30 grammes of this menstruum dissolve 15 centigrammes of yellow resinous matter, having the characteristic aroma of the flower.

It is not the same with feeble alcohol, which is macerated with the powder, already treated by sulphuric ether: it very soon exhausts the vegetable mass, extracting from it more than $\frac{1}{4}$th of very odorous active matter, and entirely analogous to the alcoholic extract of the pharmacopœias. It is probable that by treatment by heat, a greater quantity of it would be removed, if it be true, as M. Soubeiran says, that alcohol at 56° takes up as much as 40 per cent.

Ethereal Tincture of Assafœtida.—This tincture should possess energetic properties,

* *Journal de Chimie Médicale*, April, 1845.

the bitter aromatic resin, with an aliaceous odor, the same as this volatile oil. M. Soubeiran says nothing about it, and it is not noticed in the pharmacopœia of MM. Henry and Guibourt.

This tincture furnished me $\frac{1}{9}$th of very odorous, transparent, solid matter of a light yellow color, soluble in alcohol, &c. The matter remained liquid for a long time. It was then composed of essential oil and resin. It was the resin alone which constituted the ninth of the mass. Having neglected to ascertain the weight of the two bodies combined, I could only ascertain that of the resinous matter.

Ethereal Tincture of Balsam of Tolu.—It would be supposed, that this tincture was active, since the resin and volatile oil should be dissolved in it. Pharmacologists express no opinion in this respect, probably because it is no more permitted to express a doubt concerning that of the ethereal tincture of assafœtida.

There was more than $\frac{1}{9}$th of matter dissolved in this tincture. A resinous matter mixed with a volatile oil was clearly recognised in the residue of evaporation.

Ethereal Tincture of Belladonna.—According to the observations of Ranque, the ethereal tincture of belladonna should be very active, says M. Soubeiran. This opinion might be partaken, on reflecting on the solubility of atropine in sulphuric ether and absolute alcohol, which, moreover, act better on it warm than cold.

The ethereal extract furnished by the ethereal tincture of belladonna amounts to $\frac{1}{16}$th. Its physical characters are the same as those of the ethereal extract of aconite, and do not differ from those of other *solanacea*.

Alcoholic Extract of Belladonna.—The alcoholic extract which may be collected after the treatment of the powder by ether amounts almost constantly to $\frac{1}{4}$th, if it has only a semi-pilula consistence, and to $\frac{1}{16}$th, if it can be rolled into pills. This second product, compared with that furnished by belladonna, which has not undergone the influence of ether, presents no peculiarities: both have the same characters and differ in no respect.

Ethereal Tincture of Buds of Fern.—It is endowed with all the medicinal strength of fern, whilst it contains all the Peschier's oleo-resin of fern; I have not any particular observation to offer with respect to it, except that it is an excellent medicine, and merits the confidence which is accorded to it.

Ethereal Tincture of Castoreum.—It is to be supposed that this tincture possesses marked properties, on account of the volatile oil which enters into its composition, since this oil should be regarded as the principal agent of costoreum, rather than castorine, although the latter, which Brandes regards as the medicinal matter, has the peculiar odor of castoreum. The resin, moreover, may add to the properties of the tincture.

Although I approve the employment of 4 parts of ether to 1 of the substance to be exhausted, in the treatment of plants, I here recognise the insufficiency of these proportions, 8 parts of menstruum not being able to remove from castoreum all that it can yield to it: the liquors are changed to the end. It is therefore, with some reason that some pharmacologists order 1 part to 8 in the preparation of this tincture.

4 grammes of tincture of castoreum, prepared in the proportion of 1 part to 8, leave in the capsule 4 decigrammes ($\frac{1}{10}$th) of highly odorous brownish matter, part of which is soluble in ether and the other in weak alcohol; of almost dry consistence, after an exposure to the air of a month or more in order to drive off the volatile oil, maceration or displacement is resorted to. It is evident from this that the maceration recommended by the codex is completely useless; but, on account of the nature of the body to be exhausted, it would be necessary to operate on a dilution in a portion of the menstruum, before proceeding to the displacement, and that in such a manner as to reduce the castoreum to a liquid magma. Without this precaution, we should expect to see the operation increased in length, especially in operating on rather considerable quantities.

Alcoholic Tincture of Castoreum.—When 250 grammes of alcoholic tincture by displacement are collected, after the treatment by ether, from 60 grammes of powder, the exhausting of the substance is completed, and we obtain as a product a liquid as deeply colored as the ethereal tincture, although the ether has produced its full effect. Then we have nothing but an inert residue, whose quantity, with relation to that of the animal matter employed, is only about one-third, if the castoreum is free from impurity.

The evaporation of the alcoholic tincture gives us a product one-sixth of solid matter, of a brownish aspect, of a penetrating odor like that of castoreum, and besides very analagous to the residue of the ethereal tincture, deprived of its essential oil; which denotes that ether very imperfectly dissolves the active matter, although the ethereal tincture of castoreum is one of the most energetic.

Ethereal Tincture of Cantharides.—We are acquainted with two ethereal tinctures of cantharides: one which is prepared with one part of these *coleoptera* and three of sulphuric

ether; the other, which is designated under the denomination of acetic ethereal tincture of cantharides, is ordinarily obtained in eight days, 1 part of cantharides in 8 parts of acetic ether. Both tinctures are generally considered as very energetic; however, if we separately and spontaneously evaporate in two small capsules, 30 grammes of each of these products, we find in each evaporating vessel only 4 or 5 centigrammes of active matter. Does this justify the reputation of these two tinctures? I do not think so, unless it be supposed that cantharidine, which is very volatile, even at the ordinary temperature, volatilised during the evaporation in the open air, which is very probable, and nearly certain, for it cannot be denied that these tinctures possess a truly energetic action. This supposition is moreover so much the better founded as it is also justified by the volatility of the two menstruums.

Ethereal Tincture of Cicuta.—Considering that conicine or cicutine is a body very soluble in ether, it may be presumed that the ethereal tincture of cicuta is a good medicine, more especially as this product has a powerful odor of cicuta, which may strengthen this presumption, however, it must be acknowledged that it is one of those which are least employed.

One-twelfth of pilula matter is the result of a spontaneous evaporation of this ethereal liquid. The characteristic odor of the plant is also met with here.

Alcoholic Extract of Cicuta.—By submitting to the action of a sand-bath the alcoholic menstruum which is substituted for the ether, an extractive mass is obtained, which recommends itself perfectly by its physical characters, and which we are authorised in believing to be energetic. The alcoholic tincture of cicuta also furnished about one eighth, that is to say 60 grammes of powder do not give less than 8 grammes of extract.

Ethereal Tincture of Digitalis.—"This tincture," says M. Soubeiran, "which is generally considered as very efficacious, is regarded, on the contrary, by some practitioners as having only the properties appertaining to sulphuric ether." I may add, that, if the opinion of M. Dulong be correct, it must be supposed that the latter are right, whilst in believing with MM. Rein, Haase, Planavia, and Leroyer, that sulphuric ether is the best solvent of the active principle of digitalis (digitaline) we should rank among those who believe in the efficacy of the ethereal tincture. The observation made by Berzelius would also lead us to think that this product should not be deprived of the properties of the plant if it were true, as he would induce us to suppose, that the chlorophylle of this vegetable is itself very active. However, the recent works of MM. Jeuvenne and Homolle, would seem to throw new light on this question, the digitaline of these gentlemen being only a soluble in pure ether, and becoming very soluble in this menstruum when alcohol is added to it.

This ethereal tincture leaves in the capsule $\frac{1}{10}$th of soft extract, which might be supposed to be active, judging from its peculiar odor, which resembles that of the plant which furnished it.

Alcoholic Extract of Digitalis.—By acting as I have indicated, $\frac{1}{8}$th of solid extract is obtained fit to be rolled into pills. If this product be compared with the alcoholic extract of the shops, it may easily be confounded with it, so great is the resemblance.

Ethereal Tincture of Hyosciamus.—What I have said of the ethereal tincture of belladonna appears to me also applicable to that of hyosciamus. Hyosciamine having several characters which are confounded with atropine.

It is also about $\frac{1}{10}$th that is found in the capsule, if the residue be collected by spontaneous evaporation.

Alcoholic Tincture of Hyosciamus.—The 250 grammes of tincture obtained leave in the capsule rather more than 8 grammes of pilula extract, or $\frac{1}{4}$th of the plant employed. There is also an identity between this extract and the alcoholic extract of hyosciamus prepared according to custom.

Ethereal Tincture of Saffron.—Rectified ether only very feebly attacks the soluble principles of saffron. It is not the same with Hoffmann's mineral liquor, which produces a tincture tolerably charged with color; this may, therefore, be very good medicine, although the tincture arising from the action of a mixture of equal parts of ether at 56°, and of aqueous alcohol at 21° Cortier, should be preferred to it, as being more deeply colored. There is no great difference between the latter and the alcoholic tincture of saffron. However, the one leaves in the capsule $\frac{1}{20}$th of residue, and the other about $\frac{1}{18}$th. These remarks clearly prove that this ethereal tincture, prepared either with Hoffman's liquor or with ether and aqueous alcohol, should occupy a place in our pharmacopœias, and, therefore in medical prescriptions: this would be, without contradiction, one of the best of our ethereal tinctures.

Ethereal Tincture of Strammonium.—We may also apply to this product that which has been said of the ethereal tincture of belladonna, since it may be supposed that daturino, although less soluble in ether than in alcohol, is in solution in this menstruum,

the evaporation of which leaves as a residue $\frac{1}{10}$th of soft matter. Alcoholic tincture of strammonium. This tincture also furnishes an extract which cannot be better prepared than with alcoholic extracts of the *solanacea*, and, in particular, to that of strammonium of our shops. Concentration yields about $\frac{1}{8}$th of solid mass.

Ethereal Tincture of Valerian. — The ethereal tincture of valerian holds in solution all the resinous matter, the volatile oil, and the valerianic acid, that is to say, all the active parts of the root (Soubeiran).

If 8 grammes of ethereal tincture of valerian be evaporated in a capsule, there remain in this vessel only 20 centigrammes of a light yellow matter, very aromatic, very bitter, of a resinous nature, soft and poisseuse. This certainly is not the whole of the active matter of valerian; however, this ethereal tincture must be one of the most energetic.

By adding to 60 grammes of valerian, in powder, heated by ether, 200 grammes of water, the exhausting of this root is completely accomplished, and there is thus collected a very highly-colored aqueous tincture, which leaves in the capsule 12 grammes of tolerably aromatic pilula extract, which cannot be denied to possess active medicinal properties. This extract would have been more active, doubtless, if weak alcohol had been substituted for water, and it is to be presumed that, instead of 12 grammes of product, only 8 would have been obtained.

The rapid exposition which I have just made does not comprise some ethereal tinctures now very little used, with which I have not thought it necessary to occupy myself, not only because I am not able to say anything concerning them, which would not be superfluous, but also because they appear to me to have completely sunk into oblivion, although they seem to me worthy, for the most part, of the confidence accorded to them at the time.

(*To be continued.*)

HUMIPLIGIA CURED BY MEANS OF STRYCHNIA.*

BY DR. BALLOTTA.

The very considerable dose in which the medicine was administered in this case is our principal reason for giving the following abstract of it:—

A female orphan, aged twenty-nine years, plunged in her youth into profound misery, inhabiting a damp ruin, often in want of food, was reduced by degrees to the most state of emaciation. At the same time, and gradually, she lost the use and sensation of the left arm and leg, which only became sensitive as the seat of obscure pains during the atmospheric changes.

This state had existed for thirteen months, when, at the advice of Dr. Ballotta, she entered the hospital, in which, liberal diet, generous wine, the use of ferruginous preparations, stimulant frictions on the *rachis* and the paralysed limbs, restored her color and the appearance of health. The paralysis remaining, nevertheless, as bad as ever, Dr. Ballotta thought it necessary to have recourse to some more direct treatment. He, consequently, began by administering strychnia in the dose of one-sixteenth of a (Italian) grain per diem, in pills; then he raised it to one-eighth of a grain, and gradually to half a grain, and finally to one grain. These changes in the quantity of the remedy were made every four days. Obscure involuntary motions were then remarked in the paralysed limbs, and there was a sensation of pain in them, which encouraged the physician to raise the dose to two grains per diem. At the end of four days, the patient still bearing the medicine perfectly well, three grains of strychnia per diem were given to her, and this was continued for six days. Then commenced a prickly sensation in the paralysed limbs, the heat increased, they were covered with perspiration, and became the seat of shocks resembling those of tetanus; finally, the patient could easily perform voluntary motions. At this period the doses were diminished in the same order as they were increased. At the end of two months the patient had taken seventy grains of strychnia. The limbs having then recovered perfect freedom of motion, she left the hospital.

* *Il Raccoglitore Medico.*

INFLUENCE OF COMPRESSED AIR ON HEALTH.*

BY M. TRIGER.

M. Triger, civil engineer, having to establish a mine-shaft in the midst of the alluvia of the Loire, and not being able to exhaust it of water, since it would have been necessary to empty the river itself, conceived the idea of doing so as far as possible by means of pumps worked by a steam engine. In consequence of this arrangement, the workmen worked in air compressed to three atmospheres. The effects produced by this air were the following:—

From the first strokes of the piston a more

* *Annales d'Hygiène Publique.*

or less violent pain was felt in the ears. This pain ceased in every one as soon as the mercury reached in the manometer the height of three centimetres. The motions of deglutition immediately dissipated it, probably by driving into the Eustachian tubes a certain portion of air; the latter, by re-establishing, as regards the membrane of the tympanum, the equilibrium of pressure within and without, arrested the tension of this membrane, which the excess of pressure of external air, penetrating by the *conduit onditif*, had pressed on the *caisse* of the drum. The habits of individuals contribute to render this pain more or less violent. Drunkenness is a certain means of rendering it intolerable, even several hours after the intoxication has gone off. The workmen complain much of the cold produced by the stoppage of the internal air when the communication with the external air is restored. There results from this stoppage of air a very cold fog, and so much the thicker as the capacity of the box in which the men work is greater. Every one speaks through his nose and loses the power of whistling in three atmospheres. Two workmen, after having passed seven hours in compressed air, suffered, half an hour after leaving the shaft, violent pains, one in the left arm, the other in the knees and left shoulder. Frictions with spirit of wine dispelled these pains, and these workmen did not discontinue working the following days.

POISONING BY CHERRY KERNELS.*

The daughter of a widow, aged five years, ate a considerable quantity of the kernels of sweet cherries (*prunus acium*). Her brother, a few years older than herself, also ate some. After the lapse of a few hours, symptoms of poisoning appeared. When the author was called the next day, he found the girl so comatose that she could not be roused by any means. The eyes were closed, the pupils considerably dilated, the skin moist and hot, respiration exceedingly hurried, pulse small and quick, urine and fœces discharged involuntarily; the child very restless. A saturated solution (probably citric acid, with carbonate of potassa), was ordered internally, and cold fomentations to the head externally. After a few hours, vomiting of a greenish mass ensued, and was followed by retching, which lasted till death; the body was spasmodically contracted backwards. The illness lasted forty hours. On the *post mortem* examination, the stomach externally was normal, and internally rather swollen and reddened; the intestines were strictured and invaginated, but there was not any inflammation. The liver, spleen, and large vessels contained black tar-like blood. The body was not examined further. The boy, who had eaten fewer cherry kernels, became likewise ill, but recovered in the course of a month. An eruption, analagous to urticaria, came out on the fore-arms of both children; they were both perfectly well, according to the statement of the mother, before eating the cherry kernels, and no other cause for the attack could be assigned. The kernel of the *prunus acium* (*cerasus nigra, MM.*) containing amygdaline and developing prussic acid, with ethereal oil, in the stomach, is a very remarkable occurrence, and one which ought to serve as a warning.

* *Mertens of Wangroweit in der Medic. Zeit. f. Heilkunde, Prussic.*

COPPER COLIC.*

Dr. Blaudet recently read a memoir before the Royal Academy of Sciences, in which he endeavors to prove that copper colic, of which very few cases have been met with in hospital practice, is very common in workshops, and that the apprentices are attacked with it with scarcely any exception. This affection is slight and apyretic in most cases. It is characterised by fits of colic, to which is joined extreme prostration. The abdomen is not always indolent; sometimes constipation is observed, and sometimes, on the contrary, diarrhœa. This disease lasts, on the average, about forty-eight hours. It is owing to want of cleanliness, and to the inspiration of cupreous particles, particularly among workmen who turn copper with pumice. In workshops, milk is frequently employed as a preservative against copper colic. The diarrhœa should be combated by demulcents, and the constipation by minoratives.

DISCUSSIONS ON DIFFERENT POINTS OF TOXICOLOGY.†

The discussions which have been raised with respect to various questions of toxicology, demand, on account of the interest which it excites, a place in this journal (*Journal de Chimie Médicale*): we report them here in order that each of our readers may be able to

* *Annales d'Hygiène Publique et de Médicine Légale*, April, 1845.

† *Journal de Chimie Médicale*, May, 1845.

form his own judgment concerning the opinions expressed on either side.

TOXICOLOGICAL INVESTIGATIONS CONCERNING POISONING BY MERCURY.

Poisoning by mercury and its compounds is, happily, very rare, and this is not extraordinary, the soluble salts of mercury having an exceedingly disagreeable metallic taste, which would inform the victim who would reject the poison and obtain help. Therefore poisoners do not make use of it, and give the preference to arsenical preparations which are almost tasteless.*

MM. Danger and Flaudins, who have been occupied with investigations relative to poisoning by mercury, have just presented to the Institute a memoir, of which we here give an extract:

It is scarcely necessary to observe, that, in the class of metals, mercury is, after or rather equal with arsenic, the most dangerous poisonous element which can fall into criminal hands.† One of its compounds, the bichloride, or corrosive sublimate, for a long time bore the name of powder of succession. It was one of the poisons of De la Brauvilliers, and the principle, which was found in the famous casket of Sainte-Croix, seized by the government. In 1613, it was by this poison that the Count and Countess of Somerset murdered Sir Thomas Overbury while confined in the Tower of London. The murderers tried successively on the victim aqua fortis, arsenic, powdered diamond, caustic, large spiders and cantharides; corrosive sublimate, administered in enema, was followed by death in less than 24 hours. There is reason to believe that corrosive sublimate was known to the ancients, and prepared especially by the too-celebrated woman who was Nero's associate in crimes, and who distilled poisons in the imperial palace.

In the toxicological study of mercury, the only progress due to our time is the discovery and employment of Smithson's pile. The evidence of this little apparatus are well known—a sheet of tin covered by a sheet of gold developed in the spiral form. The tin constitutes the electro-negative element, and the gold the electro-positive element. Steeped in a solution containing mercury, this pile separates from it the metallic element which goes to the gold, and turns it white. It is sufficient afterwards to volatilise the metal in a small tube, in order to obtain it in the characteristic liquid globular form.

For the comparative examination of the reactions for detecting mercury in its solutions, we have ascertained that the galvanic or galvano-plastic action was the most delicate. We have been enabled, by aid of this reaction to prove the existence of mercury in the solution, containing $\frac{1}{100.000}$th. This is the limit we imposed on ourselves in our former investigations. We are happy that there is no exception to be made with regard to a metal which is not only a notorious poison, but which, under various forms, is a valuable medicine of very frequent employment.

It is not the galvanic apparatus as invented by Smithson which we have employed in our experiments; we have retained only the principle of it. In toxicological investigations, this ingenious instrument would have had inconveniences which we wished to avoid. The following is the apparatus which we propose to substitute for that of the English chemist:—

A vase served as a receiver for the test liquid. On a support is adapted a sort of funnel, terminated by a tube, the end of which is drawn out, and whose calibre is almost capillary. This tube forms, with the body of the funnel, an angle of 90°. The vessel, filled with the suspected liquid, is emptied into the small funnel. By means of one articulation of the support, such an inclination may be given to the apparatus as may be convenient for the flowing out of the liquid. In the widest part of the funnel is placed the electro-negative conductor of a Bunsen's single-couple pile, and in the bore of the capillary tube is introduced the electro-positive conductor. Each wire, at least that part which touches the liquid, should be of pure gold. The two poles must be brought so nearly together as almost to touch. In consequence of the excess of pressure on the capillary aperture of the tube, the liquid flows out drop by drop, and it is received into a capsule, the vessel acting the part of Mariotte's vessel, or of an intermittent tap, the pressure remains constant on the liquid, and the flow is regular. This effusion may be accelerated or relaxed at the will of the operator by the degree of inclination given to the apparatus. It appears to us that, in ordinary cases, it should be regulated in such a manner that in falling each drop of the liquid should mark an interval of 5 seconds. The pile, put in action, a more or less disengagement of gas is operated at the poles, an index to the

* The question of taste has been the subject of controversy. We have endeavored, by employing all suitable precautions to solve it, and we have recognised in arsenical preparations only a scarcely perceptible taste.

† In France, the perchloride was employed 10 times in 194 cases of poisoning; in England, 12 times in 461.

intensity of the current, and the mercury of the solution is deposited on the electro-positive gold wire, and turns it white. In order to ascertain that this color is due to mercury, it is only necessary to volatilise the metal in a small reduction tube, by means of an enameller's lamp.

Being certain of discovering, with this apparatus, the slightest traces of mercury which chemistry can detect, it remained for us, in order to attain the object which we propose to ourselves, to find a process of separating the mercury from organic matters, and for isolating it as much as possible without loss. The Academy was kind enough to give its approbation to the process of carbonisation by sulphuric acid which we proposed, and this process is now generally practised in medico-legal investigations. It was necessary to make endeavors to apply to the detection of mercury; we have arrived at it without the necessity, which we had at first feared, of having recourse to distillation, an operation of very difficult application in the sometimes very decisive tests of toxicology.

The following is the process which, after numerous trials, we ultimately adopted. We liquefied, at the temperature of about 212°F., the animal matters by one-third or half their weight of mono-hydrated sulphuric acid, according to the ordinary method. This liquefaction being affected, which requires only an hour-and-a-half or two hours at most, we remove the capsule from the fire, and allow it to cool to a certain degree. Then, after having placed the vessel under a chimney of good draught, in order to protect the operator from the disengagement of gases, we add to the black liquid of the carbonisation, pieces of saturated chloride of lime, stirring the mixture with a glass rod. In proportion as the matter becomes thick and turns white, distilled water is added to it which favors the solution of the chlorine, and this manipulation is stopped only when it appears to the eye that the liquid to be separated by the filter is almost colorless.

The quantity of chloride of lime to be employed will always be, within a little, relative to the proportion of sulphuric acid required for the perfect liquefaction of the animal matters. To 100 grammes of liver, on account of the bile and fatty matters which that organ contains, sometimes as much as 50 grammes of sulphuric acid and 50 grammes of chloride of lime are required; but it is rarely necessary to exceed this proportion. The matter being whitened and brought to the appearance of a calcareous earth, it is well moistened with absolute alcohol, without heat; in order to be more certain of attaining the mercurial compound, it is diluted with distilled water filtered and the precipitate washed several times, if the liquid is too abundant, it is concentrated by evaporation; after which it is submitted, in the apparatus described, to the action of a galvanic current. We have demonstrated by experiment, that the voltaic current favors the precipitation of the mercury on the gold wire, and that in all cases, at least, it has the advantage of accelerating its operation, which, without the assistance of this action, would perhaps require a long time for its accomplishment.

The metal obtained on the electro-positive conductor of the pile, it is necessary, in order to remove all fatty matter, to wash the gold wire in ether or boiling alcohol, and to dry it before introducing it into the reduction tube. The latter should be prepared and blown with the precautions requisite for avoiding moisture, which might soil the globule of mercury, sometimes extremely small, which it is necessary to render visible.

ON THE HEAVY CALCINED MAGNESIA.*

BY M. J. DALPIAS.

(Presented by M. Colas to the Société de Pharmacie, February 5, 1845.)

For half a century, Messrs. Henry, of Manchester, have prepared a heavy calcined magnesia; the consumption of which is immense, notwithstanding its very high price.

For a long time, these gentlemen alone possessed the secret of manufacturing this kind of magnesia; but for several years, two or three other English manufacturers appeared to have discovered their process. Thus, the heavy calcined magnesia of Mr. Howard, of London, is perfectly similar to that of Henrys'; its density is the same, it is at least five times higher than that of ordinary calcined magnesia. Except Messrs. Henry, Howard, and two or three other manufacturers, there are, probably, very few persons in England who possess the secret of this preparation, and I think that M. Colas has rendered a true service to science by immediately making known the process which he has just discovered, and which consists in the following manipulation:—

A very firm paste is made by moistening powdered carbonate of magnesia. This paste should be well beaten, in order to employ as little water as possible; then it is dried in an oven, and it is calcined after having been pressed down in the crucible. It also contracts by calcination.

* *Journal de Pharmacie*, April, 1845.

The crucible may also be filled with undried paste, and the whole put in the stove, in order to calcine it only after desiccation.

M. Colas has proved that it requires less time and heat to calcine magnesia, thus prepared, than for the calcination of ordinary magnesia. He has also proved that his heavy magnesia does not become hydrated, even after having been left for 24 hours in cold water. Those of Henry and Howard cannot become more hydrated; it is likewise less soluble in the acids, and is much less apt to condense carbonic acid when exposed to the action of the air.

In 1811, M. Planché (*Bulletin de Pharmacie*, vol. iii., p. 511,) observed, that calcined magnesia might be obtained more or less dense, according as it is more or less strongly pressed before calcination; but, in order to obtain it very heavy, he recommends it to be kept maintained at a white heat for 6 or 8 hours.

More recently M. Durand published a process more similar to that of M. Colas. This chemist expresses himself as follows—"Carbonate of magnesia is prepared with very pure sulphate of magnesia and carbonate of soda; before it is perfectly dry, it is strongly pressed in a mould in order to give it compactness; it is afterwards heated to whiteness for at least 6 or 8 hours."

From the foregoing, it would appear that M. Durand would consider it necessary to employ a recently prepared and still hydrated carbonate of magnesia; this alone renders his process very little practicable; and the white heat for 6 or 8 hours renders it very expensive.

M. Colas's process is, on the contrary, so convenient, since it admits of a quantity of magnesia being calcined at one time and with less fuel, that it is to be regretted that his product cannot be recommended for medical use; ordinary calcined magnesia, notwithstanding the inconvenience of its lightness, should always be preferred to it, since it is much more soluble in the acids, and in equal weight is certainly more active.

Before presenting M. Colas's product to the *Société de Pharmacie*, I thought it my duty to repeat his experiments, which I found very accurate. I will here add some of mine, which, notwithstanding their little importance, may be placed after his, for they were made in consequence of them.

In the first place, I do not think that M. Colas's process is exactly the same as that pursued by the English manufacturers, for their heavy magnesia is so fine and soft to the touch that it might almost be taken for talc. This property is precisely the only one which M. Colas's magnesia does not possess in so high a degree. That which leads me to think thus is, that in these same manufactories, a heavy carbonate of magnesia is prepared, whose density is nearly equal to Henrys' magnesia, and that this calcined carbonate gives a product equally heavy, and very soft to the touch.

The preparation of this carbonate is also a secret.

I will conclude this note with a few words on the light calcined magnesia, which comes from London and whose very moderate price is the cause of its being universally met with, and that many pharmaciens are wrong in not calcining their own magnesia.

M. Mialhe (*Journal de Pharmacie*, June, 1844) finds that "this kind of magnesia contains little or no carbonic acid." For my own part, I have never met with English calcined magnesia of commerce which did not contain carbonic acid; on the contrary, a very appreciable quantity of it is always found, and it always effervesces with sulphuric acid diluted to one-eighth. With respect to the water which it contains, his observations and those of M. Dubail are too correct for me to add anything to them. I will only say to M. Mialhe, that I do not think, with him, that ordinary calcined magnesia attracts carbonic acid only when it is hydrated; I think that it attracts water and carbonic acid at the same time, and it was with the view of proving my assertion that I made the following experiment:—

Light caustic magnesia* was left for a month over mercury, and in an atmosphere of perfectly dried carbonic acid. Not the slightest absorption took place. I afterwards placed in the top of the bell-receiver a piece of ice wrapt in blotting-paper, in order that it might traverse the magnesia without wetting it. The carbonic acid was there gradually absorbed in so short a space of time, that, in this case, the magnesia could not have been hydrated before absorbing the gas. I think that the latter explains the presence of carbonic acid in the light calcined magnesia which comes to us from England; for, as M. Mialhe has wisely supposed, the English manufacturers are in the habit of exposing their calcined magnesia to the humid air, in order thus to obtain an increase of weight, varying between 15 and 20 per cent. I have several times repeated this experiment, and, contrary to the opinion of M. Mialhe, I have never succeeded in obtaining a hydroxide, but always a mixture of hydroxide and carbonate.

* I give this name to the magnesia which we prepare in our laboratories to distinguish it from English magnesia.

An hydroxide with 1 atom of water may be directly obtained, provided that the magnesia be very caustic, and especially very light; if it possesses these two properties, it will be seen that, after having been wetted, by desiccation in an oven, it will gradually lose its weight until it contains only 30 per cent. of water; then calcination alone can make it lose more.

ACTION OF PUS ON THE SEQUESTRUM OF BONE.*

On the 17th May, Professor Cooper concluded his very interesting course of lectures "On Fractures and the Reparation of Bone." In this short abstract it is our intention to present to our readers the Professor's observations on a subject which has both novelty and interest in relation to the action of pus upon the sequestrum of bone.

The Professor commenced his lectures by making some useful observations on the functions of the osseous system in the animal economy, and then proceeded to the structure of bone itself, correctly remarking, that from its hard and apparently inorganic nature, many had been led to regard it as partaking but slightly of that vital power which characterises the softer structures, whereas, in reality, the same laws preside over and govern the formation, growth, and reparation of each. This view was illustrated by a well-chosen series of preparations and drawings, showing the gradual development of bone in the fœtus, in which the Professor pointed out that the ossific matter deposited appeared to be less with a view to the priority of use in the particular bone, than to the amount of earthy matter ultimately to be found in the various bones, but at the same time mentioned that the progressive development seemed in some instances to be regulated by both of these intentions. A parallel was next drawn between the formation and reparation of bone; here, by analogical reasoning, the lecturer demonstrated the identity of the means employed by nature in both of these cases, and proved, we think, satisfactorily, that the difference in the result is due to a difference in the degree only of the operation, and not to any difference in the operation itself; and his remarks on the part which protein plays in these important processes were not less interesting than instructive.

The nature, cause, and external appearance of fracture in bone, were next dwelt upon with great perspicuity, and the daily changes which occur after a fracture, from the first infliction of the injury, up to the period of reunion, were shown from some excellent drawings which had been made from recently-fractured bones.

The Professor next introduced to the notice of his audience an instrument, invented by Dr. Jarvis, an American surgeon, for reducing fractures and dislocations, and commented on its use. This instrument has been modestly denominated "the Adjuster," by its ingenious inventor; a name which correctly enough points out the ultimate effects produced by its application, but conveys no adequate idea of the enormous power and wonderful simplicity of action which characterise the important invention; and Mr. Cooper gave it as his opinion, in unqualified terms, that the apparatus must necessarily rise into the most general use, although at the same time he seemed to consider it as more applicable in bad cases of fracture than even in dislocations, as these forces, as now applied, through the use of the pullies, rarely failed in them, although every day insuperable difficulties in the treatment of complicated fractures occurred in the practice of the most skilful surgeons.

In his last two lectures the Professor confined himself entirely to the subject of diseased bone, to which he appears to have devoted much attention, and he succeeded in throwing great light upon the process adopted by nature to free herself from difficulties which at first sight appear insuperable.

"In the case of the sequestrum of bone," observed the Professor, "we find the dead bone enclosed, not only in the surrounding soft parts, but actually enveloped by newly-formed living bone, whence its removal by any other than mechanical means might seem impossible; yet it is well known that it is removed, and absorption has been pointed out as the method employed to effect this object. Sir W. Blizzard, and others, have shown that by placing portions of bone in ulcers, a part is removed, for the bone loses weight, and thus it was supposed to have been removed by the surface of the ulcers. The fangs of transplanted teeth are also known to be acted on, and Mr. Wilson ascribed this to their absorption. Those surgeons who have carefully studied the functions of the absorbents consider the removal of dead bone by these vessels as extremely improbable, and the merit of reconciling the facts with the true circumstances of the case, appear to be due to Professor Cooper, who has investigated nature's process throughout, and has proved, through the assistance of an experienced chemist, that the discharge arising from diseased bone

* *Medical Gazette.*

contains a large quantity of the solid constituents of bone in solution, which consequently pass off in these fluids. He was thus led to believe that nature finds in the animal portion of dead bone a solvent for the more solid and inorganic portions, and that the surrounding tissues, by the pus they throw out, join in the attempt to extrude a substance, which, although once organised, has become an extraneous and foreign body. Not only is this discovery of the greatest use as pointing out the means employed to effect the removal of the dead bone, but it also enables the surgeon to ascertain without the assistance of the probe whether bone be diseased or not; for it is sufficient to know by a chemical examination of the matter discharged, whether phosphate of lime be present or not, to infer at once the existence of dead bone. These discharges no doubt differ with the nature and extent of the disease existing in the bone; but the discharge from a case of diseased bone, at present under surgical treatment, was found to yield after incineration from 72 grains of pus—

	grs.
Phosphate of Lime	1·75 or 2·43 per cent.
Carb. Lime and Soda	·15 or ·208 per cent.

The experiment, therefore, of Sir W. Blizzard merely shows that the purulent discharge of ulcers is capable of effecting the solution of bone, and carrying it off; and no doubt the fluid which continually oozes from a transplanted tooth possesses the same property; thus refuting the arguments of Mr. Wilson on this subject.

Having discovered nature's method of removing bone, it becomes practicable for us to imitate or assist in the operation. The injection of nitric acid was long ago proposed for the purpose of dissolving dead bone, and we now see that this is strictly in accordance with the means employed by nature; but nitric acid is a powerful caustic and irritant, its use is therefore injurious to the surrounding living parts, and necessarily interferes with the process of reparation, whereas the fluid employed by nature is harmless, and permits of the reparation of the injury, whilst it removes the obstruction which impedes success. Professor Cooper, at the end of his course, said he was still prosecuting experiments on this subject, and expressed with some confidence a hope that he would be enabled to find a solvent of bone which shall possess all the requisites essential to insure a steady solution of dead bone without injury to the surrounding tissues. He is now, we believe, employing a solution of phosphoric acid for this purpose; for even when moderately concentrated, it exerts no corrosive action on the skin, and its combination with phosphate of lime forms a super-phosphate, which is so soluble as to be deliquescent, and therefore easily removed in a liquid form. The Professor also recommended phosphate of soda in preference to sulphate of magnesia, in cases of disunited fracture, when aperient medicines are indicated. This appears to us well worthy of attention, as a means of supplying at once the necessary evacuation from the bowels, and placing within the reach of the absorbents a substance fitted to supply the deficiency which exists in the system for the reparation of bone.

EFFECTS OF VAPORS OF ZINC ON THE ANIMAL ECONOMY.*

M. Blaudet, in the work already quoted, calls attention to the symptoms inherent to the operations of working in copper. These symptoms, not hitherto noticed, are manifested in the afternoon, or the morning after, of the melting days. The following are the principal symptoms:—Lassitude, muscular pains; oppression, headache, vomitings; shivering fits, continuing for three or four hours, and ending in copious perspiration and febrile reaction. These symptoms appear to be the effect of poisoning by zinc, which enters largely into the composition of bronze, brass, *maillechort*, &c. The high temperature to which these alloys are submitted, in order to reduce them to fusion, explains why these effects are developed in the factories of which we speak, although they are not ordinarily observed in zinc foundries where the temperature is not raised so high to liquefy the metal. The vapors of zinc, carrying off a small portion of copper, being oxidised in contact with the air, fill the work-room, and are deposited on the walls; it is under this finely divided form of oxide that the metal penetrates with the air into the respiratory organs. The malady produced by zinc does not last longer than 24 or 48 hours. It varies in severity according to the following conditions:—Bad draught of the chimney; contrary winds driving the smoke into the workshop; closing the latter during the cold season; the flowing of the metal into the middle of the workshop. The moulders are always ill when the workshop of the foundry and the casting-room are the same. Finally, the persons living in the vicinity of a foundry sometimes feel its effects.

To remedy these inconveniences it is important—

* *Annales d'Hygiène Publique*, April, 1845.

1st. To separate the casting-house from the foundry.

2nd. To operate the pouring and fusion under a box communicating with the chimney.

3rd. To keep the foundries, as far as possible, from populous districts.

Sweats and purgatives appear to hasten the resolution of the symptoms produced by zinc. Warm wine and tea are very much used in these cases by the working founders.

ARSENIATE OF QUININE.*

M. Bouriéres, pharmacien, has lately made known a new salt, the arseniate of quinine, which he proposes to substitute for arsenious acid, the latter being threatened with final proscription, on account of the dangers which may result from its use. He obtained this salt directly by treating pure quinine with arsenic acid. This latter body being more soluble in water, and producing more neutral salts than arsenious acid, and which are better crystallised, he has given it the preference. M. Bouricres hopes that this new compound will possess the well-proved efficacy of arsenic in obstinate intermittent fevers, presenting the advantage of being exclusively reserved for medical use, which will prevent all danger. Without pretending that the small quantity of quinine which enters into combination in the arseniate of this base performs a great part in the action of the medicine, he still does not think that its presence is entirely useless; and, in endeavoring to form with arsenic a new salt possessing febrifuge properties, he was naturally led to the employment of quinine.

TREATMENT OF LEAD COLIC BY OPIATES AND WINE.†

BY DR. TRIBERTI.

Purgatives do not possess so uniform and so certain a power over lead diseases as to maintain without dispute the title of specific, which some physicians have accorded to them. We are ever seeking others, because the want of better is felt. Dr. Triberti has been obliged to return to the treatment by opiates, with which he associates wine as soon as the patient is sufficiently well to be able to take some nourishment.

The advantages which he finds in this mode of treatment are:—1st, the promptness of the cure, the treatment lasting only from six to eight days; 2nd, the complete reëstablishment is extremely rapid—an important result, if we take into consideration the long convalescence which necessarily attends diseases treated by sanguine emissions; finally, no deaths occur, whilst by the treatment called charitable, the mortality, according to the imposing authority of M. Orfila, would not be less than 30 per cent.

The dose by which the author commences the treatment is in general from 3 decigrammes of extract of opium in six pills, one to be taken every two hours. At the same time, the patient drinks during the day 750 grammes of an emulsion, to which 60 grammes of syrup of poppies have been added. He also sometimes prescribes frictions on the *rachis* and abdomen, with a liniment composed of—

Olive oil..... 90 grammes.
Sydenham's laudanum .. 15 ,,

The quantity of opium should sometimes be carried to 6 decigrammes per diem, if the symptoms resist the first doses. With respect to wine, 180 grammes are first allowed, and very soon after 500 grammes.

With this treatment, Dr. Triberti cured, during the month of October, 1844, five patients, whose cases he gives in detail. All were cured in the space of time above-mentioned.

According to the author, "the preparations of lead, with their hyposthenisant power, attack the blood vessels and the cerebro-spinal apparatus, and feebly the digestive system; opium is, therefore, the best means to be opposed to them, *because it has a power contrary to the action of preparations of lead* on the living animal fibre, and because its stimulant or hyposthenisant action is exerted on the circulating system and on the cerebro-spinal organ, promptly causing the disappearance of all the morbid phenomena presented by lead colic."

We will say nothing of this very Italian explanation. A more important fact, because it is independent of all hypotheses, is that opiaceous preparations, which ordinarily constipate, combat, on the contrary, in this disease, and are sufficient, without the assistance of any other medicine, for causing alvine evacuations.

With respect to the superiority of this method over others, the observations of Triberti indubitably plead in its favor. But he will himself perceive that many more than five facts are necessary for restoring to reputo a system of treatment which, despite the efforts of MM. Brachet and Bricheteau, is now almost generally abandoned. However,

* *Journal de Chimie Médicale*, May, 1845.

† *Gazetta Medica di Milano*, and *Gazette Médicale de Paris*.

the principal defect in it is the length of time which it requires to effect a cure; and we have seen, on the contrary, that the rapidity of the cure is the principal advantage recognised in it by our author. Is the addition of wine to opiates the cause of this difference?

INVESTIGATIONS CONCERNING DIGITALIS PURPUREA.*

BY PYRAME MORIN, OF GENEVA.

I HAVE read in the *Journal de Pharmacie*, M. Homolle's prize memoir on "Digitalis Purpurea."

Many chemists will probably repeat the interesting experiments detailed at the conclusion of that work; they will prepare digitaline in order to be able to confirm the results which have been pointed out to us. The first point being established, they will carry their investigations further. Others, with a less general object, will determine in what the products which they have obtained differ from that of M. Homolle; they will perhaps arrive at the conclusion that digitalis purpurea contains several principles hitherto unknown; they will also doubt whether the author of the prize memoir has indeed extracted from digitalis a principle in its natural state of existence, or whether he has not made known a combination on a secondary principle. My object is not, now, to criticise the memoir in question: I desire only to make known two principles which I have extracted from digitalis, independently of the bitter principle; I also wish to abridge the labor of those who may make new investigations.

In a memoir which I read, at the commencement of 1844, before the Medical Society of Geneva, I showed that I extracted from digitalis purpurea three distinct principles, which had hitherto been little or not at all studied:—

1st. A bitter principle.

2nd. A fixed acid principle.

3rd. A volatile acid principle.

I shall not now speak of the bitter principle or digitaline.

FIXED ACID PRINCIPLE, OR DIGITALIC ACID.

Preparation.—The leaves of digitalis are treated with hot water. The infusion is acid. It is evaporated in a sand-bath, until it has acquired the consistence of a very thick syrup.

A great proportion of alcohol of 92 or 94 per cent. is added until no more precipitate is produced; a deposit is allowed to form for several days, which should not be bitter, and which would occur if the alcohol were too strong.

The liquid is filtered, then distilled in a sand-bath, in order to give a thick extract.

This extract is treated by pure sulphuric ether, and is maintained for at least an hour at a tolerably high temperature, and which is raised to ebullition. The boiling liquid is uncovered, and this operation is repeated a great number of times.

The ethereal tinctures obtained are acid; they have a slightly yellow greenish tint, which is particularly deep in the first portions. We thus obtain in solution the bitter principle or digitaline and digitalic acid. This operation occupies much time; however, the extract ultimately almost entirely loses its bitterness.

Baryta is added to the ethereal solution, in small pieces and by degrees, some hours being allowed to elapse between each addition. This base is precipitated under the form of a yellowish powder. It is ascertained that enough has been added when, after having stirred the liquid and allowed it to settle, a small piece put in remains whole, or when reddened litmus paper is returned to blue by a prolonged contact with the ethereal tincture.

The liquid is then filtered, and then contains only digitaline, not yet perfectly pure. The precipitate is collected and washed with ether in order to remove all bitterness, then with alcohol of 92 or 98 per cent., until the latter passes with little or no color.

The precipitate is suspended in cold water and mixed with sulphuric acid diluted in a manner as to precipitate the baryta. It is better to leave part of the digitalate of baryta undecomposed, than to employ an excess of acid.

By filtration, a strongly acid reddish solution is obtained. It is slowly distilled in a glass retort, in order to drive off a great part of the water, and great care is taken to avoid the temperature becoming too high.

By cooling, a slight excess of a brown substance is decomposed, it is decanted and a quantity of alcohol of 95 or 96 per cent. is added to the liquid, sufficient for precipitating the digitalate of baryta which may have escaped decomposition. It is evaporated under the air-pump until abundant crystals are formed.

In all these operations, it is necessary to avoid as much as possible the contact of the air, for digitalic acid is converted into a brown substance. Notwithstanding evaporation under the air-pump, the formation of

* *Journal de Pharmacie*, April, 1845.

this colored substance cannot be prevented; the crystals are also formed in the midst of a deep-colored liquid. The more the action of air is avoided, the more pure product will be obtained.

In order to obtain the crystals, they are separated by filtration, and pressed in blotting-paper.

The acid thus obtained is still yellow; it is redissolved in a small quantity of alcohol, evaporated under the air-pump, and white crystals are obtained.

Properties.—Digitalic acid is very soluble in water and in alcohol, but much less in ether.

It crystallises very well in needles, especially when it is obtained from an alcoholic solution.

It possesses a slight odor which is proper to it; by heating it, this odor increases; the vapor formed is suffocating; breathed in very small quantities, it causes coughing.

At a higher temperature, the acid fuses, turns black, and burns with a white flame. It leaves a coal easily incinerated, and which gives no residue. By the action of heat it produces ammonia.

The pure acid is perfectly white, but the action of light, air, and heat, being very great, readily converts it into a black-brown product which colors the solutions, and from which there is much difficulty in freeing it. The greatest precautions should therefore be taken during its preparation. Indeed, pure acid, dissolved in a small quantity of water, may be converted, during evaporation in the sand-bath, into a deeply-colored substance. This substance is itself acid, insoluble in water, sparingly soluble in ether, and very soluble in alcohol.

Digitalic acid also becomes brown more readily under the influence of alkaline substances, therefore, in its preparation, care should be taken to avoid the employment of an excess of baryta.

It has an acid taste which is not disagreeable.

It immediately reddens litmus paper.

It displaces carbonic acid from its combinations, and forms digitalates. It also combines directly with the basic oxides.

The soluble salts readily become yellow in the air.

The salts which I have obtained are—

Digitalate of Soda.—By carefully saturating carbonate of soda with digitalic acid, and evaporating under the air-pump.

It is white and very well crystallised, but in the air it promptly becomes yellow, as, also, does its solution.

It is very soluble in water.

Digitalate of Potassa is obtained like that of soda; it is very soluble and crystallises with difficulty.

Digitalate of Magnesia.—By the acid and carbonate of magnesia; soluble in water.

Digitalate of Baryta.—By saturation of the acid by means of baryta or carbonate of baryta. Soluble in water, insoluble in ether and alcohol; very colorless. In the air and by heat it turns brown.

Digitalate of Lime is obtained like that of baryta, to which it is very analogous: being insoluble in ether and alcohol, it may serve for the preparation of digitalic acid.

Digitalate of Oxide of Zinc.—By treating carbonate of zinc by digitalic acid, filtering and evaporating under the air-pump.

The salt formed is acid notwithstanding an excess of carbonate. A transparent salt is first obtained, having the appearance of a half-dried solution of gum. But, after a few days, it is converted into a host of small crystals, which present the form of a very beautiful arborescence.

This salt turns yellow in the air less readily than the foregoing ones.

Digitalate of Oxide of Lead.—A white, heavy, insoluble salt.

Digitalate of Oxide of Copper.—A greenish, insoluble salt.

Digitalate of Oxide of Silver.—A white salt, soluble in nitric acid, insoluble in water.

Solution of digitalate of soda gives with sulphate of protoxide of iron an abundant flocculent precipitate, and, with acetate of peroxide of iron, gives no change.

These reactions do not admit of digitalic acid, being confounded with gallic acid.

Probably, this acid does not exist combined with digitaline, for, after having treated the aqueous extract by alcohol, an abundant brown precipitate is obtained, which has no more bitterness, as I have stated, but which presents an acid reaction, and from which, indeed, digitalic acid may be extracted.

The properties of digitalic acid, appeared to me well established. It may be objected that it is formed under the influence of the substances of which I make use in order to obtain it, and especially by the employment of baryta.

I will reply by the following observations:—

1st. The juice of digitalis purpurea presents an acid reaction the moment it is obtained.

2nd. The product of the treatment of the dried leaves by water, alcohol, or ether, is likewise very acid.

3rd. If the alcoholic or ethereal solution obtained during the preparation, be saturated by oxide of lead, the liquid remains bitter,

and a precipitate is formed; if, after having washed it with water and alcohol, it be treated by hydrosulphuric acid, whose excess is afterwards driven off, a very acid solution is obtained, from which the acid may be extracted by crystallisation.

This process is perhaps better than that which I have described, when it is not desired at the same time to collect the bitter principle; here the action of the base employed is not energetic enough for the formation of a secondary product.

4th. If the basic action of oxide of lead be doubted, it may be replaced by carbonate of lead which leads exactly to the same results.

5th. Up to the stage of operation at which baryta is employed, water, alcohol, and ether alone are used. I employ a base for separating digitalic acid from digitaline, as facilitating the operation, no reagent could be employed; indeed, by simple evaporation, a crystalline mass is obtained; by pressure in blotting paper, and repeated crystallisations, the pure acid is collected with the properties which I have indicated.

VOLATILE ACID PRINCIPLE, OR ANTIRRHINIC.

Preparation.—The leaves of digitalis purpurea are distilled with a great quantity of water, so long as the product is odorous and reddens litmus paper.

The distilled liquid should be saturated by a slight excess of caustic baryta, then evaporated to dryness in a sand-bath. A product is obtained having an odor analogous to that of benzoin.

The powder obtained is mixed with an equal bulk of oxalic acid, and then distilled on a sand-bath in a glass retort. A highly odorous product is obtained; water is added to the retort, the distillation is continued, and so on for several times, so long as the product possesses a powerful odor.

It is afterwards rectified on a sand-bath, over chloride of calcium; the first product obtained is a saturated aqueous solution on the surface of which floats the pure acid under the form of oily drops.

Observations.—The quantity of products obtained is excessively small, as might be expected, since digitalis possesses but very little odor.

The fresh plant gives a more abundant product than the dried plant.

Instead of oxalic acid dilute sulphuric acid may be employed; however, by concentration, the latter readily decomposes the mixture contained in the retort.

Any base may be employed instead of baryta.

Properties.—Antirrhinic acid is a volatile principle of oily appearance, colorless, powerfully reddening litmus paper.

It dissolves in alcohol.

In contact with water, it is converted into white pellicles, which are probably a hydrate, and which are finally dissolved; its taste is disagreeable.

Its odor is analogous to that of bruised fresh digitalis, and which, in the dried plant is found in the first product of distillation.

The concentrated solution, reoperated several times, readily produces headache, and even giddiness.

Antirrhinic acid presents much analogy to valerianic acid; the preparation is almost the same; it scarcely differs from it in anything.

I have studied this acid in a manner still too incomplete to assign to it a name which calls to mind one of its characteristic properties, but, giving the name of digitalic acid to a well determined acid, I propose for the former the name of antirrhinic acid, in order to indicate that I have found it in several species of the genus digitalis, and even in other genera of the family of *antirrhinaceæ*.

BOOKS, &c., RECEIVED.

"Practical Facts in Chemistry," by Robt. Best Ede.—"The Railway Register, and Record of Engineering and Public Enterprise, &c.," edited by Hyde Clarke, Esq., No. VI., May, 1845.—"The Dublin Hospital Gazette," No. VII., May 15, 1845.

TO CORRESPONDENTS.

We should feel obliged to our correspondent "F. C." (Great Yarmouth), when next he asks a favor at our hands to prepay the postage.

ERRATA IN PRESENT NUMBER.

Throughout first article under the head of Chemistry, for "Ag." read "Aq." In part of our impression—Page 257, 2nd col., for "Jaspipet," read "Gas Pipette;" page 259, 1st col., 6th line, for "200," read "250."

NOTA BENE.—*All Communications, Books for Review, and Substances to be Analysed, must be addressed "To the Editors of The Chemist, 310, Strand, London." Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

OXYSULPHURET OF CHLORINE.*

MILLON found,† on treating sulphur with an excess of chlorine, that when the latter contained humidity, in addition to the sulphuret of chlorine a crystalline body was produced. He succeeded in preparing greater quantities of the latter substance, by putting proper proportions of humid chlorine gas in contact with sulphuret of chlorine. The sides of the flask in which the operation was conducted were soon covered with large, transparent, colorless crystals, to which, however, some sulphuret of chlorine still remained attached so tenaciously that, even after having been treated for 10 or 12 hours with dry chlorine gas, they could not be entirely removed.

M. Millon has not succeeded in analysing this body in its freshly formed state, as it is directly decomposed by water, alcohol, and even by damp air. By fusing it, however, in a tube, it is after some time converted into an isomeric and very mobile liquid, and may be treated with water and alcohol, unlike the original crystals, which, by a similar treatment, are decomposed with effervescence. The new product is not miscible with the said liquids, but is, however, gradually decomposed by them in the presence of either sulphuric, sulphurous, or hydrochloric acid. M. Millon took advantage of this decomposition in making the analysis, and attained the formula—$S^2 O^3 Cl^2$. The new product differs, therefore, from the combination $SO^2 Cl$ discovered by Regnault, and from that described by H. Rose, with the formula $S^2 O^5 Cl$.

—nt—

AN INVESTIGATION AND NEW ANALYSIS OF THE POLYBASITE OF BERZELIUS.‡

BY EDWARD J. CHAPMAN.

THE name of polybasite (πολυς, βασις) was bestowed by Berzelius, on this natural, though rare sulphur-salt—the Eugenglanz of the German mineralogists—on account of the large predominance of the basic over those of the electro-negative elements, in its composition—the former amounting in one hundred parts to more than seventy.

The ore has hitherto only been met with in some of the mining districts of Mexico, but it is conjectured to occur also in other localities, and especially at Freyberg, in Saxony, where it may have been referred to some other of the silver compounds which occur there.

It has a metallic aspect, and black color and "streak," and belongs to the three-and-one axis, or rhombohedral system of crystallisation. Its specific gravity varies from 6·2 to 6·3, its hardness from 2·0 to 3·0; and it usually occurs in small and very flat hexagonal prisms, or in their amorphous masses, exhibiting here and there traces of the hexagonal form.

Before the blow-pipe, it emits the combined odors of sulphur and arsenic on shifting its position in the flame, and easily fuses into a malleable globule of a light color, surrounded by a white areola of oxide of antimony. Besides this areola, a very slight yellow sublimate of oxide of lead may be obtained, with management, whilst the arsenic is deposited still further from the assay. To borax on the platinum wire, it imparts the colors indicative of the presence of copper, and by cupellation it yields a very considerable bead of silver.

One analysis only of this scarce mineral has, as yet, been published—that by H. Rose, which afforded him the following results:—

Sulphur	17·04
Antimony	5·09
Arsenic	3·74
Silver	64·29
Copper	9·93
Iron	0·06
	100·15

From this analysis, the rather indefinite formula—

$$\left.\begin{matrix} Sb^2S^3 \\ As^2S^3 \end{matrix}\right\} 9\,(AgS, Cu^2S)$$

* Liebig's *Jahresbericht*, April 12, 1845.
† *Journal de Pharmacie*, vol. vi., p. 413.
‡ Communicated by the author.

has been deduced; and I can find none other more expressive in any of the modern chemical or mineralogical works, in which mention is made of the polybasite.

Having had by me lately a small but apparently very pure specimen of this substance, I subjected it to an analysis, performed in the ordinary manner, but with some care, the methods and results of which, with the deductions derivable from it, are here subjoined.

Twenty grains, reduced to the finest powder and totally freed from the gangue (calc spar) by a diluted acid, were dried and weighed out for the analysis, and then treated with strong nitric acid, which dissolved the greater part, leaving however a small residue. This residue was afterwards separated from the clear solution, and digested in hydrochloric acid until dissolved.

Hydrochloric acid with water was then added to the nitric solution, and the precipitate, consisting of the chlorides of silver and lead, filtered off, and well washed with boiling water, which carried off the latter, leaving the chloride of silver pure. This was now properly dried and weighed, when it yielded 16·80 grs., of which 12·655 would be silver, or 63·28 per cent.

The lead chloride was thrown down from its aqueous solution by sulphuric acid. The sulphate of lead, properly dried, amounted to 0·25 grs., of which 0·171 would be lead, or 0·85 per cent.

The two solutions were now mixed, and the sulphuric acid precipitated by chloride of barium. The sulphate of baryta thus produced weighed 22·20 grs., of which 3·06 would be sulphur, or 15·32 per cent.

Hydrosulphuret of ammonia was then added in excess to the neutralised solution, so as to leave only the copper precipitate undissolved. The sulphuret of copper was then redissolved in nitric acid, the solution diluted, and the copper precipitated by an excess of potash, the whole being boiled together. The protoxide of copper amounted to 2·10 grs., of which 2·24 would be copper, or 11·20 per cent. In this precipitate, however, a very minute quantity of peroxide of iron was subsequently detected.

To the original solution, diluted hydrochloric acid was added, which threw down the sulphurets of antimony and arsenic. This precipitate carefully heated, in a proper apparatus, in a current of dry hydrogen gas, left a regulus of antimony weighing 1·35 grains, equalling 6·75 per cent.

The above numerical results added together leave 0·524 for the expelled arsenic, or 2·62 per cent.

It was not thought necessary to determine this small quantity of arsenic in a more direct manner, as its presence was fully demonstrated during the passage of the hydrogen, as well as by the previous blowpipe examination.

These results will therefore yield the following per centage analysis, and atomic deductions:—

Sulphur	15·32	0·0762	11
Antimony....	6·75	0·0083	2
Arsenic.....	2·62	0·0059	
Silver	63·28	0·0470	6
Lead.....	0·85	0·0007	
Copper with a trace of iron	11·20	0·0283	4
	100·02		

from which the accompanying formula may be obtained:—

$$6\,AgS,\left\{\begin{matrix}Sb^2\\As^2\end{matrix}\right\}S^3+2\,Cu^2S.$$

From this analysis, the polybasite appears to be composed of one atom of the psaturose or brittle sulphuret of silver, consisting of $6\,AgS+Sb^2S^3$ according to Rose's formula, and two atoms of the chalkosine or copper glance—Cu^2S.

Of the allied argentiferous sulphur-salts, the most simple is the miargyrite $AgS+Sb^2S^3$; secondly will follow the isomorphs, argyrithrose and pronstite, or dark and light red silver ores, $3\,AgS+Sb^2S^3$, and $3\,AgS+As^2S^3$; thirdly, the psaturose $6\,AgS+Sb^2S^3$; and, fourthly, the donacargyrite $2\,AgS$, $Sb^2S^3+3\,PbS$, Sb^2S^3.

Kensington, May 27th.

INVESTIGATIONS OF SEVERAL NATURAL AND ARTIFICIAL COMBINATIONS OF PHOSPHORIC ACID.*

BY C. RAMMELSBERG.

(*Continued from page* 257.)

LAZULITHE AND BLAUSPATH.

These two minerals were first examined by Kloprath. Fuchs, however, discovered in them, as well as in the wawellite, a large amount of phosphoric acid; in 1818 he communicated the analysis of the lazulithe of Rädelgraben, near Werfen,† and soon after Brandes examined the blauspath of Krieglach, in Steiermark.‡ These are, to my knowledge, the only complete analyses of the two substances, the results being the following:—

* Poggendorff's *Annalen der Physik und Chemie*, Feb., 1845.

† Schweigger's *Journal*, vol. xxiv., p. 373.

‡ *Ibid.*, vol. xxx., p. 385.

	Lazulithe, by Fuchs	Blauspath, by Brandes
Silica	2·10	6·50
Phosphoric acid	41·81	43·32
Alumina	37·73	34·50
Magnesia	9·34	13·56
Protoxide of iron	2·64	0·80
Lime	..	0·42
Water	6·06	0·50
	97·68	99·60

There is no doubt that the amount of water given by Brandes is not correct; the blauspath, as we see, contains almost as much water as the lazulithe.

In my analyses I employed a dark blue lazulithe from the Fishbacher Alps, in the county of Gratz, and a light-colored blauspath from Fressnitzgraben, near Krieglach, in Steiermark, which I obtained from the rich and beautiful collections of Styrian minerals of the Joanneum, at Gratz, and the I. R. Aulic Museum of Minerals at Vienna.

I ascertained the specific gravity of

the lazulithe to be in one experiment	3·106
In another	3·123

According to Fuchs, the lazulithe

of Werfen	3·057
The blauspath I found to be	3·021
Brandes found the same to be	3·001
Klaproth	3·046

The analysis of these substances, owing to the presence of alumina, is attended with greater difficulties than even that of the wagnerite, and, notwithstanding pains and care, and repeated attempts, I have not succeeded in obtaining concordant results, though the same might be considered satisfactory enough to establish the formula. I will here indicate the method on which I decided.

The mineral was calcined, and the amount of water determined. The protoxide of iron passes over by this operation into the oxide, and becomes of a yellow or brown color. The fine powder was fused with carbonate of soda, and the mass decomposed by hydrochloric acid, as is usually done in the analysis of silicates. After having separated the silica, the acid liquid was digested and boiled in a platinum capsule with a concentrated solution of potassa, by which the greater portion of the phosphoric acid and alumina were dissolved. From the alkaline liquid, an addition having been made of some hydrochloric acid, phosphate of alumina was thrown down by means of ammonia, and its constitution was determined, either by fusing it with silica and carbonate of soda, or by resolving it with sulphuric acid and sulphate of potassa (*vide* the analyses of the artificially-prepared phosphates of alumina to be treated upon in the sequel). The phosphoric acid left in the liquid was thrown down by chloride of calcium.

The portion left undissolved by the potassa, which contained the oxide of iron, lime and magnesia, besides some alumina and phosphoric acid, was dissolved in hydrochloric acid, and thrown down by ammonia. By this treatment, all substances were thrown down, except the greater quantity of lime and magnesia, which were obtained in the usual manner from the filtered mass. The precipitate was calcined with carbonate of soda with the addition of a little caustic potassa, washed with water, and the liquid was treated in the same manner as the solution of potassa first obtained, to which it was added, and the remainder, on being dissolved in hydrochloric acid, indicated the amount of iron, lime, and magnesia.

The phosphoric acid was not always directly determined; but the numbers enclosed in the brackets indicate the quantities found. The following are the results of the analyses of the lazulithe, and of 3 of the blauspath:—

LAZULITHE.

Silica	0·53	4·44	44·44	1·76	8·04
Phosphoric acid	42·19	41·58	44·91	40·60 [46.1]	43.26 [49·8]
Alumina	29·42	31·63	26·39	32·10	24·76
Magnesia	10·61	8.60	10·69	9·37	9·81
Protoxide of iron	10·55	6·69	6·18	9·37	7·21
Lime	1·11	1·38	2·03	0·77	1·11
Water	5.59	5·68	5·36	6·03	5·81
	100·	100·	100·	100·	100.

BLAUSPATH.

Silica	6·64	6·64	12·56
Phosphoric acid	38·05 [43·05]	34·21	40·12 [42·77]
Alumina	33·93	28·05	24·12
Magnesia	12·04	11·39	10·67
Protoxide of iron	1·54	1·77	2·56
Lime	1·32	1·54	4·67
Water	6·48	6·40	5·30
	100·	100·	100·

The differences occurring, it appears, chiefly relate to phosphoric acid and alumina, the amount of the latter, in most instances, being probably too small, which is owing to the methods of separation. The silica is foreign to the mineral; it is, however, associated with it as quartz; its quantity must, therefore, be subtracted, and then the following numbers of the constituent parts of the two minerals will result:—

LAZULITHE.

Phosphoric acid	42·41	43·84	46·99	41·33	47·04
Alumina	29·58	33·09	27·62	32·68	26·92
Magnesia	10·67	9·00	11·19	9·54	10·67
Protoxide of iron ..	10·60	6·69	6·47	9·54	7·84
Lime	1·12	1·44	2·12	0·77	1·21
Water	5·62	5·94	5·61	6·14	6·32
	100·	100·	100·	100·	100·

BLAUSPATH.

Phosphoric acid	40·95	47·36	48·73
Alumina	36·22	30·05	27·48
Magnesia	12·85	12·20	12·16
Protoxide of iron	1·64	1·89	2·91
Lime	1·42	1·65	5·32
Water	6·92	6·85	6·04
	100·	100·	100·

The following table of the quantities of oxygen may serve as the best means of deciding as to which are the proportions most likely to be correct:—

	RO.	Al^2O^3	P^2O^5.	HO.
Lazulithe. I.	6·85	13·81	23·76	4·99
II.	5·40	15·45	24·56	4·28
III.	6·39	12·89	26·33	4·98
IV.	6·07	15·26	23·16	5·45
V.	6·25	12·57	26·36	5·61
Blauspath. I.	5·74	16·91	22·94	6·15
II.	5·61	14·03	26·54	6·08
III.	6·85	12·83	27·30	5·37

It appears that in most of the experiments the amount of oxygen of the alumina is that of the bases R. That of the phosphoric acid is seemingly twice as much as that of the alumina, and the oxygen of the water might be equal to that of the bases R. Hence the proportions of the quantities of oxygen are, as $RO : Al^2O^3 :: PO^5 : HO = 3 : 6 :: 12 : 3$, or as phosphoric acid does not admit of the number $12 = 3 : 6 :: 12 \cdot 5 : 3$, which clearly appears from several experiments (lazulithe III., V.). The expressions for lazulithe and blauspath most likely to be correct are, consequently, the following:—

$$2\,RO^3PO^5 + 4\,Al^2O^3\ 3\,PO^5 + 6\,HO.$$

The first member of this formula represents the common point of saturation, and the second an anhydrous wawellite deprived of fluorine.

Lazulithe and blauspath are, therefore, one and the same mineral, and are distinguished merely by the fact that the first contains a greater amount of protoxide of iron, it being isomorphous with magnesia, and hence its dark coloration.

The phosphate of protoxide of iron being by itself colorless, and the double phosphates with the basic phosphate of oxide of iron exhibiting a blue coloration (as will appear in the following), it is not impossible that the lazulithe contains likewise some iron, which, however, is not likely to be ascertained by experiments.

AMBLYGONITE.

To the most rare, and at the same time, by its chemical constitution, extremely remarkable, mineral substances, undoubtedly belongs the amblygonite, discovered by Breithaupt; it exclusively occurs in the granite of Saxony, near Penig (at Chursdorf and Arnsdorf), and at Zinnwald.

We are indebted to Berzelius for all that we know relative to the chemical constitution of the amblygonite: he found in it 11 per cent. of lithium, besides alumina, phosphoric acid, and fluorine; the small quantity of substance he had at hand, however, did not permit him to determine the individual substances.*

Plattner afterwards found that the amblygonite likewise contained soda, as it renders the flame of the blowpipe yellow, in such a manner that the reaction of the lithium almost entirely disappears.†

By the kindness of Professor Breithaupt,

* Gilbert's *Annalen*, vol. lxv., p. 321.

† *Die Probirkunst mit dem Löthrohr*, p. 97.

who has supplied me with a sufficient quantity of this rare matter, I was enabled to examine it more minutely. I employed the gray-white amblygonite of Arnsdorf, near Penig, the specific gravity of which, according to the above mineralogist, is = 3·11.

The analysis of no mineral is attended with more difficulties than that of the amblygonite, as the phosphoric acid must be separated from the alumina, the fluorine requires a determination, and, besides, all the three alkalies are present; I have likewise found potassa in it. Much time and many fruitless attempts were required until a satisfactory result could be obtained.

I tried to decompose the amblygonite by fusing it with chloride of barium, with lime, and with lime and silica, but without success; finally, I adopted the following method:—

The finely-powdered mineral is heated with sulphuric acid in a platinum vessel until the whole of the fluorine is driven off, the solution diluted with water, and precipitated by ammonia. A precipitate is thus thrown down containing alumina and phosphoric acid; this precipitate must be decomposed by fusing with carbonate of soda and silica, or with acid sulphate of potassa (in a manner to be described hereafter).

The filtered ammoniacal mass, containing the alkalies and the remainder of the phosphoric acid, is to be treated with chloride of calcium, to remove the latter acid; and the excess of lime is separated by oxalic acid. The alkaline sulphates left behind, after evaporation and calcination, were determined in such a manner that the potassa was first thrown down by chloride of platinum, and then the weight of the sulphate of soda and lithium ascertained, and finally the amount of acid determined. Phosphate of soda cannot be made use of as a sure means of quantitatively determining the amount of lithium, as I will show on another occasion. It now remains to determine the fluorine. By the simple method proposed by Wöhler this cannot be brought about, as the decomposition requires a high temperature. The most finely divided powder of the amblygonite was put together with silica and sulphuric acid, and distilled, the fluosilicic gas directed into water, with the precautions necessary to prevent any gas from escaping. The liquid was placed in a platinum capsule, supersaturated with carbonate of soda, and finally, the carbonic acid having been removed, precipitated by ammonia and chloride of calcium.

The following are the results of the analyses instituted to determine the amount of fluorine, executed in the manner described above, and of two separate determinations of the alumina:—

	I.	II.	III.	IV.	V.
Phosphoric acid	46·00	47·15	—	—	—
Alumina	36·26	38·43	36·62	36·89	—
Lithia	6·36	7·03	—	—	—
Soda	5·48	3·29	—	—	—
Potassa	Not determined.	0·43	—	—	—
Fluorine	—	—	—	—	8·11

The total sum of the alkalies, therefore, amounts to nearly 11 per cent., though one portion only belongs to the lithium.

The amblygonite cannot be considered as a combination of lithium and soda with phosphate of alumina, as the amount of the fluorine is not sufficient to saturate the alkaline metals. It must, therefore, consist of phosphate of alumina and lithia (soda), and fluoride of aluminium and lithium (sodium). Starting, therefore, from the amount of fluorine, and basing ourselves on the first analysis in the calculation of the quantities required of aluminium and alkaline metals, taking lithium and sodium in the proportions resulting from the analyses, namely 5 atoms of lithium to 2 atoms of sodium, it will appear—

$\frac{1}{4}$ of 8·11 fluorine = 6·08 require 2·97 Al = 5·57 Al^2 O^3 = 9·05 Al^2 Fl^3
$\frac{1}{4}$ of 8·11 „ = 2·03
$\frac{5}{7}$ of 2·03 „ = 1·45 „ 0·5 Li = 1·12 Li = 1·95 Li Fl
$\frac{2}{7}$ of 2·03 „ = 0·58 „ 0·72 Na = 0·97 Na O = 1·30 Na Fl

And the composition is then as follows:—

Phosphoric acid	48·00	26·9	
Alumina	30·69	14·3	
Lithia	5·21	2·89	4·04
Soda	4·51	1·51	
Fluorine	8·11		
Aluminium	2·97		
Lithium	0·50		
Sodium	0·72		

The oxygen of the alumina in the double phosphate is only half the amount of the phosphoric acid, and the oxygen of the alkalies is only one-third of that of the former body; whereas the alumina of the phosphates contains five times as much oxygen as the alumina produced from the fluoride of aluminium (2·97 Al = 5·57 Al^2 O^3 = 2·6 oxygen).

Hence we obtain for the amblygonite the formula—

$$5\ RO,\ 3\ PO^5 + 5\ Al^2\ O^3,\ 3\ PO^5$$
$$RFl + Al^2\ Fl^3$$

This expression contains very simple proportions, as it appears at one glance—

In the upper member the O of the R : $RO^3 = 1 : 3$, $PO^5 = 1 : 1$; in the lower member Fl of R : R = 1 : 3; the metals in both the members = 1 : 5.

Since, as has been stated, the analyses gave 5 atoms of lithia to 2 atoms of soda, this may serve as a starting point in the calculation, by which we may assume the following to be the composition of the amblygonite :—

Phosphoric acid....	47·87	
Alumina.....	28·27	87·33
Lithia.....	5·76	
Soda	4·98	
Fluorine.....	8·36	
Aluminium.....	3·06	12·67
Lithium	0·51	
Sodium	0·74	

And the products of the analysis must then be the following :—

Phosphoric acid.....	47·87
Alumina	34·46
Lithia	6·90
Soda	5·98
Fluorine	8·36
	103·57

It has been stated already that from a solution of the amblygonite in sulphuric acid by ammonia, phosphate of alumina is thrown down. The quantity of the same amounted in one experiment to 68·33 per cent. of the mineral, and consisted of—

		Oxygen.
Phosphoric acid...	31·71	17·7
Alumina	36·62	17·1

It was, therefore, $5\ Al^2\ O^3,\ 3\ PO^5$, that is the same phosphate, which the formula of the amblygonite induces to suppose.

PHOSPHATE OF ALUMINA AND LITHIA.

If a saturated solution of the phosphate of alumina thrown down from a solution of alum by phosphate of soda, after having been washed by potassa ley, be prepared, and then a solution of chloride of lithium added, a bulky precipitate is formed, containing phosphoric acid, alumina, and lithia. This reaction was first observed by Berzelius. The combination was prepared in the above manner, washed with cold water, and dried at a gentle heat.

It represents a white very loose powder, which on being heated gave up a large quantity of water. With the view of being analysed, it was calcined, the residue left dissolved in hydrochloric acid, and the alumina, together with the greater portion of the phosphoric acid, thrown down by ammonia, when the remainder and the lithia were determined in the above manner. In another experiment the calcined mass was fused with silica and carbonate of soda; the lithia was in this case lost sight of.

The following were the results :—

	I.	II.	III.
Phosph. acid...	22·81	22·73	22·61
Alumina.....	35·98	38·11	
Lithia.....	7·35	—	
Water.....	32·09	32·10	
	98·23		

A very little potassa was likewise found in the combination, which may perhaps be attributed to incomplete washing, which, owing to the mucilaginous condition of the precipitate, appeared to be unavoidable. If we are warranted in taking the loss as lithia, the quantity of which, from the manner in which the operation was conducted, must certainly be too little, it amounts to 9·12 per cent., and the quantities of oxygen of LiO, Al^2O^3, PO^5 and HO are then = 5·05 : 16·8 : : 12·7 : 28·5 approach = 2 : 6 : : 5 : 10, according to which the combinations contained are—2 atoms of lithium, 2 atoms of alumina, 1 atom of phosphoric acid, and 10 atoms of water, and are to be expressed by the formula—

$$6\ LiO,\ 2\ PO^5 + Al^2\ O^3\ PO^5 + 15\ HO.$$
$$5\ Al^2\ O,\ 3\ HO.$$

or $6\ LiO,\ PO^5 + 6\ Al^2\ O^3,\ PO^5 + 3\ OHO$, which answer to the following composition :—

Phosphoric acid.....	24·36
Alumina	35·08
Lithia	9·84
Water	30·72

(To be continued.)

ON THE COMPOSITION OF THE CHLORATES.*

A. WACHTER† has examined a series of chlorates; the results of his labor are the following :—

Chlorate of Soda, $Cl\ O^5\ SoO$, is decomposed almost at the same temperature as the potassa salt, leaving behind chloride of sodium with an alkaline reaction.

Chlorate of Lithia, $ClO^5,\ LiO + HO$ forms a radiated mass: it is very deliquescent, fuses at 122°F., and at 284°F. it commences to yield water, disengaging oxygen and some chlorine. After the decomposition, chloride

* Liebig's *Jahresbericht*, April 12, 1845.

† *Journal für Prakt. Chemie.*, xxx., p. 321.

of lithium with an alkaline reaction is left behind. It is highly soluble in alcohol.

Chlorate of Ammonia, ClO^5, $NH^3 + HO$, is very soluble in water, but sparingly soluble in alcohol, crystallising indistinctly. At 215°F. it is resolved, with a red light, into water, nitrogen, chlorine, oxygen, hydrochloric acid gas, and sal-ammoniac.

Chlorate of Baryta, ClO^5, $BaO + HO$, crystallises with a disengagement of light, forming rhomboidal prisms. At 248°F. it loses its water; at 482°F., it disengages oxygen, and fuses at 752°F.

Chlorate of Strontia, ClO^5 SrO, forms large pyramidal crystals; it deliquesces, is insoluble in alcohol, and is decomposed at the same temperature as the salt of baryta.

Chlorate of Lime, ClO^5 $CaO + 2HO$. Rhomboidal prisms and is deliquescent. At 212°F. it fuses briskly in its water of crystallisation, and is decomposed like the salt of baryta.

Chlorate of Magnesia, ClO^5 $MgO + 6HO$, forms crystalline crusts, is very deliquescent, readily soluble in alcohol, and fuses at 104°F.; at 248°F. it loses its water; by raising the heat, a mixture of magnesia with chloride of magnesium is left behind, and, finally, pure magnesia. All the above mentioned salts develop, besides oxygen, a small portion of chlorine, invariably leaving an alkaline residue.

Chlorate of protoxide of Manganese cannot be prepared as a solid, and forms a colorless solution.

Chlorate of protoxide of Iron is readily decomposed into chlorate of peroxide of iron, oxide of iron, and chloride of iron. It is not known to exist as a solid.

Chlorate of protoxide of Tin is only known in the state of colorless solution. It is easily decomposed, with a disengagement of heat, and with violent explosions, leaving behind a gelatinous mass of hydrated oxide of tin, chloride of tin, and free hydrochloric acid.

Chlorate of Zinc, ClO^5, $ZnO + 6HO$, is crystalline, deliquescent, soluble in alcohol. Heated in oxygen and chlorine, it is decomposed; by applying a strong heat, pure oxide of zinc remains behind.

Chlorate of Nickel, ClO^5, $NiO + 6HO$, crystallises in fine regular octahedrons of a deep green color; it is deliquescent, soluble in alcohol, and is resolved into chlorine, oxygen, and a residue of oxide of nickel, and chloride of nickel; at a strong red heat, oxide of nickel is left behind.

Chlorate of protoxide of Cobalt, ClO^5, $CoO + 6HO$, crystallises in cubes mixed with octahedrons, and is resolved like the salt of oxide of nickel.

Chlorate of Copper, ClO^5, $CuO + 6HO$, crystallises in octahedrons, deliquesces, and is soluble in alcohol. At 212°F., it is decomposed into some bubbles of gas, and a green body, which is decomposed in its turn at 500°F., and is insoluble in water, but soluble in acids. It seems to be a basic chlorate, as its solution in nitric acid does not precipitate silver.

Chlorate of Lead, ClO^5, $PbO + HO$. By cooling the solution, rhombic prisms are obtained, which, on exposure to the air, very soon become dull and opaque. They do not deliquesce, but are readily dissolved in water and alcohol; on being heated, they leave behind a basic chloride of lead of a yellow color, invariably of the composition $PbO + 2PbCl$.

Chlorate of Silver, ClO^5, AgO, is soluble in water and alcohol; with organic substances it is decomposed with far more violence than the potassa salt. It forms exceedingly distinct prismatic crystals. With ammonia, it forms a double salt in prismatic crystals, ClO^5, $AgO + 2NH^3$.

Chlorate of protoxide of Mercury, ClO^5, Hg^2O, forms fine prismatic crystals, soluble in alcohol and water; it furnished, on applying heat, protochloride of mercury, oxygen, and mercury.

The salt of the peroxide behaves in the same manner.

Chlorate of Cadmium, ClO^5, $CdO + 2HO$ acts in a manner similar to the nickel salt. Walther has not succeeded in preparing the salt of bismuth, because basic salts were formed.

—nt—

ON THE ANHYDROUS SULPHATE OF AMMONIA.*

We mentioned in last year's report,† that Jacquelain has investigated the combination of anhydrous sulphuric acid with anhydrous ammonia, examined by H. Rose, and assigned to it the formula $4\,SO^3 + 3\,NH^3$.

H. Rose‡ has recently communicated some remarks on Jacquelain's work.

In 1834 Rose examined anhydrous ammonia, and observed the difference of its action, when dissolved in water, from that of common sulphate of ammonia dissolved in the same liquid.

The analysis of the combination led him to the formula SO^3, NH^3, though the quantity of sulphuric acid found in some analyses was greater. Rose resumed the same sub-

* Liebig's *Jahresbericht*, April 12, 1845.

† Liebig's *Annalen*, vol. xlviii., p. 206.

‡ Poggendorff's *Annalen*, vol. lxi. p. 201.

ject in two subsequent treatises* (1839 and 1840).

In the first,† he showed that, by treating ammonia with anhydrous sulphuric acid, even though a considerable excess of the latter be employed, a salt in fine large crystals may easily be obtained if the aqueous solution of that combination be treated with carbonate of baryta. After having separated the sulphate of baryta by cautious evaporation at a gentle heat, or still better *in vacuó*, crystals of neutral anhydrous ammonia are obtained.

In the second treatise, Rose has shown that the crystals thus obtained differ in some respects from the anhydrous sulphate of ammonia directly obtained, though they are both of the same composition (SO^3, NH^3).

The solution does not render turbid solutions of baryta or lead salts, even after having remained in contact for a long time. On adding hydrochloric acid, after 12 hours a slight precipitation of sulphate of baryta takes place. The other variety of the salt, formed by direct saturation of ammonia with sulphuric acid, and in which, even with the aid of the microscope, a crystalline structure cannot be detected, produces in solutions of the baryta salts a slight turbidness; but even after the lapse of months, the sulphuric acid is not completely precipitated.

These two combinations are also distinguished in some other respects, wherefore Rose called the first sulphate of ammonia, and the latter parasulphate of ammonia. He, moreover, described a salt which crystallised out of the mother liquor of the parasulphate of ammonia, and which may be considered as a combination of 1 atom of parasulphate of ammonia, and 1 atom of sulphate of oxide of ammonium.

In the treatise published on this matter by Jacquelain, he gives it as his opinion, that the combination $4\ SO^3 + 3\ NH^3$, analysed by himself, is nothing else than Rose's sulphate of ammonia, and that ¼th of ammonia which Rose found in it, is only mechanically mixed with the sulphuric acid, and not chemically combined.

Rose directs attention to the fact, that Jacquelain prepared his substance in an entirely different manner from his own, and that the same possesses other properties than he described. Jacquelain fuses the powdery combination, and directs a stream of ammoniacal gas into the fused mass. The solution of the white crystalline mass obtained does not render turbid a solution of chloride of barium, previously acidulated with hydrochloric acid.

If the sulphate of ammonia were mechanically admixed only with the ammonia, the odor of the latter body would be perceptible, as occurs whenever the ammonia is not contained in definite chemical proportions. Such, however, according to H. Rose, is not the case.

Finally, the precipitate thrown down by treating an ammoniacal solution of chloride of barium with a solution of the combination prepared by Jacquelain, and called sulphamide, is constituted according to the formula, $3\ SO^3, 2\ BaO, NH^3$, whereas the sulphate of ammonia, when cold, throws down a precipitate of common sulphate of baryta.

Rose concludes from this, that Jacquelain's sulphamide, and the combination called by himself sulphate of ammonia, are two different bodies. —nt—

* Liebig's *Annalen*, vol. xii., p. 191.
† *Ibid.*, vol. xxxii., p. 217.

SULPHATE OF LEAD WITH SULPHURIC ACID.*

Dupasquier† found that sulphuretted hydrogen produces no black coloration in concentrated sulphuric acid after being boiled with sulphate of lead. On mixing the acid with water, sulphate of lead is thrown down, on which sulphuretted hydrogen immediately produces the known reaction; every strong base acts of course in the same manner as water. —nt—

ON THE ANHYDROUS SULPHITE OF AMMONIA.‡

It is known that Langlois, some time since, discovered a new acid of sulphur, which has the formula S^3O^5, and is formed by digesting the bisulphate of potassa, at a moderate heat, with sulphur. Besides sulphate of potassa, the potassa salt of the new acid is produced. It does not appear what part the sulphur acts in this process, as the constituent parts of the bisulphate of potassa are alone sufficient to form the said products:—

$$2\ (2\ SO^2 + KO) = SO^3, KO + S^3O^5, KO.$$

The new acid may be considered as a combination of sulphuric with hyposulphurous acid; and in a great many cases it is indeed resolved into these two acids, the latter of which is again decomposed into sulphurous acid and sulphur. H. Rose§

* Liebig's *Jahresbericht*, April 18, 1845.
† *Jour. für Prakt. Chem.*, vol. xxxi., p. 417.
‡ Liebig's *Jahresbericht*, April 12, 1845.
§ Liebig's *Annalen*, vol. xxvii., p. 194.

has shown, some time ago, that dry sulphurous acid, and dry ammoniacal gas combine into one reddish-yellow body with the formula, $2\ SO^2 + NH^3$. By merely attracting humidity, this combination loses its reddish-yellow color.

The solution, especially after having been kept for a long time, acts like a combination of sulphate and hyposulphite of ammonia. According to the more recent experiments of H. Rose,* by dissolving the combination $2\ SO^2 + NH^3$, a salt is formed, containing Langlois' acid, to which Berzelius assigned the name of trithionic acid. —nt—

SULPHURETTED HYPOSULPHATE OF POTASSA.†

According to M. Plessy, the above salt is easily obtained, if a current of sulphurous acid be directed through a saturated solution of hyposulphite of potassa in 7 parts of water, and 1 part of alcohol, when the same is deposited as crystals. The fine crystalline powder is dissolved in water of from 122° to 140°F., and filtered off from the sulphur. The solution, after being treated with $\frac{1}{4}$th of alcohol, furnishes, on cooling, beautiful quadrilateral obliquely truncated prisms of sulphuretted hyposulphate of potassa.

—nt—

TETRA-SULPHURET OF AMMONIUM.‡

Fritzsche,§ by alternately directing ammonia and sulphuretted hydrogen into a solution of the pentasulphuret of ammonium, succeeded in obtaining the tetra-sulphuret of ammonium, which separates in the form of a crystalline mass. It is decomposed with the same facility as the pentasulphuret of ammonium. —nt—

INVESTIGATIONS CONCERNING CHROMIUM.‖

BY E. PELIGOT.

If a current of dry chlorine gas be directed over a mixture of ignited oxide of chromium and charcoal, a chloride of chromium is obtained, which sublimes under the form of beautiful scales of a violet color, answering in composition to the degree of oxidation Cr^2O^3, and, therefore, to be represented by the formula Cr^2Cl^3.

This product, however, is not the only one obtained in this reaction; according to Peligot, previous to the formation of the above combination, another chloride is produced, which seems to have escaped attention hitherto. These are fine, white, silky crystals, commonly mixed with carbon and oxide of chromium. By exposure to air, they attract moisture and oxygen, and are instantly converted into a green liquid. The analysis of this combination leads to the formula CrCl; the new chloride corresponds, therefore, to the degree of oxidation CrO which is unknown hitherto.

In the manner above, the new chloride is not easily obtained in a state of purity. In this we may, however, succeed without difficulty by the action of hydrogen on the violet sesquichloride. The reaction takes place with the disengagement of hydrochloric acid, at a temperature which is very far from softening glass. The violet chloride, retaining its original form is thus converted into a white felty mass, which, under disengagement of heat, is dissolved in water, with a blue color. The solution absorbs oxygen with an uncommon violence, and is rendered of a green color.

Peligot placed weighed quantities of the white powder under a bell-jar filled with oxygen, and having added as much water as was sufficient to dissolve it, he found, that 2 equivalents of the combination absorb 1 equivalent of oxygen. In three experiments the same result was invariably obtained. The constitution of the combination thus formed is, therefore, to be expressed by the formula $2\ (CrCl) + O$.

Among the remarkable properties of the new chloride, the behaviour of its solution relative to the crystallised sesquichloride of chromium is particularly worthy of notice.

According to Peligot, the sesquichloride is *completely insoluble either in cold or boiling water;* an addition of an acid produces no change; concentrated sulphuric acid, and aqua regia do not attack it. This combiation which thus may resist the most active chemical agents, is with great facility dissolved in water if a solution of the new chloride is added to it. The solution of the sesquichloride takes place under violent disengagement of heat; a green liquid is formed, which behaves precisely in the same manner as the hydrochlorate of chromium obtained in the humid way. We may now conceive how it was that some chemists were induced to assume that the sesquichloride of

* Poggendorff's *Annalen*, vol. lxi., p. 397.
† *Journal für Prakt. Chemie*, vol. xxxi., p. 348.
‡ Liebig's *Jahresbericht*, April 12, 1845.
§ *Journ. für Prakt. Chemie*, vol. xxxiii.
‖ Liebig's *Jahresbericht*, April 12, 1845.

chromium is soluble in water. This solubility was manifestly caused by the protochloride of chromium, which is almost invariably admixed.

The disengagement of heat, which takes place by the solution, seems to indicate a combination of the two chlorides into one particular compound. Peligot, however, ascertained, that this is not the case, and, that the solution is brought about rather by physical than by chemical causes. A liquid, containing only $\frac{1}{10000}$th of the white chloride in solution, continued to possess the faculty of dissolving the sesquichloride.

The protochloride of chromium, however, which absorbed the greatest proportion of oxygen it possibly could, does no longer possess the same faculty. If its solution is shaken only for some moments in a vessel containing air, it loses the property of dissolving the sesquichloride. If it is, therefore, intended to make the above experiment, the water employed must previously be freed from air by boiling.

The green liquid obtained by the action of a diluted solution of the protochloride on the sesquichloride, at a gentle evaporation under the recipient of an air-pump, yields grainy crystals, which, notwithstanding their solubility, are easily separated from the mother-ley. Their composition is expressed by the formula $Cr^2Cl^3 + 12$ Aq.* A chloride of chromium with less water is obtained by evaporating, in vacuo, the liquid formed by the action of alcohol and hydrochloric acid on chromate of lead.

Action of Potassa on the Protochloride of Chromium.—Potassa produces in the blue solution of the protochloride a dark-brown precipitate, which, after some time, takes a reddish tint. This precipitate Peligot considers to be the hydrate of the protoxide of chromium, answering to the degree of the chloride, CrCl; but this oxide is still less stable than the answering combination of the chloride. As soon as produced, it decomposes the water at the ordinary temperature, and is converted into a stage of oxidation, ranging between the oxide and protoxide, which, relative to its constitution, answers to the ferroso-fernic oxide.

The formation of this oxide, called by Peligot the deutoxide, as appears from the aforesaid, is attended with the disengagement of hydrogen, which may easily be ascertained by instituting the experiment in a bell-jar confined by mercury. The water is, however, completely decomposed only by raising the heat to the boiling point.

The new oxide, after having been most carefully washed and dried, in vacuo, presents the color of Spanish tobacco. It is but little attacked by acids. On being heated, it loses at first water; at a stronger heat, under the phenomena of ignition, it is converted into the oxide of chromium. In an atmosphere free from oxygen, this conversion is attended with the disengagement of hydrogen, the oxidation being brought about at the expense of the hydrating water.

According to Peligot's analyses, instituted in various manners, the intermediate oxide is composed as expressed by the following formula, $Cr^3O^4 + HO$.

Acetate of Protoxide of Chromium $\overline{A}$, $CrO + HO$.—By mixing a very dilute solution of protochloride of chromium with acetate of soda, we constantly perceive small, red, transparent crystals, which gather at the bottom of the vessel. On exposure to air, these crystals are decomposed, wherefore they must be prepared in an atmosphere of carbonic acid. By the determination of the carbon of this salt instituted with great accuracy, Peligot is induced to believe, that the present equivalent of chromium, 351·8, is too high, and that 328 may be substituted for it. He directs attention to some sources of error, which might have influenced Berzelius's determination; but considers his investigations, by no means conclusive enough fully to warrant the equivalent of chromium.

It exists as a double salt of sulphate of protoxide of chromium and potassa, and which, according to Peligot, is expressed by the formula $SO^3, CrO + SO^3, KO + 6$ HO, which, consequently, corresponds with a great number of double sulphates.

Peligot remarks, that the discovery of the protoxide of chromium presents an analogy between chromium and iron, between which metals so many are already known to exist.

There are, therefore, 5 combinations of chromium with oxygen:—

1. Protoxide of chromium (chromorydul) CrO
2. Deutoxide of chromium .. .. Cr^3O^3
3. Binoxide of chromium .. .. CrO^2
4. Sesquioxide of chrom. (chromoryd.) Cr^2O^3
5. Chromic acid .. .. .. CrO^3

Peligot prepared the metallic chromium neither by treating the oxide with carbon, which would have furnished a product analogous with cast iron, nor by the action of ammonia on the violet chloride, which would have produced nitruret of chromium; but, with the above view, he decomposed the just mentioned chloride by potassium, and the metal thus obtained was soluble in diluted sulphuric acid with the disengagement of hydrogen. The solution possessed the property of a protosalt of chromium. The proto-

* Compare *Moberg. Ann. der Chem. und Pharmacie*, vol. xlviii., p. 228.

salts of chromium, like the protosalts of iron, may absorb a great quantity of oxide of nitrogen, and become, thereby, of a brown color. —nt—

ACTION OF NITRIC AND SULPHURIC ACIDS ON IODINE.*

MILLON† put into a mortar nitric acid, with one or two equivalents of water and iodine, and, after triturating these substances for some time, he obtained a voluminous yellow powder, which he regards as a combination of nitric acid with an oxide of iodine.

On being put in contact with water, this body is directly resolved into nitric acid, iodic acid, and iodine; the same change is produced by heat. If the action of nitric acid be continued for a long time, pure iodic acid is produced. By treating with dilute alcohol, Millon obtained small quantities of a yellow substance, which he found to be constituted as IO^4, and called hypoiodic acid.

By allowing hydrated sulphuric acid in almost a boiling state, to act on iodic acid, the same chemist‡ obtained, on cooling, a deposit being the combination $3SO^3$, $HO + IO^5$, HO in the form of a white pulverulent mass, with a pearly lustre. No gas was disengaged at the same time. With diluted sulphuric acid (SO^3, 3HO) M. Millon obtained the combination 3 (SO^3, 3HO) + IO^5, HO. According to M. Millon, by heating a mixture of 30 grammes of iodic acid with 150 grammes of sulphuric acid until some bubbles of oxygen are disengaged, in the boiling liquid, lamellæ of a sulphur-yellow color, in great quantities, were formed, which are composed according to the formula: $4IO^5 + IO^4 + SO^3$, HO. On longer boiling the said mixture, under continued disengagement of oxygen, crystals are formed of a much more intensely yellow color, to which Millon assigns the following formula: $2\,IO^5 + IO^4 + SO^3$, HO. By water the latter combinations are resolved into sulphuric acid, iodic acid, and iodine.

In the same manner behaved two combinations, which Millon obtained by allowing to continue the action of sulphuric acid on iodic acid, until, besides oxygen, iodine was disengaged. The formulæ which he found in this combination, are—$I^4 + 2O\,SO^3$, HO and $I^5O^{19} + 10\,SO^3$, HO.

M. Millon believes that, by the action of damp air, he has produced the intermediate oxides of iodine, which he supposes in the above formula, viz: IO^4 and I^5O^{19}.

According to Millon, the following oxides of iodine exist:—

Periodic acid	IO^7	
Iodous acid	IO^6, I =	$2IO^3$
Iodic acid	$2IO^5$ =	$IO^7 + IO^3$
Hypoiodic acid ..	$4IO^4$ =	$IO^7 + 3IO^3$
Sub-hypoiodic acid	I^5O^{19} =	$IO^7 + 4IO^3$

—nt—

DOUBLE SALT OF IODIDE OF LEAD WITH CHLORIDE OF AMMONIUM.

VOELKEL found that, if a solution of iodide of potassium and chloride of ammonium be treated with acetate of lead, crystals of a yellowish color and beautiful silky lustre, were deposited on cooling. On the addition of water, the precipitate became red and was resolved into iodide of lead and chloride of ammonium. On throwing iodide of lead into a boiling solution of sal-ammoniac, he obtained the same body; the crystals, however, were smaller, and less beautiful. According to his analysis, it is a combination of two equivalents of iodide of lead with three equivalents of chloride of ammonium.

—nt—

ACTION OF SULPHUROUS ACID ON ZINC AND IRON.*

IF a current of sulphurous acid be directed over chemically pure zinc and covered with water, according to Koene, † no hydrogen is disengaged; if the zinc be washed with water and sulphuric acid poured on it, sulphuretted hydrogen is developed; ‡ and sulphite and hyposulphite of zinc are found in the solution. If the action of the sulphurous acid takes place under exposure to air, oxygen is absorbed in an atmosphere of carbonic acid, 3 equivalents of sulphate of zinc, and 3 equivalents of hyposulphate of zinc are formed.

The following are the particulars of this process:—

In the first period, 3 equivalents of sulphurous acid act on 3 equivalents of zinc in such a manner that 2 equivalents of sulphite of zinc and 1 equivalent of sulphuret of zinc are produced; in the second period, the elements of the sulphuret of zinc act on another

* Liebig's *Jahresbericht*, April 12, 1845.

† *Annales de Chimie et Phys.*, 3 ser., t. xii., p. 330.

‡ *Ibid*, p. 336.

* Liebig's *Jahresbericht*, April 12, 1845.

† Poggendorf's *Annalen*, lxiii., p. 295.

‡ According to Fordos and Gelis (*Journal für Prakt. Chemie.*, xxxi., p. 402), the formation of sulphuret of zinc does not take place in the presence of an excess of sulphurous acid.

portion of the sulphurous acid, when 3 equivalents of sulphurous acid with 2 equivalents of sulphuret of zinc form 2 equivalents of hyposulphite of zinc; and 1 equivalent of sulphur is separated at the same time, which, however, the first moment of the formation, enters into the constitution of the sulphite of zinc, and forms the hyposulphite of zinc; hence 6 equivalents of zinc and 9 equivalents of sulphurous acid, would yield 3 equivalents of sulphite of zinc and 3 equivalents of hyposulphite of zinc.

Koene assumes, that sulphurous acid has a tendency to form a combination, which corresponds to that of sulphuric acid, S, O^2S; he, therefore, considers the hyposulphurous acid, as sulphuric acid from which one equivalent of oxygen is displaced by one equivalent of sulphur, and calls it oxysulphosulphuric acid. He based his view on the facts, that the formation of the sulphites preceded that of the hyposulphites; that the sulphite of zinc forms hyposulphite of zinc, only after having formed sulphuret of zinc; that the protosulphurets in contact with oxygen, give no hyposulphites; that the sulphites are more easily oxidised in the air, and that hyposulphurous acid is a stronger acid than sulphurous; that, moreover, the hyposulphites, on being treated with fuming nitric acid, give sulphur, though it cannot be supposed that a combination SO, on being oxidated at the expense of another body, could part with a portion of its sulphur.

Koene considers Langlois's simple sulphuretted hyposulphurous acid (S^2O^2) as a combination of sulphuric acid with hyposulphurous acid, S, $O^2S + SO^3$, and brings this combination into a parallel with the oxychlorosulphuric acid discovered by Rose, S, $O^2Cl + SO^3$. The double-sulphuretted hyposulphurous acid discovered by Fordos and Gelis, he likewise considers as a geminated acid, and assigns to it the following formula:—S, $OS^2 + SO^3$.

In the action of sulphurous acid on iron, the phenomena are almost the same as with zinc. To separate the sulphites and hyposulphites of iron and zinc from one another, Koene employs alcohol, which dissolves easily the salts of the latter, but has no influence on the former.

The sulphite of zinc is deposited from a hot solution as a crystalline powder, whereas if the liquid be gradually evaporated it forms lamellæ. It is readily soluble in water containing sulphurous acid, an acid salt, however, could not be formed with it. In the air it is not sensibly altered, but Dr. Muspratt* states that it easily passes into sulphate of zinc.

The formula of the salt is $SO^2,ZnO + 2HO$, which coincides with that given by Muspratt, and found by Fordos and Gelis. Sulphite of protoxide of iron is difficultly soluble in water, easily soluble, however, in aqueous sulphurous acid; it eagerly attracts oxygen, and has the formula—2 (SO^2, FeO) + 5HO. Koene also describes a salt of oxide of iron with one-third sulphurous acid, of a yellow color = $SO^2Fe^2O^3 + 7HO$ or $6HO$, and a semi-sulphite of iron = $3SO^2 + 2Fe^2O^3$. Both are amorphous.

The hyposulphite of zinc is a gummy substance, of a yellow color, soluble in all proportions in water. It oxidises in the air, depositing sulphur, whereas the aqueous solution, under the same circumstances, deposits sulphuret of zinc.

Hyposulphite of protoxide of iron is a greenish-blue mass; the solutions in water and alcohol are not changed; in contact with sulphurous acid, they deposit sulphur. The salt has the formula—S, O^2S, FeO + 5HO.

A solution of sulphite of oxide of iron in sulphurous acid, treated with potassa, gives a difficultly soluble double salt = SO^2 KO + $2SO^2$, Fe^2O^3.

—nt—

ON A NEW KIND OF ADULTERATION OF THE MURIATIC ACID OF COMMERCE.*

BY DR. A. VOGEL, JUN.

The contaminations of muriatic acid hitherto known, mostly originating from the sulphuric acid employed in the decomposition of common salt, consist for the greatest part of sulphurous, sulphuric, and nitric acids, chlorine, the chlorides of arsenic, tin, iron, and probably of selenium also, if the acid of Nordhausen be made use of in the decomposition. I lately received an acid of commerce, which, besides traces of chlorine and iron, contained none of the above impurities, being, therefore, considered pure enough, it was employed in some qualitative analyses. On directing, however, the practical chemical manipulations, I had frequent occasion to remark that, from completely pure metallic salts, the acidulated solutions of salts which are not precipitated by sulphuretted hydrogen, yet on being acidulated with the above muriatic acid, a black precipitate was invariably thrown down. This observation naturally raised the suspicion, that the hydrochloric acid contained a body causing the said precipitate; I, therefore, examined the acid more minutely. Several experiments showed, that the hydrochloric acid contained

* Liebig's *Annalen*, vol. l., p. 283.

‡ Buchner's *Repertorium*, xxxvii., p. 3.

chloride of lead, in no inconsiderable quantities. The precipitate thrown down by sulphuretted hydrogen which was brown-red at first, and on employing an excess of the latter body turned black, was sulphuret of lead. On diluting the acid with water, small shining leaflets of chloride of lead, were gradually deposited, owing to the greater solubility of this salt in the concentrated acid than in water. The crystals easily fuse before the blowpipe, and on being calcined with soda, the metallic lead is reduced; and by being moistened with hydrosulphate of ammonia they become black. With sulphuric acid a white precipitate is thrown down in the acid. Potassa and ammonia throw down a precipitate, which on account of the traces of iron admixed is somewhat yellowish.

To determine the amount of chloride of lead contained in the acid, one ounce of the same was slowly evaporated to siccity. The dry residue weighed 4·75 grammes, which corresponds nearly to 1 per cent.

The presence of chloride of lead in hydrochloric acid, may easily be accounted for, since, in some manufactures, as is known, the decomposition of common salt, and the distillation of the acid is effected in leaden vessels.

Very distinct crystals of chloride of lead may be obtained by evaporating the acid to one-third of its volume, when on cooling this salt is deposited in large quantities. Hydrochloric acid, which contains so much lead, is, of course not fit to be employed in chemical experiments. By distillation, however, the acid may be completely freed from that impurity. No trace of lead is met with in the first receiver. —nt—

PROVISIONAL NOTICE ON THE IDENTITY OF LEUKOL WITH CHINOLINE.*

BY LIEBIG.

Dr. Aug. W. Hofmann continued his investigations on pit-coal-tar-oil. By carefully comparing the properties of leukol with those of chinoline, he removed the doubts relative to the identity of the two said bodies, pronounced by himself in his first treatise. Leukol and chinoline are identical. Completely purified leukol gives with chromic acid, the yellow crystalline precipitate, which Gerhardt designates as characteristic of chinoline. This fact, the details of which will be communicated hereafter, was ascertained in our laboratory last summer, and, in my absence, communicated to Prof. Wöhler. —nt—

* Liebig's *Annalen*, March, 1845.

ON ALUMINIUM.*

BY PROFESSOR WÖHLER.

The following may serve as a sort of supplementary article to what I have previously stated relative to the above metal. In the process originally recommended, the reduction was operated by potassium from the chloride of aluminium. The sulphate of alumina, now a very common article of commerce, furnishes an excellent material for preparing the alumina in an easy and simple manner. The salt is mixed with about a quarter of its weight of dry carbonate of soda, the mixture calcined, the mass extracted by water, the alumina left behind completely washed out, mixed with lampblack and starch-paste so as to constitute a soft mass; this is formed in the shape of cylinders of the diameter of the glass tubes to be employed, and then dried and calcined in a covered crucible. While still hot, to prepare the chloride of aluminium, it is put into an appropriate glass or porcelain tube, and at a moderate heat exposed to a stream of dried chlorine gas.

Owing to the violent phenomena of ignition, the reduction of the chloride of aluminium is an operation attended with great difficulties, and can be executed only on a small scale. It answers best to heat the potassium and the chloride separately, in such a manner that the potassium is fused in the vapor of the chloride. Glass tubes in this operation never fail to break. I employ a platinum tube, eighteen inches long and half an inch wide, which may be closed at one of its extremities by a well-ground platinum stopper. Into this the chloride was first thrown, and then near to it is introduced a scaphoid platinum vessel containing the potassium. The tube was then gradually heated over coals until it finally became ignited. There is no doubt that a copper or iron tube may as well serve in this operation, and that an impurity from these metals is not to be feared. The reduction may, moreover, be brought about in a common crucible, into which is placed a smaller crucible containing the potassium, and the space left between the two crucibles charged with chloride, the whole being afterwards well covered and heated over charcoal. The best proportion seems to be about equal volumes of the potassium and the chloride. After the whole is completely cooled, the tube or the crucible is put into a large glass filled with water.

The aluminium is obtained in the form of a gray metallic powder. On closer inspection, however, even with the naked eye alone,

* Liebig's *Annalen*, March, 1845.

a great number of fused metallic globules, white, like tin, are discovered in it, many of them of the size of a pin's head. Viewed with the microscope of about 200 powers, the whole powder is found to consist of similarly-fused metallic globules. Sometimes coherent spongy masses are obtained, which are also composed of the same metallic globules. From this it appears that the aluminium is fusible at a temperature required in the process of its reduction. It appeared from subsequent experiments that this need not be very high, that even the blowpipe flame is sufficient to fuse the aluminium. A fragment of aluminium, on being flattened and placed in a fused bead of borax or microcosmic salt, may be fused into a globule before the blowpipe; however, it is always oxidised in this, and completely disappears after being exposed for some time to the blowpipe flame; in the borax probably with the reduction of the boron, and in microcosmic salts with the reduction of the phosphorus and the formation of phosphoric acid; because in the latter the metallic globule is surrounded by an atmosphere of continually renewed bubbles of gas, and it does not appear that this is owing to an oxidation operated by the flame at the expense of the aqueous vapors. The powder of aluminium was put into anhydrous borax in a crucible, and at the temperature required for melting crude iron it could not be brought to fuse into one mass. It disappeared completely, and the borax was converted into a black-brown slag, having probably derived its color from the reduced boron. The failure in the experiment might, however, partly have been attributed to the circumstance that fused aluminium being specifically lighter than fused borax, hence being buoyant on the surface, burned thereon.

The aluminium in the state of fused bright globules is almost of the same color and lustre as tin. It is highly malleable. Globules of aluminium may be hammered out into the thinnest plates. Its specific gravity determined with two globules, weighing together 32 milligrammes, was = 2·50 at 50°F.; with three globules hammered out, weighing together 34 milligrammes = 2·67. Owing to the very small amount of absolute weight employed, these numbers can, of course, be considered only as approximative.* It is absolutely non-magnetical. On exposure to the air it remains bright. At the common temperature it does not decompose water; but at 212°F., even in this compact state, it gradually disengages hydrogen from water, and in a diluted solution of potassa it is completely dissolved, with a vivid decomposition of the water. In caustic ammonia this takes place less rapidly, and the greatest part of the alumina is left behind undissolved and opaque, retaining the form of the metallic fragments employed.

A bright little lump of aluminium may be heated in oxygen until the commencement of fusion, without being more than superficially oxidised. If it is, however, rapidly and strongly calcined before the blowpipe, it burns with a dazzling white flame, very similar to that of tin. The alumina thus produced sometimes contains bright globules of aluminium, which escape combustion.

The behavior of this metal relative to the dissolved salts of other metals which are easily reduced, is very striking. Bright aluminium reduces neither lead nor silver from solutions of nitrate of lead or silver, even when these are acid. If it is, however, put into a solution of oxide of lead in potassa, the formation of the most beautiful arbor Saturni instantly begins. It likewise reduces tin in shining needles from a solution of oxide of tin in potassa, and silver from an ammoniacal solution of silver; the latter, however, not as a crystalline vegetation, but as a compact superficially-crystalline mass, which after some time may be removed from the lump of aluminium as bright plates. In a solution of sulphate of copper, however, it soon becomes coated with copper, and is gradually covered with a compact mass of copper. On putting it in contact with a solution of lead or silver and zinc, the reduction of these metals commences instantly on the aluminium also; this process, however, is discontinued as soon as the contact with the zinc is broken. —at—

* From two globules, amounting together to 84 milligrammes, after having been fused, the specific gravity of the magnesium appeared to be = 1·87. It was weighed in petroleum and alcohol, because in water the magnesium was surrounded with gas bubbles. In sal-ammoniac, as well as in carbonate of ammonia, it is dissolved with a vivid disengagement of hydrogen. From lead and silver salts it reduces the metals with great facility, with a disengagement of hydrogen. (*a*)

(*a*) Professor Liebig sent to me last year some magnesium fused into globules, to ascertain its density. One determination with 0·371 grammes of substance gave the density 1·69; another with 0·456 grammes substance gave the density 1·71; one with 0·487 grammes substance gave the density 1·69. The temperature was 62°F. It was weighed in water previously boiled; no disengagement of gas was perceived.—Kopp.

ON THE DOUBLE CHLORIDE OF COPPER AND AMMONIA.*

BY DR. HEUMANN.

To prepare the above double salt (kupfer-salmiak, kupferchlorid-ammoniak, einfach chlorkupfer-salmiak, einfach chlorkupfer-ammoniak) we have two methods. [See Leopold Gmelin in his *Handbuch der Theor. Chemie*, last edit.] Graham directs us to mix equal weights of chloride of copper and chloride of ammonium, to dissolve them in water, and to evaporate to the point of crystallisation.

Cap and Henry direct us to treat a solution of oxide of copper in hydrochloric acid with caustic ammonia, until the precipitate at first thrown down is redissolved, presenting a clear liquid of a deep azure-blue color; on gentle evaporation the salt separates in well formed octahedrons.

Graham states, that this salt is completely soluble in water, though with less facility than the chloride of copper; it seems to be a combination of one atom of hydrated chloride of copper with one atom of chloride of ammonium, which must be expressed by the formula—$N^2H^8\,Chl^2 + Cu\,Chl^2\,2\,Aq$.

Cap and Henry say that the salt prepared according to their directions is soluble in water, but only partially (?), and in alcohol also, especially if heat be applied; that the residue left behind undissolved by the water, after being dried, is green, pulverulent, difficultly soluble, very nearly resembling chloride of copper in its constitution. According to analysis, the salt consists of one atom of chloride of copper, one atom of chloride of ammonium, and one atom of water; it is, therefore, distinguished from Graham's salt by containing only one atom of water less. They attribute to it the formula—$N^2H^6 + Cu\,Chl^2 + 2\,HO$.

If, from the various methods of preparing the two salts, doubts may arise relative to the identity of chemical composition, these are augmented, bearing in mind the various behavior with reference to water; the contradictions which exist between Graham's statements and those of Cap and Henry, we intend to consider in the following:—If, according to Graham, 11 parts by weight of the dry simple chloride of copper, and 7 parts of chloride of ammonium are evaporated in water, and the clear, light green liquid evaporated to the point when crystallisation commences, small crystals separate, in the form of rhombohedral tables, which, however, on account of their truncated angles, frequently appear to be of a hexagonal form. After being dried by pressure between blotting paper, the salt becomes of a pure light blue color, and is easily and completely soluble in water and alcohol, though somewhat slower in the latter liquid. The analysis gave the following results:—

I. 0·320 grammes of the salt, dissolved in water, precipitated when still hot by a solution of pure caustic potassa, gave oxide of copper, which, after being carefully washed with hot water, dried and calcined in a platinum crucible, weighed 0·080 grammes, answering to 0·063861 Cu, or 19·95 per cent.

II. 0·440 gave 0·110 CuO = 0·087808 Cu, or 19·93 per cent.

I. 0·315, on being precipitated with nitrate of silver, gave chloride of silver, which, after being well washed, dried, and fused, weighed 0·600, answering to 0·14802 chlorine, or 46·99 per cent.

II. 0·260 gave 0·495 fused chloride of silver = 0·1210 chlorine, or 46·53 per cent.

I. 0·452 grammes of the salt on being dissolved in alcohol, precipitated by chloride of platinum, taking care to use all the precautions required to prevent a loss of platinum, gave the double chloride of platinum and ammonia, which, on being calcined in a covered platinum crucible, behind 0·300 of metallic platinum; these answer to 0·05213 ammonia, or 11·54 per cent. The mean of the numbers found, amount in 100 parts of the salt to—

Chlorine	46·26
Copper	19·94
Ammonia	11·54
Water	22·26
	100·00

19·94 copper combine with 21·65 chlorine, forming 41·59 CuChl. 11·45 ammonium require 22·48 chlorine to form 34·48 chloride of ammonium; hence there is a surplus of 2·13 per cent. of chlorine, which, undoubtedly, may be considered as free hydrochloric acid, which adhered to the crystallised chloride of copper employed.

The constitution of the salt is represented as follows:—

Chloride of copper	41·59
Chloride of ammonium..	34·48
Free hydrochloric acid..	2·17
Water	21·76
	100·00

If, in calculating the atomic composition, the free hydrochloric acid, which acts no part in the constitution of the salt, is left out, we find the following numbers:—

Chloride copper ..	1 atom.	838·35	= 42·76
,, ammonium	1 atom.	669·60	= 34·20
Water	1 atom.	449·92	= 23·04
		1957·87	100·00

* Buchner's *Repertorium*, xxxvii., p. 3.

If an excess of caustic ammonia be added to a concentrated solution of oxide of copper in hydrochloric acid, a clear liquid of an azure-blue color is obtained, from which, by evaporation at a gentle heat, a salt separates in crystalline crusts; well-formed octahedrons, as described by Cap and Henry, could never be remarked; whether the solution of the chloride of copper employed was diluted or concentrated, whether the blue ammoniacal liquid was evaporated to crystallisation at a gentle heat or briskly, nothing but the above-mentioned crystalline crust of the azure-blue color was invariably formed.

If this blue salt, after being dried with blotting-paper, be treated with water, a small quantity of it is dissolved with a blue color, whereas the greatest portion of it remains undissolved as an amorphous green powder; the blue solution, on being diluted with much water, is rendered turbid, with a separation of a green precipitate. By applying heat this change of the blue coloration is still more rapidly brought about.

1·650 grammes of the dry salt, after being washed with water, until this latter liquid is evaporated on a watch-glass, leaves behind no perceptible residue, and which, on being dried, weighed 0·760 grammes. On evaporation, the blue solution threw down a green precipitate, which, after being washed and dried, weighed 0·160; the latter appeared from its behavior to be completely analagous with the green residue. By evaporating the liquid, which is now colorless, chloride of ammonium is obtained, which, after being dried, indicates only traces of copper.

Hence the salt of Cap and Henry was resolved by water into—

	In 100 parts:	
Chloride of ammonium ..	0·780	44·24
Basic copper salt	0·920	55·76
	1·650	

The green salt of copper, as mentioned already, after having been dried, presents a green powder, which, on boiling with water, neither imparts anything to that liquid, nor is changed in color, but, by being heated in a glass tube, loses much water, the green color passing over at the same time into the black. It is easily dissolved in dilute acetic acid, and without leaving behind any residue. This salt, therefore, possesses all the properties met with in the combinations known by the name of oxide of copper; simple chloride of copper (Gmelin, L. C), and oxychloride of copper.

Chemical analysis has still to decide what is the proportion between the chloride of copper and the oxide of copper.

I. 0·320 grammes of the salt dried at 212°F., on being dissolved in water, somewhat acidulated by nitric acid, gave oxide of copper, which, on calcination, weighed 0·230 grammes = 0·1836 Cu, or 57·34 per cent.

II. 0·220 grammes gave 0·160 CuO = 0·127721 Cu, or 58·05.

Mean—58·29.

I. 0·250 grammes, on being dissolved in water acidulated with nitric acid, and precipitated with nitrate of silver, gave chloride of silver, the weight of which in the fused state, amounted to 0·0094 = 0·023189 chlorine, or 9·95 per cent.

II. 0·270 grammes 0·110 chloride of silver = 0·027137 chlorine, or 10·005 per cent.

Mean—9·99.

In 100 parts were—

				Found.	Calcted.
Copper..	58·29	7 atoms	= 2769·99	= 57·40	
Chlorine	9·99	1 ,,	= 442·65	= 9·19	
Oxygen	12·47	6 ,,	= 600·00	= 12·49	
Water ..	19 25	9 ,,	= 1012·32	= 20·92	
	100·00		4824·87	100·00	

From this investigation, it appears that the green salt is a sexabasic oxide of copper = simple chloride of copper, to which corresponds the formula CuCl; 6 CuO, 9 Aq; it forms the blue salt, containing various quantities of chloride of ammonium, proportionate to the amount of free hydrochloric acid present, which is mixed with all solutions of chloride of copper, and this is the salt which Henry and Cap erroneously considered to be of the same composition as Graham's salts.

The formation of the salt reposes on the division of the chlorine between the caustic ammonia and the copper by which of course a basic salt must be formed.

A similar process is observed if the case is reversed: viz., if metallic copper is put into contact with a solution of chloride of ammonium. The investigation of the salts produced in that manner will be communicated hereafter.

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IODIC ACID AND IODATES.*

To prepare the iodic acid, Millon† employed a mixture of iodine and chlorate of potassa, which was heated with water and a little nitric acid until a vivid disengagement of chlorine took place. In a short time the iodine is completely oxidised, whereas the old method (by the action of nitric acid) requires rather a long time. The acid formed

* Liebig's *Jahresbericht*, April 12, 1845.

† *Annales de Chim. et de Phys.*, 3 ser., t. ix., p. 400.

is precipitated with baryta, and separated again by sulphuric acid. When the acid was contaminated with sulphuric acid, Millon obtained larger crystals than if the solution was pure.

By his experiments Millon is led to assume two hydrates of iodic acid. The first IO^5, HO, this is easily soluble in water and alcohol; by heating to 166°F., or treating with absolute alcohol, it passes over into IO^5, $\frac{1}{2}$ HO, which is insoluble in alcohol, and on being brought in contact with water passes over into the first hydrate. At 338°F. it forms the anhydrous iodic acid, which in contact with water or alcohol passes over into its hydrate IO^5, HO. Rammelsburg, however, found that the iodic acid formed in small crystals was anhydrous, attributing the trifling loss which they undergo by exposure to heat to an amount of sulphuric acid adhering, whereas the acid obtained on slow evaporation, and freed from sulphuric acid, still retains 5·35 per cent. of water,* hence 1 equivalent, and is consequently identical with the hydrate of Millon.

In addition to these, Millon has examined a series of iodates, several of which have been analysed already by Rammelsberg,† and in some cases with a different result. Millon obtained a neutral anhydrous iodate of potassa (IO^5, KO); moreover, a bi-iodate with one equivalent of water (IO^5, KO + IO^5, HO); finally, a tri-iodate, which is formed in the presence of a great excess of acid (IO^5, KO + 2 IO^5, HO). Millon could not obtain a basic potassa salt, but he had an opportunity to study two salts discovered by Serullas, which he found to be composed exactly in the manner stated by this chemist, viz., SO^3, KO + IO^5, HO and ClK + IO^5, KO + IO^5, HO.

Iodate of lime. Millon obtained a salt with 6 equivalents of water, which falls into powder in a very dry air only, and loses at 212°F. 5 equivalents of water; and at 374°F. the whole amount of its water is driven off. Rammelsberg found the salt to be very constant in air, and only to contain 5 equivalents of water. The anhydrous salt has the formula, IO^5, CaO.

Iodate of baryta (IO^5, BaO) contains, according to Millon and Rammelsberg, 1 equivalent of water, which at 266°F. escapes.

Iodate of strontian behaves like the baryta salt.

Millon found that iodate of soda crystallises under various circumstances with 16, 12, 10, 6, 4, and 2 equivalents of water; whereas Rammelsberg could obtain but two markedly distinguished salts with 10 and with 2 equivalents of water. The acid iodates of soda are highly soluble in water. Millon obtained only white, almost gummy, masses, which furnished no accurate results.

Millon, as well as Rammelsberg, obtained iodate of magnesia, crystallising with 4 equivalents of water (IO^5, MgO + 2 HO); at 302°F. it passes over into IO^5, MgO + $\frac{1}{2}$ HO, retaining most pertinaciously this little quantity until the temperature reaches 420°F.

Iodate of silver has the formula IO^5, AgO.

Of iodate of copper Millon obtained three modifications: — IO^5, CuO, HO + IO^5, 3 CuO + 2 IO^5, HO and IO^5, 6 CuO + 2 IO^5, HO.

Millon could not obtain iodate of ammonia of a constant composition, because this salt invariably loses ammonia. The acid iodates of ammonia, at 338°F., previous to losing water, explode with violence.

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* Poggendorff's *Annalen*, lxxii., p. 417.
† *Ibid*, xxxiv., p. 545.

LITHIUM.*

The reaction of lithium† before the blowpipe, as is known, is very equivocal in the presence of soda, and becomes only distinct if in 200 parts of chloride of lithium only one part of chloride of sodium is contained. Stein attributes this circumstance to an excess of temperature, and the same chemist has found, that if the essay is fused to the platinum wire, in such a way, however, that the same remains porous, and is afterwards impregnated with tallow and heated by the flame of a candle, the red flame of the lithium is then still clearly to be distinguished if 1 part of lithium is mixed with 2,580 parts of sodium.

—nt—

* Liebig's *Jahresbericht*, April 12, 1845.
† *Journal für Prakt. Chem.*, xxxi., p. 361.

DOUBLE SULPHATES OF THE MAGNESIAN SERIES WITH SODA‡

At a temperature exceeding 92°F., at which, as it is known, the anhydrous sulphate of soda is precipitated, Arrott,§ succeeded in obtaining a double salt from a mixture of the latter salt with sulphate of magnesia, whereas at a lower temperature he could not obtain a similar compound. The method most successful in producing this and other double salts of the magnesian series, was,

‡ Liebig's *Jahresbericht*, April 12, 1845.
§ *Phil. Mag.*, 3 ser., xxiv., p. 502.

to mix equivalent quantities of the respective salts, and to evaporate the whole at 135°F. Arrott obtained

Dble. sulph. magnesia and soda....		4HO.
,,	,, of zinc and soda.....	4HO.
,,	,, of protoxide of iron and soda	4HO.
,,	,, of oxide of copper and soda	2HO.
,,	,, of protoxide of manganese and soda	2HO.

—nt—

ZIRCONEARTH.*

On examining the properties of zirconearth,† Hermann ascertained some of its qualities which have been but incompletely known hitherto. With reference to the atomic weight of the earth, the numbers resulting from his experiments are from 1131·30 to 1151·40, supposing the formula to be $Zr^2 O^3$, the mean 1139·45 is, therefore, so near to that ascertained by Berzelius 1140·40, that Hermann based his calculations upon the latter. Hermann prepares the zirconearth by calcining either its hydrate, carbonate, or nitrate. He obtained transparent, hard lumps, being rough to the touch, with the lustre of mother of pearls, color of pearls. On precipitating the zirconearth from substances which contained foreign matters, Hermann found that a portion of the latter was carried down with the precipitate. By applying heat to a similar zirconearth it blackened, and exhibiting peculiar phenomena of ignition, it was afterwards burned white. Of the metallic oxides likewise small quantities pass over in the precipitated earth and adhere to it pertinaciously. Of potassa this property was known already. Hermann found 3·11 per cent. in a similar earth, which by its exterior appearance alone may be distinguished from the pure earth. By precipitating a neutral solution of zirconearth with carbonate of potassa, soda, or ammonia, Hermann observed, that the portions which were first added caused the formation of a soluble basic salt, and that by a further addition, under the disengagement of carbonic acid, a sixbasic precipitate is formed, which precipitate is then dissolved in an excess of an alkaline carbonate, forming a double carbonate, whereas the hydrate of the zirconearth was not dissolved in an alkaline carbonate, even in its recently precipitated state. Alkaline sulphites offered completely the same behavior as the carbonates. Hermann found to be inherent in this earth a great tendency to forming basic salts; he obtained with sulphuric acid in addition to the known three salts a sixbasic one, with nitric acid, a tribasic, and a bibasic salt, with chlorine in addition to chloride of zirconium, the simple and the half hydrochlorate, likewise one-third hydrochlorate, and with fluorine half and two-thirds of fluoride of zirconium.

—nt—

* Liebig's *Jahresbericht*, April 12, 1845.

† *Journal für Prakt. Chemie.*, xxi., p. 75.

ON THE CONVERSION OF SULPHATE TO SULPHURET OF LEAD BY ORGANIC MATTER.*

It is well known that the sulphate of lime is reduced and converted to a sulphuret by the organic matters with which it is left long in contact. A plaster cast soaked in oil acquires commonly after a few days a most offensive smell. M. Kersten has performed some experiments with the sulphate of lead, and he has found that this salt, moistened with water, and left for eight months in contact with sugar, decomposing wood, or green leaves, is completely transformed to sulphuret; and this transformation was observed to take place more rapidly with the leaves than with the wood.

ANALYSIS OF THE WATER OF ARTESIAN WELLS.†

We have been favored by a correspondent in Paris with the analysis of the water of the celebrated Artesian well of Grenelle. It is remarkably soft, and contains, according to the undermentioned analysis, only 8·05 grains of saline matter to the gallon, the greater part of which is formed of carbonate of lime and bicarbonate of potash. This is in the proportion of about 1-7000th. The quantity of saline matter in the filtered Thames water is about 12 grains to the gallon, or nearly 1-5000th; the principal saline contents being carbonate of lime and sulphate of lime.

The Artesian water of Grenelle, like the Artesian water of Trafalgar-square, possesses an alkaline reaction; this being due, in the former case to bicarbonate of potash, and in the latter to carbonate of soda. This last-mentioned alkaline salt is said to exist abundantly in all the deep Artesian water of London. It is difficult to say whence it is derived, unless we suppose that it is

* *Annuaire de Chimie*, 1845, and *Medical Gazette*.

† *Medical Gazette*, June 13, 1845.

produced by the reaction of common salt contained in the sea air on the chalk strata which form the outskirts of the London basin. Efflorescences of carbonate of soda, from this cause, have been found on the chalk downs for many miles inland, and the superficial washings of these strata may become thus impregnated with the carbonate of soda; and the water may afterwards infiltrate into the London basin between the beds of sand and chalk.

One interesting fact connected with the Artesian water of Grenelle is, that it is the deepest source in the world from which water is obtained. The shaft or tube, which, for a large part of its course, traverses chalk, extends into the earth to the depth of 1,794 feet (English.) The temperature of the water is 82° F. or 31° above the mean temperature of the climate of Paris.

The analysis of 100,000 parts of the water, after being freed by filtration from mechanical impurities, gave of

Carbonate of lime.....	6·8
Carbonate of magnesia.....	1·42
Bicarbonate of potash	2 96
Sulphate of potash	1·2
Chloride of potassium	1·09
Silica.....	0·57
Organic azotised matter	0·26
	14·30

The water rises to the surface, and is discharged at the rate of 600 gallons per minute. This is about the quantity obtained from the Artesian well in Trafalgar-square.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ON GAS AS A MEANS OF LIGHTING PUBLIC SCHOOL-ROOMS.*

BY DR. SIEBENHAAR.

If light is produced by either gas or candles, and oil lamps, the process of combustion which takes place is the same, with the difference, however, that the bicarburet of hydrogen is ready made in the gas, whereas in burning candles or oil, the hydrogen and carbon are combined by the intervention of heat, this difference becomes striking enough by the intensity and color of the light. Gaslight is the more intensely bright, and of a yellowish-white color; hence the more approaching to sunlight, the less the number of foreign gases are mixed with the bicarburet of hydrogen, and the higher the state of concentration of the bicarburet the moment it is urged through the aperture of the burner.

Only if the gaslight is too bright, and the flame flickering it may become dazzling, and cause some injury to the eye. If a light is too intense it is almost as injurious as if too feeble. The celebrated oculist, Dr. Schmalz, found a sure criterion of over excitation of the eye, in the wonderful phenomenon by which the image of an object viewed at a gentle light disappears soon after our eyes are directed towards another region, whereas subjects illuminated by an intense light, even after being removed, leave behind for a long time, an impression in the eye which for a short time interferes with the inspection of other bodies, and is what we call dazzling. This phenomenon is most distinctly called forth if, after having awhile looked in the sun or a flame, we close our eyes. The image remains for a short time, passes through the more intense colors of the rays of light, becomes surrounded by the polarised blue, violet, green, and in a shorter or longer time disappears gradually, in proportion to the less or more strong impression. On considering the length of time after which the impression ceases to exist in the closed eye, as well as the attending polarisation of color, we obtain a standard by which to measure the intensity of the luminous object, which differs not only in general, but also with reference to each individual. If the above experiment is made with both the gaslight and the flame of a common argand lamp, one and a half inches broad, the difference becomes scarcely sensible. If any of the two flames are looked on for one minute the impression remains for one minute more, and one minute and a half if the eye be rather sensible.

It, therefore, stands nothing in the way to the introduction of gaslight in school-rooms. To prevent, however, any injurious effects on the eye, the following two precautions may be had recourse to:—

1. Not to allow the gas to burn too free, but properly to screen it.

2. In certain cases to protect the eyes from the direct rays of the light.

—nt—

* Siebenhaar's *Magazin f. d. Staatserzneikunde*, vol. iii., p. 1, and Neumaister's *Repertorium*, xxxii., 1845.

ON AURIFEROUS GLASS.*

It is known that glass, in the composition of which gold enters, on being exposed in a crucible to a very high temperature, is rendered colorless and diaphanous; and if but gently heated again, becomes ruby-red and opaque.

According to Splittgerber the following proportions are well calculated for the above experiment—

3 pounds	4½ ounces	of fine white quartz
1 ,,	14 ,,	nitre
	13 ,,	pure white soda
	4 ,,	carbonate of lime
	2½ ,,	white arsenic
	4½ ,,	minium
	4 ,,	pure antimony.

The solution of a Dutch ducat (3·41 grammes) is dissolved in aqua regia, thrown into the sand, which is then mixed with the other substances, put into the crucible, and above three hours exposed to a white heat.

This method of preparing, differs from that formerly made use of, as neither gold purple nor oxide of tin is necessary, which seemed to be hitherto indispensable. The glass obtained in that manner is completely colorless, and at 932°F. of an intensely ruby-red.

This coloration takes place alike if the glass is heated in the air, in an atmosphere of oxygen or hydrogen, or in a close crucible, in sand, in charcoal powder, in protoxide of tin, in fused and ignited nitre, in chlorate of potassa. By fusing only small pieces with the aid of oxygen the glass may be rendered colorless again; on applying heat again, it resumes its color.

Splittgerber could notice no loss of weight. The moment the glass becomes red, he, however, remarked that its specific gravity diminished. On analysis the amount of gold was found to be 0·059 per cent., which was less than half of the gold employed; in addition to this were found:—46·17 silica, 2·01 lime, 23·41 potassa and soda, 5·06 lead, 2·40 antimony, but no arsenic.

Splittgerber believes that the gold was contained in the glass as a silicate, and in particular to the colorless state as an oxide, and that on being heated it passes over into the ruby-red oxide, indicated by Berzelius, by yielding one portion of its oxygen to another ingredient, which in this case may perhaps be the antimonious acid, and by this change in the chemical combination, a slight expansion of the glassy mass might have been produced.

In favor of this view of a commencing reduction may also be adduced, the circumstance that the glass on being exposed to a flame, supported by a stream of oxygen, is rendered colorless again; whereas, on continued heating in a crucible, the gold may be still further reduced, and even separated in the metallic state; hence the hepatic color of the glass, if the rays of light are reflected, and the blue color, if the same traverse the glass.

It remains, however, to be accounted for, how it happens that the gold, endowed with so great a tendency to be reduced from its combinations into the metallic state, is not reduced completely in the atmosphere of the smelting furnace. Already the fusion of the glass taking place at a high white heat, or on cooling when it passes through all degrees of temperature, and that this transfer of oxygen of the gold is obtained only by a fresh application of heat, which is comparatively inconsiderable. By employing the gold purple of Cassius, a colored glass was obtained by Splittberger, which, however, was rather verging upon the violet. —nt—

* *Annalen der Chemie und Pharmacie*, vol. lii., p. 2.

ON THE IRON ORE OR IRON SAND OF SAMAKOFF IN TURKEY; AND ON THE BEST MEANS OF REDUCING IT INTO THE STATES OF CAST AND WROUGHT IRON, &c.*

BY WILLIAM NEALE CLAY,
DIRECTOR OF IRON MINES AND WORKS TO THE SUBLIME PORTE.

Under the direction of Mr. Fairbairn, of Manchester, to whom the experiments on the Samakoff iron ores were entrusted, the writer was empowered to make such trials and experiments as, in his judgment, were most conducive to the full and perfect reduction of the ores, into the cast and malleable states. Acting upon these instructions, no time was lost in making the necessary preparations; and from the facilities afforded, both at Manchester and the Backbarrow Iron Works, near Ulverstone, the most satisfactory results were obtained.

It would appear that, on a small scale, and at a great sacrifice of time, fuel, and labor, this ore had been long used by the natives, for the production of small quantities of wrought iron, by the ordinary primitive method—namely, the mixture of the ore with charcoal in a deep hearth, and the employment of a weak blast.

The casting of ordnance being a great ob-

* Read before the Institution of Civil Engineers; from *Newton's Journal*.

ject with the Ottoman Government, more recent attempts had been made to convert the ore into cast iron, but without success; or, at all events, with so little, as only to produce a white iron, quite incapable of being remelted for common purposes. Very meagre information was obtained, as to how or where the attempts had been made; it being merely stated that every means had been tried, and that the iron produced could not be recast, even into a cannon ball. That these trials had been made in various countries, but always with like results, and the continuous failure in the production of iron fit for remelting, seemed to have produced a conviction that there was something inherent in the ore, which prevented its being reduced, by any means, into cast iron, of good, or even of common quality.

From the information obtained, it appears that the supply of this beautiful ore in the lower valleys of Roumelia, at a short distance from the sea, is almost inexhaustible. Although much smaller in its crystals than the celebrated Woortz ore from Porto Nuovo, in the East Indies, it is so similar, that probably, like it, it may be found in the mine, mechanically combined with quartoze crystals; if so, nature has nearly purified it by the action of the torrents, which have brought it from its bed; for when it is deposited in the valleys, the small portions of silex still commingled with the ore are readily separable by many mechanical means.

In appearance it very much resembles the titaniferous iron ores, and Mr. Watson, on being shown a specimen, expressed his conviction that he should find considerable quantities of that metal, as he had done in other samples of a similar appearance; but this proved not to be the case—no trace whatever could be detected of titanium. When freed from its accompanying siliceous earths, which amount to 12 per cent., it yields 72·4 per cent. of iron, a quantity which denotes its extreme purity, being the richest form in which the ore is ever found in quantity. When combined with the silex, the yield, by analysis, is 63·7 per cent., which is very nearly what was obtained by fusion with carbonaceous matter, and a flux, in the crucible.

Among the parties consulted, a difference of opinion has existed as to the necessity of having the silex separated. Some contending that, when vitrified by a due proportion of lime, the resulting glass was rather advantageous; while others maintained that such a glass was not obtainable from silex and lime alone, and that the presence of the former, unaccompanied by clay, rendered many ores of iron comparatively valueless, there being much difficulty in reducing the silicated oxides of iron which such ores would form when exposed, *per se*, to a high temperature.

It appeared to the author, that the investigation of the phenomena, attendant on the fusion of the earths generally found commingled with iron ores, was of the greatest importance, and that much of the successful practice of iron smelting, depended on the proper vitrification of these earths; that an important analogy existed between the manufactures of glass and of pig iron, and that in order to accomplish successfully the production of the latter, it would be necessary to study the theory of the former. When this was done, he conceived that, more particularly with reference to the richer descriptions of ores, the mists which were generally supposed to envelope the manufacture of pig iron, must be rapidly dispelled before the advancing light of science, and that many results, now looked upon as unaccountable phenomena, would be reduced to their more rational position, as calculable effects from appreciable causes.

The author believes he may claim priority of discovery, as being the first to point out the true atomic character of glass, or, rather, that the quality of that production depended on its being formed of ingredients, which were compounded in certain definite atomic productions. This result he arrived at (as nearly as he can recollect) in the year 1835, when connected with the establishment of Messrs. Chance and Hartley, near Birmingham. The making of crown or window glass at that period, like the making of pig iron at present, was looked on as a sort of manufacturing lottery, and the "founder," or glass smelter, who managed the furnace, was an important person, who responded to complaints of any badness of quality in the glass, by laying upon the furnace all the blame of the defects. The consequence was, that the manufacturer was constantly oscillating between excesses of soda and lime, respectively, as "ambittiness"* or "cord"† preponderated; the former being a sympton of too much lime, the latter of too much soda. The author obtained the mixtures which had made the very best glass, and after much laborious investigation and study of the several decompositions, that would take place, came to the conclusion, that this description of glass was a quinque-silicate of

* "Ambittiness" denotes the presence of small opaque particles.

† "Cord" is a striated line, distorting objects when the pane of glass is looked through.

lime and soda. On this hypothesis, he obtained from Mr. Richard Philips analyses of the several materials used, and by compounding them in the above proportions, found, that five atoms of silica, or say 80 pounds, with one atom of pure carbonate of soda, say 54 pounds, and five atoms more of silica to one of carbonate of lime, say 50 pounds, or such quantities of the commercial articles as contained these proportions, universally and without an exception, produced a beautiful transparent glass, free from all the evils of the old system.

The pots, which previously were stopped working at two-thirds of their depth, were then cleared out to the bottom; the immaculate "fonder" was dethroned, and his excuse "of the furnace's whims" was no longer allowed. From glass thus made, in conjunction with the late esteemed Dr. Ritchie, the author produced lenses for optical purposes, of great size and purity; he believes that the death of that gentleman alone prevented the mose perfect accomplishment of this great desideratum in science; and he takes this opportunity of expressing his humble conviction, that optical glass will never be successfully produced, except upon such principles.

In conjunction with Mr. Denham Smith, he soon after took out a patent, for the employment of barytes as a solvent of silica, and the first trial, on a practical scale, proved the soundness of the theory; for one atom of carbonate of barytes, say 98 pounds, with five of silica, or 80 pounds, made a true and beautiful glass, in combination with the previously named proportions of soda, lime, and silica.

But there exists this peculiarity about the silicates of lime and barytes, that, *per se*, and even when combined together, they make a very infusible glass; that made with lime, more particularly, is viscid and opaque, except at an extremely high temperature. If glass bottles were produced from lime and sand alone, a few, only, could be made from the top of the pot, while the "metal" was hot; its cooling would cause the succeeding articles to be more and more filled with "stones" or "knots," till the whole mass would become opaque and unworkable. This, in his opinion, demonstrates the necessity of having another neutraliser of the silicic acid, in the smelting of iron ores, which, when of a purely siliceous character, are found very refractory, if lime alone be used as a flux.

The glass bottle makers, however, soon found that clay (brick clay was the best for black bottles) formed the best, because the cheapest ingredient, to add to the lime, as a co-neutraliser of the silica. The author regrets that he had no opportunity of accurately investigating what compound of silicate of alumina is most fusible, but from the fire clays containing a great proportion of the latter, he thinks it is very probable, that here also, a quinque-silicate may prove the most fusible compound; that is, in opposition to the fire clays, a small proportion of alumina to a large one of silica. Before this can be accurately determined, the higher order of chemists have to decide on the atomic number of alumina, which at present seems unsettled; when that is done, he has no doubt that a quinque-silicate of lime and alumina will be found the best glass or scoria for the manufacturer of iron to produce, flowing more readily, and releasing the oxide of iron, which some unappropriated silex might have formed a connection with, to a more profitable combination with the carbon of the fuel.

The accurate experiments of Mr. Mushet, and the practical views he deduced therefrom, satisfied him that lime alone was insufficient, as a flux for siliceous iron-stones, and the practice of the Backbarrow Company, now nearly the only smelters of iron with wood charcoal in the kingdom, is, to introduce an iron-stone, from the Workington Coal Mines, the alumina of which supplies the desired proportion; hence their "cinder" bears the character of a true glass—homogeneous and transparent, except when slightly tinged by charcoal.

The author has gone thus far into the exclusive subject of the vitrification of silex, because that article seems, by analysis, to be the only adulterative matter commingled with the Turkish ore. He considers, therefore, that it is necessary to be separated as far as possible, and if 2 or 3 per cent. (as is probable) still remained, that with lime (if it contains no alumina itself) a small portion of clay should be added. If "natron" be obtainable at a very cheap rate, its soda would, with the lime, occasion a more rapid and fluid fusion of the silex; in this case, if clay be not readily obtainable, none would be requisite.

NEW METHOD OF BLASTING ROCKS, BY MEANS OF GUNPOWDER.*

BY M. COURBEBAISSE.

THIS method consists in forming in rocks to be blasted, cavities or mines, of any required size, according to the effect desired to be produced, instead of the small cylindrical holes ordinarily employed, which do not admit of much variation in size.

* Acad. des Sciences.

An experienced miner will be able to judge, upon careful examination, according to the size and nature of the mass of rock to be removed, the most convenient situation and size of the cavities required to effect the object. This having been ascertained, a cylindrical hole, either vertical or inclined, is made in the ordinary manner, at each place where it is thought requisite, and this is afterwards enlarged, either by any chemical agents, which will have the effect of destroying the rock, or by mechanical means.

Whatever may be the means employed, a large quantity of powder can be lodged in this way cheaper than in small cylindrical holes, and less expense is incurred for ramming and firing than for the same quantity of powder lodged in small holes. Besides, the powder, being in large quantities, and under greater pressure, burns better, gives out more heat, and produces a more powerful effect than if divided into small quantities.

The experiments made by the inventor have realised his expectations; they were made upon a calcareous rock, in which he formed the cavity to receive the powder by means of a chemical agent (hydrochloric acid); the rock being detached in larger masses by this mode of applying the powder than by the old method, the blocks are not thrown off with force, but only moved a short distance. As the gases evolved by the combustion of the powder do not escape into the atmosphere until they have expanded to their fullest extent, they do not detonate.

When the cavity has been produced in the calcareous rock, by the effect of the hydrochloric acid (which is poured therein three several times through a copper funnel about three yards long, and water afterwards added thereto; and is allowed to act upon the rock for two hours) it is emptied by means of small vessels of the same diameter as the hole, and from four to five inches in height, which act in a similar manner to pump-buckets, being furnished with valves opening upwards. It is afterwards dried, by stuffing it with tow, which is turned about and drawn out.

In order to charge, the powder is poured in until the cavity is two-thirds full; upon this, one of Bickford's fusees is placed; the other third part of powder is then poured in, and the hole is then filled up with sand,—after which the fusee is ready to be lighted. The explosion takes place without either flash or detonation, or scattering the materials; a dead sound only is heard, arising from the cracking of the rock, and only a slight trembling of the mass is perceived, which is suddenly raised a very little distance, and falls again, cracked in every direction.

The operation by these improved means only costs about five pence per square yard, whilst by the ordinary method it costs from 2s. 6d. to 3s. 6d.

FRAUDULENT MANURE DEALERS.*

BY ALFRED GYDE, M.R.C.S.E., PAINSWICK.

Some months since, a sample of what was called guano was forwarded to me through the post, with an initial address to the post-office, Swansea, and with an intimation that if I would recommend it, three or four tons would be at my disposal. Upon examination, as I had mentioned privately at the time, I found that it did not contain more than five per cent. of real guano, the remainder being composed of a powdered sandstone. It is true that since that time no adulteration of so flagrant a kind has fallen under my notice, although some of the samples recently submitted to me for examination have been, to use a trade phrase, "doctored," *i. e.*, mixed, not only with guano of an inferior quality, but with other substances of no very fertilising properties. Of this the public should be informed, as well as that guano is not the only adulterated manure now attempted to be palmed on them. I have now in my possession a sample of what is sold as prepared night-soil, but which is totally unlike the article which name it bears. It came under my notice in the following manner:—being offered in the way of business to a gentleman as chemically prepared night-soil, and purporting to contain all the fertilising properties of that most valuable manure in the most convenient form for use, he applied for a sample, which he forwarded to me for analysis. The article, when received, was in a moist state, had the peculiar smell of night-soil, but seemed to be mixed with a quantity of earthy matter, limestone, and ashes. The following is the result of my examination:—

Water separable at 212.....	27·0
Organic matter, yielding traces of ammonia	6·5
Soluble saline matter, principally common salt, with a little sulphates	7·0
Residuary matter, consisting of earth, with corbonate of lime and phosphates.....	59·5
	100·0

Upon this report a quantity of this manure was ordered for trial, but when received it had a very different appearance from the sample sent, so much so, indeed, that it was objected to. This was explained

* *Agricultural Gazette*, June 14, 1845.

by the manufacturer, who stated that the difference in appearance arose from the night-soil having its ammonia fixed by oil of vitriol and gypsum, and dried on hot iron plates in order that it might not contain too much water—that the volatile portion was completely fixed by the gypsum and vitriol, but that it contained the sulphate of ammonia in its stead; the manufacturer also inclosed the analysis, which, I presume, was intended for the wet sample. This, however, did not quite satisfy the purchaser, who forwarded me a portion of the bulk for further examination. This sample was dry to the touch, devoid of smell, and when placed under a low power of a microscope, was found to consist of fragments, carbonate of lime, burnt earth, and siliceous sand, and mixed with a powder of a gray colour, the whole sample bearing the appearance of garden soil, such as would be found resting on the chalk formation or Bath stone of the Oolite. In order that the public may appreciate the genuineness of the article, I append a copy of the analysis of the wet sample as supplied by the manufacturer, an analysis of human excrements by Berzelius, and the analysis which I have made of a sample taken from the bulk of the dried article supplied. Analysis said by the manufacturer to be the analysis of the prepared night-soil:—

Carbonate of lime	16·50
Sulphate of lime	12·0
Sulphates of soda, ammonia, potash, and magnesia	14·50
Phosphate of lime and magnesia	4·25
Chloride of sodium	3·0
Organic animal matter yielding ammonia	10·50
Moisture	26·25
Earthy matter	13·0
	100·0

Analysis of excrements of a healthy man by Berzelius:—

Water	73·3
Solid matters	26·7
	100·0

100 parts of dry excrements contain—

Albumen, bile, and other animal matters, with portions of undigested food	86·8
Carbonate of Soda	0·8
Sulphate of soda, with a little sulphate of potash and phosphate of soda	0·8
Phosphate of lime and magnesia, and a trace of gypsum	10·0
Silica	1·6
	100·0

Analysis of the article sent as dried night-soil:—

Carbonate of lime	19·0
Sulphate of lime (gypsum)	2·8
Sulphates of soda, potash, and magnesia	traces.
Do. of ammonia	do.
Phosphate of lime and magnesia.	5·2
Chloride of sodium (common salt)	1·0
Organic matter, principally vegetable, yielding only a minute trace of ammonia	9·5
Moisture	6·0
Earthy matter, consisting of sand, burnt earth, oxide of iron, &c.	56·5
	100·0

In order to bring the comparison more clearly under the comprehension of the reader, I have reduced the three previous tables into one, as nearly as the different descriptions of ingredients will permit:—

	ANALYSIS BY		
	Berzelius, of dried human excrements.	The Manufacturer of the prepared night-soil.	Myself, of the article supplied.
Moisture	0·	26·25	6·0
Carbonate of lime	0·	16·50	19·0
Carbonate of soda	0·8	0·	0·
Sulphate of lime	trace	12·00	2·8
Sulph. of soda, potash, ammonia, and magnesia	10.8	14·50	traces
Phosphate of lime and magnesia	10.8	4·25	5·2
Chloride of sodium (common salt)	0·	3·00	1·0
Silex	1·6	—	—
Earthy matter	..	13·0	56·5
Albumen, bile, and animal matters	86·8	—	—
Organ. animal matter	..	10·50	—
Organic matters chiefly vegetable	..	..	9·5
	100·0	100·00	100·0

The manufacturer's analysis shows this cheap and efficient manure to have been prepared by mixing with a small quantity of night-soil a small portion of common salt and gypsum, which were dried with ashes and earth, so as to bring the compost into a marketable state. But the manure does not seem to be the only article made up. The analysis is a very ingenious compound for the purpose of disposing of this efficient manure, and certainly a very useful manure

it would have been, had it contained the weight of ingredients stated in the analysis, although in composition very unlike the analysis of the genuine article as given by Berzelius. But we find that the bulk when delivered bore a very different appearance from the sample, and this fact caused some doubts as to its genuineness—doubts which were answered by the manufacturer in a manner calculated to satisfy the suspicion of most persons, though in the present case they were not removed, and a portion of the bulk of the dried night-soil was forwarded to me for analysis; the result of which at once proves that its composition, as sent out, is totally unlike the analysis offered as its composition, and also unlike the composition of genuine night-soil; for had the moist sample contained the ingredients stated in the analysis offered by the manufacturer, the fact of drying it on iron plates, by which 20 per cent. of water was evaporated, would have increased the solid constituents in the manure in the same ratio. This we do not find to be the case; on the contrary, the manure as sent out is far inferior to the moist sample. 56·5 of earthy matter with 19 of carbonate of lime is not a very valuable manure; 75·5 per cent. of the ingredients are to be found in many very poor soils; and yet it is offered to the public at the moderate price of 5*l.* 5*s.* per ton, with the additional liberal offer of delivering it at any wharf or railway station in London.

NODULES.

In the geological section of the British Association a communication was made by Professor Henslow that promises to be of great importance to the English agriculturist. In the red crag, London clay, and green sand, there abound nodules which have been found on analysis to yield 56 per cent. of phosphate of lime and phosphate of iron, and these nodules are in great abundance. Phosphate of lime is the most valuable manure, and as the supply of bones, from which it has hitherto been chiefly obtained, is diminishing, the importance of the discovery will be at once seen. So important, indeed, was it considered to have a new source for the supply of phosphates, that Dr. Daubeney visited Spain to examine a reported natural phosphate formation in Estramadura, but he found that it was confined to a small space, and that it was difficult of access. It now seems, however, that the agriculturists of the eastern counties possess an abundant quantity of this manure under their lands. A discussion arose as to the nature of the nodules, it being the opinion of Professor Henslow and Mr. Solly that they are formed of the exuviæ of animals, as many fragments of animals are found among them; but Dr. Buckland contended that they are aggregations, and that the phosphates they contain are derived from the iron which enters into their composition.

ELECTRO-CULTURE.*

The extraordinary effect of electro-culture, as stated by Dr. Forster on his barley crop, has induced me to make an experiment precisely according to the plan and diagram already given; but as I only put the wires down so lately as the 23rd inst., after the barley was above ground, it is too soon yet to expect any difference, and none is yet visible; but as I purpose making two or three more experiments on other crops, and I observe in your last number that the experiment of Dr. Forster's plan is described as not being a very scientific one, and that experiments are now in progress in Norfolk and elsewhere on more scientific principles, I shall feel greatly obliged to you if you can furnish me with the particulars of such experiments, or of any one that you consider more likely to bring out the best results for my guide, before putting down any more diagrams on this interesting subject.—*X. Y. Z., A Subscriber.*

The effect, or rather the *tendency* of any metallic connection between the soil and the air, some 15 or 20 feet above, clearly must be to induce a similar electric condition between the two—to hinder any irregularity in the distribution of electric influence—simply because metals are good conductors of electricity. Whether such a result is desirable as regards the crops growing on the soil remains to be ascertained; but certainly, in order to obtain it that arrangement of wires must be best, in which there is the most perfect connection by conductors between the soil and the air. Perhaps other purposes may be answered by Dr. Forster's arrangement of wires; but so far as they tend to connect by electric conductors the soil and the air, we do not by any means think it the best that could be suggested. We have had since February a copper wire 120 yards long, studded with upwards of 1,200 metallic points, suspended in a direction magnetically north and south, in a somewhat elevated position, at a distance from trees, and at an elevation of 20 feet from the ground; and this wire is connected with the ground by another, which, when it meets

* *Agricultural Gazette*, June 7, 1845.

the ground, branches out and spreads over an extent of about the 8th part of an acre, on which wheat is growing: but the plants have not *in appearance* benefitted the least from it. Dr. Forster declines to be responsible (and justly so) for the results of experiments performed otherwise than as he has directed; but we cannot see what influence his arrangement of wires possesses that ours has not in a greater degree.

The following is another communication on this subject, just received:—Since I communicated my trial of Dr. Forster's plan of electrical cultivation, a plot of potatoes, similarly treated, has been followed by similar effects. The rows within the wire are distinctly visible, those without only partially appearing. In both cases, the row adjoining the buried wire is favorably affected, though not included in the square. This lateral influence every electrician would expect; the great wonder is, how the electricity is so perfectly confined within the wires. Though, of course, the theory is nearly a mystery at present, we must infer that the aërial wire collects the fluid and the buried wire distributes it to the ground, through which it percolates and stimulates the vegetation. Of course if you extend the inclosed area too much, the supply of electricity would become inadequate. If you increase the number of aërial wires you would obtain more electricity but fail in its gradual and equal distribution. For these reasons, which I think Dr. Forster has misapprehended, I still recommend future experimenters to try long narrow parallelograms, and avoid large areas. What has hitherto been done on this subject is of small moment: the attempts have been merely trifling and theoretical. Dr. Forster has the sole merit of first suggesting a useful and economical application of this wonderful agency. In fact, the great objection to the success of his system is, that such a sudden advance in cultivation as this promises, has never been made before, and seems almost contrary to the laws of nature.—*Beta.*

A late Number, containing a statement of your mode of supplying the electric fluid to plants, appearing to you as good an arrangement as that which I have, after years of thought directed to the subject, adopted, and which has been since promulgated, will, I hope, be a sufficient excuse for giving some of the reasons why it is not as yet with you attended by beneficial effects. Both the north and the south end of the wire suspended and collecting the electric fluid ought to be in metallic communication with the buried wires, otherwise the circulation of the electricity is intercepted, or nearly so, for I find that the corners of my plots do not affect the magnetised needle similarly; thus, although the south end of the suspended wire attracts the south pole of the needle, and the north end of the wire the north pole of the compass-needle even at some distance, the south-eastern and north-eastern corners of the buried wire both attract the north end of the needle, while the south-western and north-western attract the south pole of the needle; and as this result cannot be obtained with the wires buried and disposed as in your experiment, so also the result cannot be similar to that which it is in my arrangement. Besides, the electric tension of the air has, until the last few days, been so low, that any very perceptible beneficial influence for the last two months could not have reasonably been expected, any more than that a windmill would be of use when there exists little or no wind. Experiment on eight acres in one plot has already proved that large areas may be electro-cultured, although as at first advised by me the parallelograms should be longer north and south than they are wide from east to west in the proportion of one to two or three, and every increased width of about 30 to 40 yards, beyond the first 30, should have at least one additional suspended collecting wire. "Beta" observes, that the effect of the electricity extends without the plot surrounded by the buried wire. My observations lead me to believe that it does so in a very limited degree to the eastward, the line of corn improved on that side being concave, or receding from the east in the mid-length of that side, which it was in about the same degree convex, or tending beyond the wire to the west, on that side. The explanation given above why the diverging wires had not the same influence when supplied with electricity as the circumventing wires, also indicates the reason why the effect is so nearly confined to the plot, or area, included by the latter. I would also advise your floricultural readers to have the whole of the buried wires, poles, &c., arranged at once, if not previously done, but not to erect the suspended wire until the flower-buds are beginning to be formed, otherwise the plants will thrive to an unusual extent, as I have seen this year they did in the hands of a friend, and the flowers will in consequence be injured in a degree, while if the additional power is added just as the flower-buds are formed, nearly the whole of the increased supply of fluid will be employed in the development of the flowers, which are, in consequence, larger, more perfect, and the colors more brilliant.—*R. Dewey Forster, Findrassie, near Elgin.*

FEEDING OF ANIMALS.

BY M. BOUSSINGAULT.

Oily grains contain a large proportion of animal matter, similar in its composition and properties to caseïne from milk. Oil-cake retains the whole of this azotised principle; the proportion of azote, which amounts generally from 0·05 to 0·6, indicates about 42 per cent. of animal matter. Theory indicates so high a nutritive value, that 100 parts of hay may be replaced by from 22 to 27 of oil-cake. Its general use in cattle-feeding is ample proof of its nutritious qualities. One of our most skilful farmers in the south of France has succeeded in keeping sheep almost exclusively on this food. Being at the head of extensive oil-works in the department of Herault, and finding it difficult to dispose of the oil-cake, he determined to connect with his establishment one for feeding cattle, which he keeps on Linseed-cake and the refuse of grapes. Cows subjected to this regimen give, on an average, nearly two gallons of milk a-day. Their food is about 15½lbs. of oil-cake per head, taken at three meals, immediately after which they are supplied with water. Each cow has, besides, 13½lbs. of straw or corn-chaff. The cake is broken into fragments, moistened, and kneaded with water till it is of the consistence of dough. Cows sometimes refuse it; it must then be given in balls about the size of the fist; and, in general, two or three trials are sufficient to reconcile them to their diet. Supposing them to be properly nourished with 33lbs. of hay and that 6 of straw are equal to 1·4 of hay, we find that 15½ of oil-cake take the place of 30 of hay; the equivalent of oil-cake then becomes 51·5, a very different result from 22, which is given by analysis. It must, however, be remarked that the equivalents are very differently stated by authors, as 42, 57, and 108. M. Perrault de Iotemps has found, from observations made on four milch cows, that 500 parts of Rape-cake, given in the form of gruel, are equal to 1,370 of hay. The equivalent, then, is 36·5; analysis indicates 23. We see, then, that results obtained in practice, though very different, agree in giving to oil-cake a nutritive value lower than that which is indicated by theory. I am the more inclined to dwell on this disagreement, because it seems to me to depend on a circumstance which it is highly important to observe. I mean the influence of bulk as distinguished from quality. Vegetable food has, for the most part, nearly the same specific gravity, one, viz., slightly above that of water; the bulk, therefore, of the meal depends on its weight. A regimen, then, formed of some extremely nutritious food, and, consequently, of very small bulk, would present serious inconveniences. A cart-horse of ordinary size requires about 26½ lbs. to 33 lbs. of solid food, and 26½ lbs. to 31 lbs. of water in 24 hours. The bulk of this allowance, reduced by mastication, to the ordinary consistence of the contents of the stomach, is about a cubic foot. Now, if for the ordinary food, one five times more nourishing is substituted, as oil-cake for example, the dry ration, according to the law of equivalents, would be reduced to about 6½ lbs., and its total bulk would be about ten inches cubed. The animal would not be filled, and would, without doubt, experience the pains of hunger. If, on the contrary, food of low nutritive powers be substituted, as wheat-straw, which has an equivalent of 500, the 33 lbs. of dry food would become 165 lbs., and be too bulky to admit of their being consumed in a day. In feeding, then, the bulk of food is a material element; the stomach must be properly filled; the bulk, whatever be the food, must be sufficiently great; and hence arises the difference above-mentioned in the nutritive equivalents. The exact appreciation of the limit beyond which any substance ceases to be nutritive is very difficult. When we add to a regimen, known to be sufficient for the support of an animal, a new portion of food, the effect of the addition is scarcely perceptible; so that in practical observation we are subject to underrate the nutritive value of food, when administered in too large quantities. This has been proved to us by a series of experiments on the feeding of milch cows. A cow, which had in Jerusalem artichokes and dry fodder, the equivalent of 33 lbs. of hay, had in addition to this allowance 6½ lbs of rape-cape, so as, in fact, to double its nutriment. The cow took half only of the cake, and notwithstanding this addition, she did not yield more milk. According to this observation, the equivalent of the cake, though one of the most nutritious substances, was in effect zero.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

ON PHYSIOLOGICAL REAGENTS.

BY DR. BUCHNER, SEN.

If a narcotic alkaloïd be present in almost imponderable quantities, or mixed with a great excess of other substances, and diluted, chemistry is inefficient to demonstrate either their qualities or even bare existence. Physiology, however, furnishes us with far more delicate reagents than those afforded by chemistry: the former are, therefore, to be made available, when the latter cease to render any service. By the aid of physiological reagents, questions, which commonly are medico-legal ones, it is true, may be answered only with a certain degree of probability; but chemistry itself generally gives no information when organic matters are to be detected.

Such was last year the case of the Royal Medical Committee of the University of München, when a tribunal demanded their opinion in a criminal case in which there was a suspicion of murder having been committed by poisoning with a narcotic powder. Only a very small quantity of this corpus delicti was attached to the legal documents, and the powder had already been extracted by buttermilk; in this case chemistry could not give the desired information; but with the aid of the microscope and two living rabbits the questions put by the royal court were answered, partly with certainty and partly with a high degree of probability. It was ascertained that the powder was of a vegetable nature, and possessed the narcotic properties of belladonna. It was compared by means of the microscope with other powders, and with the powder of belladonna also, and was found completely to agree with the latter; the absence of any mineral poison was shown at the same time. The corpus delicti was treated with alcohol; the filtered liquid evaporated at a gentle heat, and the mass thus left extracted by a few drops of distilled water. This solution, on being applied to the eye of a rabbit, by the means of a fine hair pencil, speedily produced a total and long-continued dilatation of the pupil.

As a controlling experiment, the powdered root of belladonna was treated in a similar manner, and the solution finally obtained applied to the eye of another rabbit. The properties of the two extracts, as well as the physiological phenomena, were found to coincide. Notwithstanding this, the medical committee gave their opinion only as a highly probable one: as it could not be decided whether the corpus delicti was prepared from mandragora, hyoscyamus, or stramonium. The eye is so delicate a test for atropine or hyoscyamine that, according to Riecke, $\frac{1}{100}$th of a grain of extr. hyoscyami, and $\frac{1}{1000}$th of a grain of atropine, dissolved in water, and applied to the eye of a rabbit, produces dilatation of the pupil; nay, that $\frac{1}{10000}$th of a grain of atropine, after having been twice applied to the eye, produces the same effect. Runge found that the said alkaloïds of the solanaceæ are again evacuated by the urinary organs, and that, therefore, the urine of rabbits, to which either belladonna, stramonium, or hyoscyamus has been administered, on being applied to the eyes of other animals, invariably produces a dilatation of the pupilla.

Dr. Auer, of Munich, some years ago instituted many instructive experiments relative to belladonna, which we regret have not been published.* From these it appears that all parts of this plant contain atropine, not only the bark, but also the woody portion of the petals, the calyx, the corolla, the stamina, and anthers, gave watery extracts, severally causing contractions of the pupil. The root and seeds were found to be most efficacious, the epidermis of the berries was the least active, and the juice expressed from the berries contained very little atropine; whereas the energy of the calyx was little behind that of the root. According to Riecke, the action on the eye induced by atropine is stronger, and continues for a longer time, than that of hyoscyamine; whereas extractum hyoscyami acts more powerfully than extractum belladonnæ; the latter fact agrees also with the observations of Counsellor Reisinger. The result obtained, however, varies with the species of animals subjected to the experi-

* Buchner's *Repertorium*, vol. xxxvii., 3.

* The treatise of Dr. J. G. Auer, the manuscript of which is in my possession, contains a minute and carefully compiled history of belladonna, in a botanical, chemical, pharmaceutical, medical, and toxicological point of view, with many new physiological experiments with rabbits, cats, dogs, and even human beings, instituted and described by the author.

ment. Dr. Auer has shown that the eyes of cats and dogs are far more susceptible of the action of atropine than those of rabbits, the latter being sometimes scarcely at all affected by a preparation, an equal quantity of which may in cats cause the iris almost completely to disappear, and to continue in this state for several hours. Similar differences were noticed after the internal use of narcotic vegetables, herbivorous mammalia being in general less affected than the carnivora. Riecke found that 5 grains at least must be given to a rabbit to produce dilatation of the pupil. It must, however, be borne in mind that other narcotic bodies, especially opium, salts of morphia, cicuta, œnanthe, and even digitalis, if they are administered internally in large quantities, may sometimes cause a dilatation of the pupil; so that from this physiological phenomenon we cannot conclude with certainty which is the species of the alkaloïd in question. It is known that strychnine and brucine are distinguished from all other alkaloïds by the fact that on being in very small quantities applied to a vein or a fresh wound, they produce most violent irritations of the spinal marrow, and the motory nerves with tetanic fits; this physiological property may likewise be made available in the detection of nux vomica or strychnia.

W. Arnold ascertained by his experiments, that a living frog was the most delicate reagent for it, if the preparation of strychnia or brucine be introduced into a wound inflicted in the skin of the back. A very trifling dose of these is sufficient to produce tetanus in the frog. One drop, for instance, of a concentrated solution of nitrate of strychnia, prepared with cold water, was diluted with 99 drops of water; from 10 to 12 drops of this dilution, applied in the above manner were found to be sufficient to produce tetanus. One grain of a mixture of one part of strychnia with 99 parts of milk of sugar applied to a wound in the skin of the back of a frog, caused tetanus after 8 minutes, and when afterwards one grain taken from this mixture was rubbed with 99 grains of fresh sugar of milk, tetanus followed after 13 minutes, though only $\frac{1}{10000}$th parts of strychnia had been employed. On further diluting the dilution with sugar of milk Arnold found, that $\frac{1}{1000000}$th of a grain of strychnia, after half an hour or later, increased the irritability of the frog to a certain extent.

Having, however, in one of his experiments employed a frog, which on the preceding day in consequence of a dose of $\frac{1}{10000}$th part of a grain of strychnia, suffered from tetanus for several hours; in this animal $\frac{1}{1000000}$th of a grain of strychnia produced tetanus, lasting for some hours, and proving fatal. Similar experiments have been instituted with nitrate of strychnia; $\frac{1}{100}$th of a grain of this salt produced tetanus after seven minutes, and $\frac{1}{10000}$th acted similarly after 15 minutes. Extractum nucis vomicæ spirituosum, in an alcoholic solution diluted 100 times, put under the skin of a frog, produced, after 20 minutes, tetanus, even though only one drop of the concentrated solution was contained in the dilution; $\frac{1}{1000}$th of a grain of an alcoholic solution of extract of brucine, on being applied only once, produced no complete tetanus. The aqueous extract of brucine acted more feebly; one drop of the same, diluted with 99 parts of water, caused tetanus, only after 38 minutes; and $\frac{1}{10000}$th of a grain produced no sensible effect. One drop of tinctura nucis vomicæ diluted with 99 of water, produced, after an hour and a half, tetanic rigidity. These experiments show how uncommonly sensible frogs are to strychnia, and, therefore, how well they are qualified for demonstrating, at least with some probability, quantities too small to be discovered by chemical means.

Dr. P. Pickford, of Heidelberg, communicated a treatise, very worthy of a perusal, "On the Results of some Experiments relative to the Knowledge of Narcotic Remedies in general,"* and, after having repeated these experiments, he found the same facts corroborated; but the blood of frogs which had been poisoned with large doses of strychnia, having been introduced under the skin of other frogs, produced in the latter no tetanic symptoms; the same was the case with the urine of poisoned frogs. The blood of a rabbit killed with 60 drops of a concentrated solution of nitrate of strychnia, one ounce of which being introduced into the abdominal cavity of another rabbit produced no symptoms of poisoning in the latter. The same negative result was obtained with the blood of rabbits and dogs killed by prussic acid or strychnia, when applied to rabbits or frogs in the above manner. Finally, several rabbits, killed with large doses, were given to a large dog, who consumed them without injury to his health. From these experiments Dr. Pickford concludes that the symptoms of poisoning are not produced by the irritation of the narcotic circulating in the blood, but by the peculiar affinity of the poison to the nervous mass; owing to which, on coming in contact with the blood, and thus intimately combining with the same liquid, it produces a change in the vital properties of the nervous system, in such a man-

* *Archiv. für Physiol. Heilk. v. Dr. Koser. Jahrg.* iii., 3, pp. 347—371.

ner that the blood may be considered only as the vehicle of the poison.

We may perhaps still better explain this fact in the following manner:—The alkaloïds enter with the blood into a chemical combination, similar to that of iron, which also cannot be discovered until the blood is destroyed, though the amount of iron is considerable, in such a manner, that only the living blood can act on the living nerves of the same organism, or, in other words, the narcotic must first enter into an organic combination with the living blood to act on the nervous system; hence from regions abundantly provided with vessels the narcotic action is propagated with greater celerity and with more energy; and several physiologists have found by experiments on living animals, that on bringing narcotics into immediate contact with the brain, medulla, or nerves, they do not act as narcotics if carefully prevented from communicating with the blood-vessels; if, therefore, contradictory symptoms are sometimes observed, this may be owing to the incomplete exclusion of the coöperation of the blood.*

Several narcotics, as strychnia, morphia, nicotin, and cyanogen, in the state of soluble solutions, brought into contact with the heart of a frog, laid bare, may paralyse the action of the heart, as appears from experiments instituted by Fontana, Humboldt, Wilson, Brodie, Arnold, and Pickford, in searching after these substances this physiological property could likewise be made use of. The known constipating and antiemetic property of opium, as Dr. Pickford has clearly shown,† is founded only on the circumstance that the narcotic exerts a paralysing effect on the stomach and bowels.

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ON THE PHARMACOLOGY OF ALCOHOL, ETHER, FOUSEL-OIL, AND ETHERIAL OILS.*

BY FR. APOIGER.

I. ALCOHOL.

It appears from numerous experiments with animals and man, instituted by Percy, Magendie, Mayor, Courten, Baglio, Lanzoni, Friend, Sprögel, Viborg, Brodie, Segalas, Orfila, and Tiedemann, that alcohol, after being swallowed, or injected into the stomach of an animal, produces, first, irritation and redness in the mucous membrane of the stomach, and is then absorbed and conveyed into the blood, and exerts an exciting action on the heart and brain; that it accelerates the pulsations of the heart, and induces intoxication in the cerebral activity; that this effect on the heart and brain takes place also if the alcohol be injected into a vein; that the alcohol partaken is evacuated by the urine; that an adult rabbit was not killed by 1½ ounces of spirit of wine, containing 44 per cent. of absolute alcohol, injected into the stomach; that, however, with spirit of wine containing 90 per cent. of absolute alcohol, the consequences are fatal, &c. Dr. C. G. Mitscherlich instituted a fresh series of experiments on the physiological action of alcohol, and communicated the results in the *Med. Zeitg. d. v. f. h. in Preussen*, 1843, No. 20. From this we borrow the following facts and conclusions:—

Casein and albumen, on being coagulated by alcohol, are insoluble in water. If the alcohol be considerably diluted with water it no longer coagulates the albumen; hence, the same cannot pass over from the stomach into the blood without effecting the coagulation.

On the surface of the body alcohol produces only a sensation of cold, the natural consequence of evaporation, without causing any perceptible change in, or penetrating through the epidermis; the cells underneath are, in some measure, rendered hard and dry. Through the epithelium of the stomach, however, alcohol passes much more easily, speedily producing a slight inflammation, with the sensation of burning; the cells of the epithelium contract into a smaller volume, probably owing to loss of water.

In fresh wounds, also, if the nerves are laid bare and wounded, alcohol instantly produces violent pains and inflammation.

One ounce of absolute alcohol, injected into the stomach of a large rabbit, almost instantly produced diminution of sensibility, motion, and functions of the senses, with great debility of the muscles. Circulation and respiration were highly accelerated; finally followed slight convulsive motions of the feet and eyes; the pupil, narrow at first, afterwards dilated; the breath became shorter and feeble, the pulse continuing highly accelerated; finally, after from 1½ to 2 hours, death followed without convulsions. Immediately after death, having opened the abdominal cavity, an odor of alcohol was distinctly perceived, with a feeble peristaltic motion of the intestinal canal, and irritability of the muscles. The stomach was, externally, red-brown, partially grayish-brown and grayish-black; it contained a great deal

* Similar contradictory phenomena have been collected by Dr. Pickford, l. c. 373.

† L.c. 368.

* Buchner's *Repertorium*, xxxvii., 3.

of liquid exhaling the odor of alcohol; the cells of the mucous membrane were contracted, and the same membrane in some places covered with blood globules. The epithelium was in most places found to have shrunk. The vascular membrane of the stomach, especially in the region of the fundus, and the great curvature, was red-brown, gray, and black-brown, and gorged with blood; it contained a considerable quantity of coagulated blood in an uncoagulated state. The muscular tunic, and the peritoneal coating of the stomach presented no visible alteration. In the small intestines no other change was observed than that the epithelium was partially converted into mucus. The alcohol seemed to have been retained in the stomach by the contraction of the pylorus. All the other organs seemed to be normal; the vessels of the meninges and brain were not particularly filled with blood.

Inflammation of the stomach has, however, frequently been noticed in men who have died in consequence of the excessive abuse of strong brandy; since alcohol attracts water, we can easily account for the induration found in the stomach of deceased brandy drinkers. If the brandy is highly diluted with water, then, like wine, it is not capable of causing the said change of structure. That, moreover, the alcohol passes from the stomach into the blood, the liver, bile, urine, and even the brain, and may in all these organs be shown to be present by chemical means, has already been proved by John Percy (*Experimental Researches, &c., London*, 1839). This passage of the alcohol from the stomach into the blood is not brought about solely by the veins, but likewise by its penetrating the epithelium, and diffusion in the texture of the vascular membrane being thus partly received by the vessels, and partly conveyed to the muscular tunic; this absorption follows more rapidly after the use of diluted than concentrated alcohol. The effects of alcohol on the heart, brain, medulla, and nerves of the senses seems to be propagated from the stomach in a sympathetic manner.

II. ETHER.

A great number of observations relative to the pathological effects of ether have been communicated; however, they have hitherto been but little confirmed by experiments on animals. Orfila injected ½ an ounce of ether into the stomach of a dog, and then tied the œsophagus. The first symptoms were inclination to vomit, then vertigo, temulentia, accelerated and embarrassed respiration. After 40 minutes the animal recovered so far as to be able to keep an erect position; it, however, continued staggering; afterwards insensibility followed again, and after three hours, death, without convulsions. The stomach contained brown mucus, and the mucous membrane was black-brown, as is the case after intoxication with alcohol. Of the intestinal tract the duodenum alone was reddened. The lungs were filled with blood, and the blood was found partly in a coagulated state. Orfila introduced 3½ drachms of ether into the cellular tissue of a wound in the thigh of a dog; after 12 hours the animal uttered frequent whining cries, staggered, gradually becoming more exhausted, and died the fourth day.

Jäger has also instituted several experiments with rabbits, ducks, and pigeons, and found that ether most rapidly pervades the organism, causing the spleen, liver, and kidneys to become gorged, and that the effect may easily have a fatal issue.

Mitscherlich found that the physiological effects of ether have some resemblance to those of alcohol, and are distinguished only by the circumstance that the ether is far more volatile, and subtracts no water from the organism. Diluted albumen and milk, on being shaken with ether, are separated with a gelatinous appearance; the albumen of the serum of blood, however, suffers no visible change; it is the same with the epidermis; the cells of the epithelium do not contract. Ether seems, therefore, to exert no chemical action on the skin, and to subtract no water from it, differing in this respect from alcohol. The cold produced by evaporation on the skin is much greater; if, however, it be prevented from evaporating, and if the action continues for a long time, the vital process is excited in the skin, heat and burning are perceived, but the epidermis undergoes no change.

By introducing ether into the cavity of the mouth, the epithelium is not changed, as is done by alcohol; a sense of burning, however, is speedily perceived, and a considerably augmented secession of the epithelium with inflammation may follow, because ether passes easier through the mucous membrane than through the epidermis. The sense of burning most rapidly follows in the eye and wounds.

One drachm of ether was injected into the stomach of a large sized rabbit; without any perceptible excitation the abdomen became tympanitically inflated; the progress of the ethereal gas into the smaller intestines became audible. Almost immediately after the injection the animal was rendered insensible and benumbed; it fell on one side, and was unable to stand upright; the exertions made in breathing were gradually augmented,

and after fourteen minutes it died, without convulsions. Immediately after death the abdominal cavity was opened, when a distinct odor of ether was perceived; the peristaltic motion of the abdominal canal was only feeble, the muscles contracted by mechanical irritation; the stomach and small intestines, up to the cœcum, were in the highest degree inflamed, and inflated by the ethereal gas. The stomach, being externally, chiefly at the fundus ventriculi, and the great curvature of a brown-red color, contained little ether, but much food, which seemed to have undergone no considerable changes. The mucous membrane was in some places covered with blood, exhibiting cells partially normal, partially swollen, partially contracted; in the epithelium itself, which was very much reddened, blood globules were found, and cells, which for the greatest part exceeded the natural bulk from eight to ten times. The vascular membrane was highly injected, and softer than in ordinary cases; in the muscular tunic of the stomach, and peritoneum, no change was discovered. The duodenum being likewise inflamed, and externally of a very bright colored red, contained much mucus and ether, but little food; an uncommonly marked secession of the epithelium was noticed, it being converted into mucus, with extravasated blood under it. The cells of the epithelium of the stomach and duodenum are either not at all changed by ether, or for the most only contract; they most probably swell, in consequence of the congestion of the blood; and from this cause the secession of the epithelium may likewise be derived. The urinary bladder was found full, the heart gorged with blood, the other organs normal.

Several other experiments were instituted with rabbits, into the stomachs of which Mitscherlich injected half an ounce of ether, when the abdomen invariably became meteoristically inflated, and death followed in a few minutes by suffocation. If this termination followed very rapidly, then on dissection, immediately after death, the intestinal canal was found unaltered; if death followed after five minutes, inflammation of the stomach and small intestines was already present, to a less extent, however, than in the first experiment, in which, after having administered one drachm of ether, death followed after fourteen minutes. The urinary bladder was found either filled or evacuated shortly previous to death.

Ether, therefore, acts by its thin liquid and volatile nature, and in two peculiar ways; it expands, in proportion to the quantity of liquid contained in the stomach as gas, and permeates with great ease and rapidity the membranes of the stomach and intestines; pervading in this manner the epithelium, it arrives in the vascular membrane and the blood vessels themselves; by this it causes vivid irritation, and inflammation of both stomach and small intestines. That it likewise becomes a diuretic by its rapid penetration into the vessels, is known. It permeates also the muscular tunica, and the peritoneal coating of the intestinal canal, and as a gas it passes into the abdominal cavity, the same having been proved relative to alcohol and ammonia. A chemical decomposition of the organic texture seems not to occur.

III. FOUSEL-OIL.

The poisonous qualities of the fousel-oil seem to have been overrated. Dr. Edward Fürst, of Berlin, deserves, therefore, our thanks for having thrown a new light on this matter by experiments instituted with animals, published in the *Med. Zeitg.* 1844, Nos. 23 and 24.

He procured some fousel-oil from a manufactory of potato brandy, and purified it by shaking with water, thus removing the alcohol. It was colorless, of a most disgusting odor, which strongly affected the organs of respiration, and of a burning acrid taste. The specific gravity he found to be 0·8.* We may be allowed to remark, in a chemical point of view, that according to the investigations of A. Cahours, fousel-oil is a liquid analogous to alcohol, the radical ($C^{10}H^{22}$) of which obtained the name of "*amyl*," and like alcohol, which being considered as the hydrate of oxide of ethyl, is expressed by $C^4H^{10}O + H^2O$. The potato fousel-oil, looked upon as the hydrate of oxide of amyl, is calculated as $C^{10}H^{22}O + H^2O$.† Dr. Fürst found that fousel-oil, like alcohol, may coagulate albumen and casein, being, however, endowed with less affinity for water than alcohol, it cannot contract the mucous membranes; and being also less volatile than the latter liquid, it cannot permeate the organism with so much facility, and its action is also less irritative.

The physiological experiments have been instituted with rabbits; in one experiment only a dog was employed. The fousel-oil was injected into the stomach in the manner directed by Mitscherlich, according to whom a thin elastic catheter was introduced into

* According to Pelletan the specific gravity of fousel oil of potatoes at 62°F., is 0·823; and, according to Stickel, the fousel-oil of wine is=0·856.

† *Vide* Buckner's *Repertorium* lxx., 1st series, p. 170.

the esophagus of the animal, and the fousel-oil injected by means of a syringe. In this manner, to a middle-sized rabbit, one drachm of fousel-oil was administered. The animal remained tranquil for some time; became afterwards gay, and appeared to be intoxicated; it began to run, striking itself against objects standing in its way; the number of pulsations was extremely augmented, and the same was the case with reference to the temperature of the body, especially on the ears. After ten minutes it was so exhausted that it could no longer stand upright, and seemed to be deprived of sensation. After half-an-hour the lungs exhaled the odor of fousel-oil. This state lasted two hours and three-quarters, after which the animal gradually recovered. A second, and rather larger, rabbit was less affected by one drachm of fousel-oil; it seemed to be not benumbed by it. Two drachms of fousel-oil were injected into the stomach of a very large rabbit: during the first few minutes, the animal remained tranquil; after a quarter of an hour, however, followed restlessness, heightened temperature of the body, especially of the ears, and the number of the pulsations of the heart was highly augmented; the respiration being deep and slow. Universal debility was then observed, and after seven hours, diarrhœal evacuations followed, and finally recovery. Two drachms of fousel-oil were also injected into the stomach of another rabbit: this animal immediately appeared to be benumbed; rotary motion followed, the common attendants of intoxication, and an extraordinary acceleration of the pulsations of the heart and the respiratory functions. After this, the animal seemed to be deprived of the faculty of motion and sense, the respiration became slow and difficult, and after one hour and a half death followed. Feces and urine had not been evacuated. On opening the abdominal cavity a strong odor of fousel-oil was perceived; the stomach contained much mucus; in the fundus ventriculi extravasated blood of a dark brown color was noticed; the cells of the epithelium were in a swollen state; the duodenum and small intestines reddened, and covered with mucus; the bladder was empty, the kidneys normal, and without blood; the lungs more vividly reddened than in ordinary cases.

In addition to these, similar experiments were instituted with three other rabbits; into the respective stomachs of which were injected three drachms, four drachms, and one ounce of purified fousel-oil of potatoes. The phenomena were numbness, accelerated pulsations of the heart, raised temperature of the body, chiefly of the ears, and afterwards senselessness, and death—which, after three drachms followed in 55, after four drachms in 15, and after one ounce in four minutes. After three and four drachms, Dr. Fürst observed that the respiration became slow, and the gas exhaled by this function had the odor of fousel-oil. The feces and urine deposited by the animal shortly previous to death had no similar odor. It is highly remarkable, that the stomach was by no means inflamed; on the contrary, it was found to be more pale than commonly, but covered with a great quantity of mucus and a brown-red extravasated blood; the cells of the epithelium were highly relaxed, and softened. The superior half portion of the small intestines was in one rabbit rather reddened, and contained much mucus; heart and kidneys were normal; the lungs somewhat overfilled with blood. In the two other rabbits, which died respectively after 15 and 4 minutes, the heart, liver, kidneys, and lungs were found in a normal state.

Four drachms of fousel-oil were injected into the stomach of a middling-sized young dog. The dog did not vomit, and was at first gay and very agile; after five minutes it seemed to be intoxicated, like blind, striking against almost all objects standing in its way; it continued, however, to hear when called. After some minutes an abundant secretion of a tenacious saliva followed; after a quarter of an hour spastic movements were perceived, and the secession of a yellowish tenacious mucus, with some fecal matter from the rectum; after these, loss of motion and sense followed; the pulsations of the heart were not extremely frequent, the respiration anxious, short and quick; the exhalation of the lungs offered the odor of fousel-oil, the pupilla was widened. After 25 minutes the animal uttered most distressing cries: this state lasted five minutes, when torpor supervened, and death after six hours. Dr. Fürst likewise found in this animal, that the stomach and intestinal canal were not inflamed on their exterior surface; the stomach contained a great quantity of mucus, mixed with cells of epithelium; in the fundus ventriculi, the epithelium was particularly softened, the mucous membrane reddened only on some spots which were rather prominent; the duodenum and small intestines normal, and without odor of fousel-oil; the same was the case with the kidneys, lungs, and brain. The main features of the results of these experiments are the following:—The fousel-oil acts at first as an exciting and intoxicating agent, and exerts afterwards a depressing effect on the whole nervous system by causing sleepiness; it affects the stomach in a sympathetic manner; the mucous mem-

brane of the stomach and intestinal canal becomes relaxed, and is destroyed, and the tunica propria itself, chiefly in the fundus ventriculi, is found to be attacked; the fousel-oil is absorbed, as it may be recognized by its intoxicating power, and by the odor exhaled from the lungs; it does not act on the kidneys. After large doses only, death follows. —nt—

(To be continued.)

ON KYSTEIN DISCOVERED IN THE URINE OF PREGNANT WOMEN.*

BY KLEYBOLTE.

To discover a sure criterion of gravidity, which under all circumstances, and in the first months also, never fails to be decisive, has long baffled the attempt of medical men; such as have hitherto been recommended are partly uncertain, partly rarely practicable, to ascertain which being frequently repugnant to female modesty. To the latter belongs the livid color of the *rima pudendi* and vagina, lately recommended as an unerring mark by Kluge. The character stated by Nauche may in all cases be ascertained, and medical periodicals have communicated observations which corroborate the certainty of its indications. I have myself instituted experiments with the urine of ten females, at various stages of gravidity, and found that the fifth day the kystein was invariably formed. I never found it in the urine of persons not being pregnant; a doubt, however, is still left, whether the same is not formed likewise in diseases of the female parts, as my own experience has not yet furnished me with a number of cases sufficient to settle this point. If the latter case does not occur, nothing will stand in the way of making this test of a practical application, but the inconvenience of waiting six days until the kystein is formed. In the following I have communicated my observations on this matter with the remark, that in all the ten cases the urine has been deposited in the morning before breakfast, and after being slightly covered to keep off dust, and other small bodies, it was allowed to stand, at a mean temperature, for ten days.

First Case.—Urine of the tenth week of gravidity, color peculiarly yellow, with a greenish tint, similar to the color of clear, recently boiled veal broth. 2nd day, mucus sediment, verging into yellow, no other change. 3rd day, no change. 4th day, Turbidness ascending from the bottom of the vessel. 5th day, white points and leaflets on the surface, turbidness ascending from all parts of the bottom; the sediment is almost entirely dissolved. 6th day, kystein distinctly observed on the surface, the same has completely the appearance of cooled fat eyes swimming or bouillon. 7th day, no change. From the 8th to the 10th day the kystein disappears, the turbidness descending again, and the sediment noticed the second day reproduced.

Second Case.—Urine of the 6th month of gravidity, color the same as in No. 1. From the second to the tenth day the same changes as in No. 1.

Third Case.—Seventh month of gravidity, the urine has a yellowish color, and is by no means distinguished from the common one deposited in a healthy state. 2nd day, the sediment is verging into the yellowish-red; other changes up to the tenth day are the same as in No. 1.

Fourth Case.—Urine in the eighth month of pregnancy, color as in No. 1, but somewhat deeper yellow. 2nd day, yellowish sediment. The changes in the other like that of No. 1.

Fifth Case.—Eleven weeks after conception, the urine of the usual light yellow color. 2nd day, yellowish sediment. 7th day, the kystein is augmented to an extent as to cover almost the whole surface; the other changes remaining the same as in No. 1.

Sixth Case.—Fourteen weeks after conception, the urine clear like water, resembling the spastical urine. Changes from the second to the tenth day as in No. 1.

Seventh Case.—Sixteen weeks after conception, the urine of a yellow color. 2nd day, a yellowish-red precipitate; the rest up to the tenth day like No. 1.

Eighth Case.—Urine of the 7th month of gravidity, colored yellowish; 2nd day, yellowish red precipitate; for the rest as in No. 1.

Ninth Case.—End of the third month, color and changes of the urine as in No. 1.

Tenth Case.—Sixth month of gravidity, the individual suffers from rheumatismus acutus developed in a high degree; the 3rd day, a yellowish-red precipitate is formed, and the sixth day kystein could not be overlooked. From these cases the following may be deduced.

I. The urine of pregnant females has no determinate color, but varies in a variety of shades from the yellow color to the achromatic limpidity of water. I lay particular stress on this point, as it is stated in an article published at the commencement of 1841, in Schmidt's *Jahrbucher*, that the

* Casper's *Wochenschrift*, April 26, 1845.

color described in the first case is invariably met with.

II. Towards the second and third day turbidness and mucous sediment are formed, of a yellow or yellowish-red color.

III. The fourth day the sediment is dissolved, and renders the turbidness universal, which ascending towards the surface, the 5th or 6th day forms the kystein.

IV. The eighth day the kystein is dissolved, and the sediment is reproduced again.

V. It follows from No. 10, that rheumatismus febrilis at least does not interfere with the formation of kystein.

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NOTE ON KIESTEINE, A SUBSTANCE PECULIAR TO THE URINE OF PREGNANT WOMEN.*

BY M. LOUIS VICTOR AUDOUARD, OF BEZIERS.

SOME foreign physicians, Bird, Stark, Letheby, Vannoni, and Cozziz, having, in turns, studied a substance which is found only in the urine of pregnant women, and which has been called gravidine or kiesteine. The French scientific journals, at least those which I read, having given only abstracts of the works of these experimenters, I resolved to seize the first opportunity of trying some experiments on the same substance.

A young lady of Beziers, in the third month of pregnancy, and of very good constitution, having been kind enough, in the month of November last, to lend herself to my experiments with the best grace in the world—that is to say, to supply me with her urine several times—I was enabled to examine kiesteine. The results which I obtained not agreeing perfectly with the results mentioned in the extracts of the works I have mentioned, I have now decided on publishing an article on this substance.

The urine was always that passed in the morning, as I had requested. Examined only two or three hours after its remission, this liquid had an urinous odor, but I could distinguish in it another peculiar odor, insipid and sweetish, which, however, I cannot exactly define. It was light colored, and perceptibly, but slowly, reddened litmus paper. Left to itself, in a large test-glass, it deposited, in a short time, a very considerable quantity of whitish pellicles: it is known that these pellicles constitute keisteine or gravidine.

I filtered, at each experiment, about 300 grammes of this urine. The filtered liquid was perfectly transparent; no species of coagulum was formed by ebullition.

The pellicles remained at the bottom of the filter; I washed them perfectly, by pouring distilled water on the filter several times; then, having turned it, I plunged its inferior end into about 200 grammes of distilled water, always cold. I kept them immersed for four hours. After this time the pellicles were detached from the filter, and the water had become whitish. By acting thus, I was enabled to isolate kiesteine, and to obtain it simply in water. The following are the characters which it presented in that state:—

Sulphuric and nitric acids dissolved only a portion of it. There always remained in the liquor some undissolved pellicles. The foreign experimentalists whom I have named, found, on the contrary, that kiesteine was completely soluble in these two acids.

Heated to ebullition in a porcelain capsule, the water containing the pellicles dissolved them, and became nearly transparent: the boiling solution was neutral; but the cooling of the liquor did not cause all the pellicles to re-appear, as the above experimentalists have stated. After a very long time, only a few appeared here and there, adhering to the bottom and sides of the glass. The liquor from which they were insensibly precipitated retained the viscidity of a weak solution of gum. Infusion of galls, subacetate of lead, and bichloride of mercury produced an abundance of whitish flakes, which render this liquid similar to the aqueous solution of sperm and other mucous matters.

Agitated for some time with sulphuric ether in a corked cylindrical vessel, then left for 24 hours to settle, the same water containing the pellicles presented the following phenomena:—

The liquor had become very limpid; the ether had separated the pellicles, and retained them at the upper part of the liquid, no longer in the form of pellicles, but in the form of a greyish emulsive matter, which gave to the lower part of the layer of ether something of the appearance of barley syrup. After having been separated with all the necessary precautions, the ether, evaporated first at the ordinary temperature, and then at a very gentle heat, left at the bottom of the capsule a thin layer of a greyish substance. This substance, on the addition of a small quantity of cold distilled water, was converted into whitish pellicles having the same properties, since they are partially in sulphuric and nitric acids, and completely soluble in boiling distilled water; and as the aqueous solution from which a portion was separated a long time after cooling, was pre-

* *Journal de Chimnie Médicale*, May, 1845.

cipitated by subacetate of lead, infusion of galls, and bichloride of mercury.

Such were the different characters several times presented by the kiesteine which I examined. One principal fact appears to me to result from my experiments as well as from the foreign works I have mentioned, namely, that the discovery of kiesteine in *recent* urine presents an excellent means of deciding on a case of pregnancy, in more or less doubtful cases.

Since my experiments, I have examined six samples of urine of young women *affected with* amenorrhœa. I found kiesteine in five of these samples. The sixth, whose odor was very powerful, and whose color was very deep, did not contain pellicles, but a very abundant pulverulent whitish deposit, almost entirely composed of phosphate of lime. The woman to whom this urine belonged was not pregnant: a serious affection of the kidneys and uterus, for which she is under treatment, gave rise to the abnormal urine which she passed.

Moreover, I had to examine the urine of a young actress of the Beziers theatre, who believed herself to be either pregnant or a prey to a dangerous disease. Here the case was very difficult. This lady experienced, night and day, for two months, an extraordinary and truly unquenchable thirst: the necessity for drink was so imperative that she was often compelled suddenly to leave the stage, and go behind the scenes to quench her thirst. The great quantity of water which she swallowed every moment compelled her to empty her bladder very frequently, so that it never continued well for urine; thus I could only act on a colorless, tasteless, and inodorous liquid, having only very slightly the characters of normal urine. However, three pellicles of kiesteine which I was enabled to discover and extract from it led me to announce to the young lady that, in my opinion, she was almost to a certainty in a pregnant state. The event confirmed my opinion, and now there is no longer any doubt as to the interesting situation of the young actress. The thirst still exists almost to the same degree; but it probably constitutes one of those often strange peculiarities which woman presents in a pregnant state.

UNCOMMON EFFECT OF AN ENEMA WITH OPIUM.*

BY DR. STEINTHAL.

To a man about thirty, with rather an inert intestinal canal, piles, lately visible, habitual cephalæa, frequent hypochondriacal state of mind, on rather a hot evening, I ordered to be administered an enema of amylum, containing one grain of pure opium. The day preceding this evening, the patient suffered in consequence of a great error in diet, by copious colical pains, attended with a mucous watery diarrhœa, which caused 25 openings on that day, whereby he became much weakened, and suffered, as is usually his case, by headache. The abdomen was somewhat tumid, but not sensible on pressure, the tongue pure, the temperature of the head normal, the pulse somewhat irritated. Previous to applying the enema, I ordered cold fomentations on the head, which on previous occasions happened to alleviate his sufferings. Half an hour after, I was sent for with great anxiety, and found the patient in a state of considerable opiumnarcosis; he complained of a most intolerable headache, yet the head was not hot, the eyes were red, in tears, glossy in appearance, half opened, the the face reddened, the tongue very dry, head and face at the slightest touch uncommonly sensible, to an extent that even the change of the cold fomentations caused some pain. The patient was in a state much like intoxication, highly talkative, but sometimes in half-finished phrases, most abruptly speaking to the questions put to him, however, properly answering, showing the tongue whenever requested, moving arms and legs; after some time, however, apparently in a state of great debility, in a manner that one or the other arm, on being raised to the head, soon fell down again. Patient was very sleepy, but could not, however, sleep, and was laying down only in a half soporiferous state. A cup of an infusion of strong coffee (without milk, half an ounce to one cup) soon produced a notable remission of all the phenomena, but this effect lasted merely a quarter of an hour, the narcotic symptoms became violent again, and, gradually lessening, continued for several hours, until the patient was overcome by a sleep which was at first agitated, but finally became normal, from which, though somewhat affected, the patient awoke, gay and nimble, the colical pains and diarrhœa having then ceased entirely. A tenesmus-like sensation was only once noticed; three hours after the enema had been administered, the patient desired to have an opening. He, however, remained benumbed, and the muscles relaxed to an extent that it was only with my assistance and some exertion that he could put himself in an erect position, the head spontaneously fell backwards, and I required the assistance of another person to put him again into his bed. The remission took place gradually,

* Casper's *Wochenschrift*, May 3, 1845.

Some authors maintain that opium has a stronger effect on being introduced by the rectum than by the stomach, and we find in the medical annals recorded several instances of poisoning caused in that manner. According to Dupuytren, from five to six drops of opium tincture, given as an enema in the delirium tremens, act with greater energy than three times the dose taken internally. I feel, however, inclined to doubt whether this statement is of general application. Horn ordered in the *charité*, in many hundred cases, opium enemas to be administered in a similar manner, without having met with a single case of subsecutive narcosis. I cannot recollect myself of a similar occurrence, and we must admit that the foregoing must be attributed to an individual idiosyncrasy. I may, however, be allowed to remark, that the above is to be considered as a hint to be particularly cautious in administering enemas containing very active principles to persons affected with piles, or such individuals as have shortly previous lost blood from the arms. The patient in question had leeches applied to the arms a short time previous to the above enema.

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MEMOIRS ON ETHEREAL TINCTURES.

BY M. EMILE MOUCHON.

SECOND PART.

The foregoing details are, doubtless, tedious to certain light minds who pay little regard to the foundation of these things; however, they should not appear uninteresting to those who reflect maturely, and who desire to base their judgment on positive facts. Now, by placing facts by the side of the data hitherto admitted, I think I have already proved, to a certain extent, that these data are false and liable to dispute, as regards almost all the ethereal tinctures most used in medicines, and that they may be correct as regards some others, without, however, anything decisive with respect to any one.

Indeed, if on one hand, we have reflected on the nature of the solvent with relation to that of the substances to be treated; if we have also observed that the matter dissolved is small in proportion to that which resists the action of the solvent; if, on the other hand, we have acquired the certainty that treatment by ether leaves in tact all the extractive matter, that, in a word, which should constitute the properties of certain vegetables, serving as a basis for the products under consideration, such as the *solanaceæ*, aconite, digitalis, and hemlock; if we reflect, I say, on all these things, what should we think of all those ethereal tinctures which should most fix our attention? Nothing very advantageous, doubtless, and it is not without difficulty that one sees his conviction so powerfully shaken, in presence of a class of medicines which have been daily used in medical practice for many years, without endeavoring to give an account of the true nature and physiological action of these same agents.

As we have seen, all the alcoholic extracts resulting from the second treatment are, in every respect, similar to the alcoholic extracts of the codex (French pharmacopœia). To assure myself of this fact, which I consider authentic, I took care to exhaust with alcohol at 21°, and by displacement, cients, aconite, belladonna, digitalis purpures, hyosciamus, and strammonium, in order to be able to compare the quantity of products obtained with that which I was able to extract from the same plants by aqueous alcohol, after treatment by sulphuric ether. That which I expected, occurred: with very little difference, more or less, I have always collected, with both, the same quantity of extract; as I have had occasion to say several times, I have constantly found a perfect identity between the products examined physically. Moreover, nobody is ignorant that ether exerts no action on the extractive principles, and that which I have proved in this respect confirms that which Beaumais knew, although, as he himself said, in his time, the ethereal tinctures of castoreum and amber alone were employed.

From all this, but one conclusion, completely unfavorable to ethereal tinctures, can be drawn, and the opinion which is contrary to them is strengthened more as we advance further into the field of investigation; however, we have yet found only probabilities, strong, it is true, and we want to acquire positive certainties in order to leave nothing in doubt.

What, then, is it necessary to do to render our conviction full and entire? The thing is very simple: it is necessary to study, physiologically and comparatively, all the ethereal extracts, residues of concentrated ethereal tinctures, and all the alcoholic extracts collected, either after exhaustion by ether, or by the direct treatment of the plant by weak alcohol. It is evident that if these first products be ascertained to have no action, and that if the other, on the contrary, be very energetic and even mortal to the animals we have left to the chances of death, we shall leave completely torn away the veil which has hitherto concealed a mystery, and the

principal object which I have proposed to myself will be completely attained.

The useful information which will necessarily result from the physiological and toxicological experiments which form the subject of this second part, must make us feel the necessity of entering into general considerations relative to the modes of operation to be put in practice, to the nature of the solvent to be employed, and will naturally lead us to seek for the means proper for converting into reality, at least so far as appears to us possible, that which is doubtless only the result of a gratuitous supposition.

PHYSIOLOGICAL AND TOXICOLOGICAL EXPERIMENTS.

First Series—Ethereal Extracts.

Experiment I. Ethereal Extract of Aconite.—Eight grammes of ethereal tincture were evaporated in a capsule; the product amounted to 60 centigrammes of soft extract.

Sulphuric ether having a great influence on certain animals, and particularly on gallinaceous ones, as I have ascertained, I took care to expose the ethereal extract of aconite for a long time to the air, as well as all those employed in my experiments, in order to avoid any action but those of these products.

These 60 centigrammes of extract were introduced, *divided* in sugar and water, into the stomach of an adult hen, who exhibited no sign of indisposition, and ate with customary avidity the food presented to her.

The dose was doubled a few days after, then quadrupled, without the animal suffering; she appeared as usual, and her appetite was not impaired. Now, it will be observed that the last dose represented 30 grammes of ethereal tincture of aconite.

It was evident that I should not have obtained the slightest effect from a larger quantity of this ethereal extract; therefore, I did not think it worth while to experiment any further on the hen with the same product.

Experiment II. Ethereal Extract of Belladonna.—The ethereal extract of belladonna had no more action on the hen than the foregoing extract, although we employed the product of 8, 16, and 32 grammes of ethereal tincture. It was the same with an extract resulting from the evaporation of 30 grammes of ethereal tincture, having Hoffman's mineral liquor as a menstruum, which I thought proper to a certain extent for dissolving the active principles of the vegetable, which, however, furnished one-third less residue than the ethereal tincture prepared with pure ether.

Experiment III. Ethereal Tincture of Cicuta.—I proceeded as in the foregoing. The first dose of extract did not appear to have any influence on the hen; the second affected her a little, and the third exerted a very powerful action for some hours, to such an extent that I thought the animal was poisoned. The poisonous effect was characterised by a continual motion of deglutition, which denoted considerable constriction of the throat; by immobility of the whole body, fixing of the eyes, and other symptoms of minor importance. However, having gradually nearly recovered its normal state a few hours after having taken the poison, the hen began to eat some grains of barley, and in the evening it ate as usual, without evincing the slightest indisposition.

From the above, I am induced to believe that, by doubling the dose (60 grammes reduced to the state of extract), I should have determined formidable symptoms, and perhaps death; but I own that I omitted this cruel experiment, partly from a feeling of pity, partly from a consideration of the number of experiments which I had to perform, and partly, also, because this experiment seemed to be sufficient in favor of the ethereal tincture of cicuta.

Experiment IV. Ethereal Tincture of Digitalis Purpurea.—I repeated in this case what I have said with respect to the ethereal extracts of aconite and belladonna, without any effect, in large or small dose, on the hen which had been so powerfully effected by the ethereal extract of cicuta.

Experiment V. Ethereal Extract of Hyosciamus.—This time, being less fearful and reserved than in the foregoing experiments, I at once administered to the hen the extract obtained from 30 grammes of ethereal tincture of hyosciamus niger, without making any experiments with milder doses. I did not remark any perceptible physiological action, the hen having had a good appetite for the food which was given to her an hour after taking the so-called poison.

Experiment VI. Ethereal Extract of Aconite.—An adult rabbit swallowed in the space of 24 hours, first, the product of 30, and afterwards that of 60 grammes of ethereal extract of aconitum napellus, without suffering anything abnormal, at least, so far as could be perceived, for it was able to eat two hours after taking each dose—that is to say, when the extractive mass, which had been mixed with a little wheaten flour and water, had passed the primæ viæ. After that time the animal presented all the signs indicative of a good state of health.

Experiment VII. Ethereal Extract of Belladonna.—The same doses and the same remarks as above. One hour had scarcely elapsed after the second dose when the animal ate tolerably well. A short time after, no sign of indisposition existed; indeed, nothing worth mentioning had been remarked before, so trifling was the action of the extract. In the first experiment there was no action at all, and in the second scarcely any.

Experiment VIII. Ethereal Extract of Cicuta.—The residue of 30 grammes of ethereal tincture of cicuta having had little or no effect on a rabbit, the dose was doubled next day. The latter dose had only a feeble influence on the animal, which, however, did not take any food for some hours after, and which, during this interval, remained in a very evident kind of torpor. As soon as it began to eat, its ordinary habits returned, and at night nothing evinced the slightest indisposition.

24 hours after, another adult and vigorous rabbit was subjected to the influence of 125 grammes of the same ethereal tincture, reduced to the consistence of soft extract, namely, to 10 grammes of residue. This time the effect was very manifest during the whole day. There was evident torpor, somnolence, perceptible indisposition, inappetence, and secretion, during the night; of very fetid sanguinolent urine, with a slight smell of cicuta. Next day, a restoration of health and complete cure.

Experiment IX. Ethereal Extract of Digitalis.—The foregoing experiments having rendered me much bolder than I was at first, I had recourse, in this instance, to large doses of ethereal extract; now, I first employed 8 grammes of this matter, representing 64 grammes of ethereal tincture. My anticipations were confirmed: the rabbit was not at all affected; it ate as usual, without evincing any kind of indisposition. It was the same next day, although 16 grammes of the same extract were given to it in one dose; for it must be borne in mind that these 16 grammes of extract were the product of 125 grammes of ethereal tincture, and that this tincture is regarded by many practitioners as one of the most active of those now employed.

I should lie to my own conscience were I to consider that this ethereal extract of digitalis purpurea has no action, for it is true that the urinary secretion became much more abundant, without being excessive, as larger doses of this agent were given; but it must also be stated that this is the only remarkable effect that I can mention. The alvine evacuations were not more frequent than usual; no sign of indisposition or irritation occurred in the stomach, which supported the matter without any antiperistaltic motion, or any spasm capable of determining the slightest vomiting. It is worthy of remark, on the contrary, that the digestive functions were performed without any unusual trouble, for the animals submitted to these kind of experiments were able to eat one hour as well as several hours after taking the ethereal extract, which has not been the only one, moreover, to increase the flow of urine.

Experiment X. Ethereal Extract of Hyosciamus Niger, &c.—The more I multiply my experiments on the ethereal tinctures the more do I consider superfluous the new facts which come in support of the first. Thus I think it would be idle and useless, to my readers as well as to myself, were I to furnish a greater number of experiments to convince every one of the innocuity of the ethereal tinctures, on which we ought to be able to place most dependence. Indeed, let any one try, as I have done, and in very large doses, the ethereal tincture of hyosciamus niger on a rabbit, and let the ethereal tinctures of tobacco, strammonium, and others of the same nature be tried in the same way, and a negative result will always be obtained; the ethereal tincture alone should be excepted, since it has shown itself, in two cases, endowed with a certain energy, without, however, answering very satisfactorily to what should be expected from it; for it is evident that it can produce any effect only in doses which it is not permitted to employ in any case. Would it be possible to use it thus, even for external use, there would be little or nothing to be hoped for from this medicament.

What conclusion is to be drawn from the foregoing? That the ethereal tinctures in general, with the exception of those to which I shall have occasion to return, are bad medicinal agents, which can inspire no confidence; and that, if they sometimes produce a salutary effect, all the merit must be attributed to the sulphuric ether alone, and not to the principles which it holds in solution.

These considerations, which certainly do not require comment, will be fully sanctioned by new proofs, still more convincing than those I have just furnished.

It will readily be comprehended that it is now necessary to enter upon the question of the alcoholic extracts of which I have spoken, and less is not expected from me than a long series of physiological and toxicological experiments on these products, to form a definitive judgment concerning the question.

(*To be continued.*)

DEATH FROM POISONING OR DISEASE.*

[The following case has been communicated to us from a quarter upon which we can rely. For obvious reasons we omit all reference to names.—Ed.]

I was sent for to see a patient, A. B., who had been amaurotic, and in some degree paralytic, for about seven years. He was suffering from general derangement of the system, without any marked symptoms, and expressed himself as suffering *no pain* either in head, stomach, or bowels. I prescribed for him an aperient and some diffusive stimuli. He continued in much the same state for about two days; and on the fourth day was attacked with vomiting of a brownish matter, but unattended with pain. The abdomen was much distended with flatus, and quite tympanitic. On the evening of this day, while sitting on the close-stool, he was seized with faintness to such an extent, as to induce those about him to suppose he was sinking; but after a short time he rallied. The next day he appeared worse; but still there was no marked symptom of disease, and in the evening he complained of a frequent desire to make water, but he was not able to pass much. He was now seen by a medical friend, who found him in such a hopeless state, that he thought any interference useless. The next morning, between nine and ten o'clock, he expressed a frequent desire to micturate; he got out of bed, but soon fainted, after which a good deal of urine passed away involuntarily; and in a short time he died.

On a *post-mortem* examination all the organs of the body, except the stomach, liver, kidneys, and bladder, presented a healthy appearance. There was marked inflammation of the stomach, which was quite contracted, but free from any signs of ulceration or corrosion. It contained about two drachms of a turbid liquid, possessing no peculiar odor. The vessels of the stomach were much injected, and the lining membrane presented patches of inflammation. The same appearance of redness was met with for about four inches in the upper part of the duodenum; but this almost entirely disappeared after the third day. The intestines contained no solid nor liquid matter, but were distended with flatus. The liver and kidneys were congested, and the bladder contained about two pints of urine. There was no sign of inflammation in the organ, nor was there any appearance of peritonitis.

I am not at all aware of any of the symptoms having been increased after any meal or administration of food, nor can I in any way account for the gastritis, of which, during life, the deceased had no symptom except slight vomiting. I am fully aware that the accompanying history is a very imperfect one.

A coroner's inquest was held in this case, and a verdict returned of "Died from inflammation of the stomach;" but the cause of the inflammation could not be assigned.

After the inquest it was considered advisable to examine the stomach and the liquid taken from it. The liquid was of a brownish color, slightly acid, and partially decomposed. It contained a turbid brownish sediment; and the whole amounted to two drachms. The liquid and the sediment were separately examined for arsenic, and the more common irritant poisons, but not a trace of any of these bodies could be detected. The mucus of the stomach was then examined, but with a like result. The whole of the organ was then cut into small pieces, and boiled in a mixture of one part of pure muriatic acid, and ten parts of water, for two hours. The stomach was then in this way reduced to fine shreds; the liquid thus obtained was of a dark color, highly acid, clear, and free from viscidity. It amounted in quantity to about four ounces. A piece of bright copper was introduced into the liquid, when it had been brought to the boiling point, and allowed to remain for half an hour. For some minutes the copper underwent no change; but in less than a quarter of an hour the whole surface of the metal acquired a dark-gray tarnish, very closely resembling that produced by arsenic under the same circumstances. The copper was dried, and the gray powder scraped from the surface and heated in a reduction tube; but no white crystalline sublimate could be obtained. It should be observed that the greater part of the powder scraped off was copper, the coating amounting to only a very thin film. The result was, therefore, by no means conclusive; and as neither the liver, spleen, nor any part of the body of the deceased could be procured, in order to confirm or remove the suspicion excited by this deposit on the metal, the investigation was abandoned.

The symptoms, so far as they could be observed in the case, were not those of poisoning by arsenic; and yet it is difficult to account for the well-marked inflammation of the stomach and duodenum, and the chemical effect on the copper; for the whole of the materials were pure, a parallel experiment having been performed with these materials on double the weight of healthy animal matter. After boiling, and half-an

* *Medical Gazette*, June 20, 1845.

hour's immersion, the piece of copper employed came out in this case as bright as when it was placed in the liquid.

ON GLUCOSURIA OR DIABETES MILLITA.*

BY M. BOUCHARDAT.

I. By determining by weight the quantity of every aliment taken by the patients in 24 hours, and by measuring the quantity of urine passed during the same period, I established, in my first work on diabetes, the relation between the proportion of feculent matters swallowed by diabetic patients, and the diabetic sugar (glucosi) contained in their urine. I give, in this memoir, numerous examples, which completely confirm this discovery.

II. Some persons have formed a very imperfect idea of my works on glucosuria. Some physicians have told me that my method of treatment consists principally in the indication of animal diet exclusively. Nothing can be less correct, as I shall show in this paper. The point on which I have especially insisted, and which, indeed, should first fix attention, is the urgent necessity for patients laboring under diabetes, to suppress, or at any rate to greatly diminish, the amount of feculent matter swallowed; but (and which is not of less practical importance) it is also necessary to substitute, for the injurious feculent aliments, other aliments of the same physiological order.

The feculents and sugars belong to that group of substances now called aliments of respiration; it is therefore indispensable to select in this group the aliments to replace the feculents which the glucosuria cannot turn to account. Alcoholic drinks and fatty bodies are the substances which I have adopted, and on the employment of which I have always insisted.

III. In my first memoir on diabetes, presented to the Academy of Sciences on the 12th of March, 1839, I pointed out the existence of disease in the stomach of persons affected with this disease. I describe, in the memoir which I now present, the means I employed to obtain it in a state of purity. The process I have adopted is exactly based on that given by M. Payen, for extracting the diastase from germinated malted barley. I thus obtain a substance which does not differ in any respect from the diastase of barley. The solvent properties of these matters, obtained from such different sources, are precisely the same. Like ordinary diastase, that which is procured from the stomach of a diabetic patient loses all its action when exposed in solution to a temperature of 212° F.; its solvent property is likewise *removed* by the substances which deprive the diastase of barley of its solvent properties, and whose action I have described in my memoir on glucosic fermentation. The diastase of diabetic patients is composed of oxygen, carbon and hydrogen. I have not found a single property in this substance which does not correspond with the substance extracted by M. Payen from *germinated* barley: I therefore regard them as identical.

It remains for me to make known the precautions by means of which I extracted diastase in the pure state. In several cases I could not obtain it so active as that of *germinated* barley, because I had been able myself to collect the vomited matters, and to isolate them in the proper manner. On the 11th of May, 1844, a man named Pierre Debout, powerfully affected with glucosuria, was admitted into the Saint Magdalen's Ward of the Hôtel Dieu: two days after his admission, M. Honoré prescribed for him powdered ipecacuanha as an emetic. This emetic was taken in the morning before breakfast; a few minutes after, the patient drank two glassesful of warm water, and he soon after vomited an equal quantity of limpid liquid, which was immediately filtered, and the product of the filtration was received into rectified alcohol.

The diastase which was extracted from the first vomitings was perfectly pure; that procured from the subsequent vomitings was infinitely less active. At the end of six months, this patient left the hospital in a very satisfactory state; but, not being able to take proper care of himself, he soon relapsed, and was re-admitted into the hospital on the 19th of November. A few days after his admission, an emetic was administered to him, and I extracted very pure diastase from his first vomitings.

Does there exist in the gastric juice, secreted in the stomach of man or animals in health, a substance acting the part of diastase? I have investigated with much care the action of normal gastric juice and of the first vomitings of healthy man, fasting, on intact fecula, on jelly of starch and on bread, and I have not been able to discover the slightest specific solvent action. All that I have observed in this respect is perfectly in accordance with the experiments given in M. Blondlot's work, and in our memoir on the digestion of feculents. Thus I regard the existence of diastase in the gastric juice as a pathological, and not as a physiological, fact.

* *Comptes Rendus*, No. 14, April 7, 1845.

IV. The most important question relative to the blood of diabetic patients is that which leads to the existence of glucosi (diabetic sugar) in this blood. I think I have given, in my first memoir, experiments and reasonings, which settle this controverted question. The following observation and facts I consider useful to be made known :—

There is a very important reason, which has led into error experimentalists who have sought for glucosi in the blood of diabetic patients, and have not found it. Most frequently the physicians who send the blood to chemists for analysis wait 24 hours to allow the clot to form ; during this time, the glucosi existing in the blood may be converted into lactic acid. To verify this conjecture, I divided the blood of a diabetic patient into two parts : the first half, analysed immediately, gave traces of glucosi ; the second half did not give the slightest indication of its presence after 24 hours.

The occasions on which it is necessary to bleed diabetic patients are very rare, in my opinion ; however, this disease sometimes occurs in plethoric persons who are tormented by sanguine congestions on the side of the encephalon ; bleeding may then be indicated. The following is the way in which I now proceed to detect the glucose :—

I receive the blood, on leaving the vein, into a graduated flask containing four times as much rectified alcohol as I collect of blood. When the solid matters of the blood are precipitated, I decant the supernatant liquid, express the deposit and filter ; and after having removed the alcohol by distillation, I complete the evaporation of the liquid in a sand-bath. I redissolve the residue in distilled water, filter and evaporate immediately to a syrupy consistence. Notwithstanding all the precautions I have taken, I have never been able to extract crystallised glucose from the blood. In the aqueous liquid decolored by animal charcoal, I endeavored to find, with the aid of Biot's apparatus, the rotatory character, but have never been able to do so ; but Frommberg's reagent, and the employment of beer yeast furnished me clear and decisive proofs of the presence of glucose.

V. I have compared the alkalinity of the blood of persons affected with diabetes, and that of healthy persons, and I have found no difference.

VI. Patients affected with glucosuria sometimes die slowly, and they are often struck with sudden death. In these two conditions I have not found glucose in either the digestive apparatus, the blood, or the urine ; I relate in my memoir two very remarkable cases relative to this fact. Two persons were affected with diabetes in a high degree ; the urine of one contained as much as 135 gr. 71 of glucose the evening before death ; and although he had been carried off by a disease of eight hours, the urine in his bladder at the time of death did not contain a trace of it ; his stomach, however, was full of feculent aliments, the digestion of which had been suddenly interrupted. The secretion of diastase, in the stomach of the patient, is interrupted as soon as a serious incident malady supervenes.

VII. Do diabetic patients digest feculents like persons in health, as some have pretended ? or do fundamental differences exist, as I have stated ? The thirst of diabetic patients is in the direct ratio of the feculent aliments which they consume ; nothing similar is observed in persons in health : the phenomenon of thirst in diabetes is, therefore, connected with the digestion of feculents. The quantity of water necessary for a patient to enable him to digest the fecula is precisely equal to that which must be added to diastase in order to convert fecula into glucose. If a healthy man fasting be made to vomit, the water of the first vomitings, mixed with bread, exerts only a very feeble solvent action ; the matters vomited by diabetic patients, on the contrary, possess a very remarkable solvent action, and glucose may easily be detected in these solutions. If the matters vomited by a healthy man, who, two or three hours previous, has taken a feculent meal, be collected, only very small quantities of glucose will be detected. If, on the contrary, a diabetic patient be made to vomit, two hours after a feculent meal, the presence of a very considerable quantity of glucose will readily be demonstrated in these vomited matters. From these facts, I conclude that diabetic patients digest feculent matters differently from persons in health. I might also add that in the healthy state, man does not digest crude, or only very badly, and that I have observed two diabetic patients in whom grains of fecula were as easily attacked as in granivorous animals.

VIII. Direct experiments on animals give fresh confirmation to the facts I have communicated. The limits in which these experiments have succeeded are, it must be owned, extremely narrow ; but it will readily be understood that it is very difficult to institute experiments in which the conditions which exist in a patient suffering under diabetes shall be accurately and constantly fulfilled.

In the seven experiments detailed in my memoirs, I made four dogs and three rabbits

swallow an abundant feculent meal, after having mixed diastase with the feculent matters, and in five of the seven experiments, the presence of glucose was proved in the urine; and I should add, in order to give to these facts their full value, that I have several times analysed the urines of these same animals, after simple feculent meals, and have never found the least trace of glucose.

IX. Hygienic means, in my opinion, *govern* the treatment of diabetes: those which are of the greatest importance relate to diet, clothing, and exercise.

The first rule to be observed in the dietary of a person affected with diabetes is the suppression, or, at least, a considerable diminution in the quantity of feculent food. This suppression or diminution forms the basis of the treatment. The aliments which may be permitted are very numerous. It is not necessary to restrict the patient to an exclusively animal diet; it is far preferable to vary the regimen as much as possible, in order not to cause disgust and anorexia. The deprivation of bread and feculent aliments is strongly felt by diabetic patients; and, if some means were not adopted to deceive this desire, very few would resist this incessant temptation. For more than three years that I have employed gluten bread, its utility has never failed; it is an adjutant which has been of the greatest value to me in a great number of cases. Some persons have wished to prove that gluten bread is an exclusive remedy; such has never been my idea. I have only sought for an aliment to substitute bread without its inconvenience to the patients, and I think that I have attained this object.

Wine constitutes a considerable feature in the treatment of diabetes, and I am firmly convinced that I have rendered this class of patients, perhaps, as great a service in replacing the feculent aliments by alcoholic drinks, as in demonstrating that abstinence from feculents is indispensable for them.

X. Medicines act a secondary part in the treatment of diabetes. I have been able, as I explain in the particular observations, fortunately to cure several patients who have had recourse only to hygienic means; however, I have often found that carbonate of ammonia, with a tonic and stimulant preparation of opium, powerfully contributed, in rebellious cases, to restoring the urine to the normal state.

XI. In my memoir on glucosic fermentation, I have studied in detail the influence of different agents on the action of diastase on starch. These investigations were undertaken with the view of elucidating the treatment of diabetes; indeed, if what I have observed concerning the nature of this disease be correct, by employing in a proper manner the agents which are opposed to the formation of glucose, we may hope to prevent this formation in the stomach. The solution of this problem would be of especial importance to patients who, obliged to work for their living, cannot, with their exhausted strength, earn sufficient to provide for the extraordinary and daily expenses contingent on the hygienic treatment, which alone is constantly efficacious. What are the substances which are opposed to glucosic fermentation, and which may with impunity be introduced into the stomach in moderate proportion when suitably diluted with water? If my memoir be consulted, we shall find caustic potassa and soda, lime and magnesia, the powerful acids, such as sulphuric, nitric, hydrochloric, and oxalic, and alum. If the authors who have written on this subject be consulted, it will be seen that all the substances which are proper for arresting the glucosic fermentation, and which can be fearlessly introduced into the stomach, have been extolled by high authorities, and several observers often agree as to their efficacy. These facts perfectly accord with the opinions I have given, and still I ought to say that I have repeated all these trials with the greatest perseverance: attentive observation has shown me that none of these agents has any absolute utility. In the most fortunate cases, I have observed only a simple diminution of the symptoms; I should add that these trials were always directed against rebellious cases. These negative results may easily be comprehended; indeed, when the alkalies or alkaline earths are employed, the acids constantly secreted in the stomach often neutralise them, and their effects are annihilated. When the preference is given to the strong acids, we are constrained to prescribe them in such a state of dilution that their retarding influence is much less powerful; and since the liquids in the stomach dilute them still further, their action is then very limited. Be it as it may, if difficulties be found in the application, as the principle is correct, good results must be hoped for in following this course.

ON THE EXTERNAL USE OF TOBACCO IN PRURIGO.*

BY J. G. FRENCH, ESQ.,

THE infusion of tobacco (which has, I believe, been hitherto used only as an enema,

* From the *Medical Gazette*, June 20, 1845.

and which, in that mode, produces such powerful physiological effects) is a most useful, and, as far as my experience goes, a most innocent external application.

The complaints for which I have prescribed it with undoubted success are prurigo, and the purulent opthalmia of infants. I have also found greater benefit from it as a collyrium in scrofulous conjunctivitis, than from any other remedy. In prurigo pudendi I first discovered its extreme usefulness in a case which had for some time been relieved by a strong lotion of hydrocyanic acid, viz., a drachm of Scheele's solution to six ounces of water; this at length losing its effect, the infusion of tobacco was prescribed, from which the patient found more complete relief than had ever been experienced from the hydrocyanic acid. Since then, two years ago, I have always used this infusion in such cases, and consider it to be a highly satisfactory remedy.

Although we have already several useful remedies for the purulent ophthalmia of infants, yet in some cases these appear to produce irritation, and the complaint under their use sometimes becomes stationary. In such cases I have found the most marked benefit from the infusion of tobacco.

The formula of this remedy for these purposes, is double the strength of that generally used as an enema, viz., one drachm of the tobacco to half a pint of boiling water; and it may be remarked that the tobacco sold under the name of "shag," was found to be more efficacious than that called "pigtail."

CASE OF POISONING IN A YOUNG CHILD, BY A STRONG DECOCTION OF POPPIES—RECOVERY.*

BY DR. PUPKE, OF GIESSEN.

A WOMAN, in order to quiet her child, about six weeks old, gave it several teaspoonfuls of a strong decoction of poppy-heads. In a quarter of an hour the child fell into a deep sleep, from which it could not be roused. About thirty-six hours after the occurrence of this somnolency, Dr. Pupke was called in, and he found the child almost moribund. The eyes were sunk, the lids half open and surrounded by a livid circle, the pupils dilated and insensible, the face pale, with a slight bluish tint, the extremities almost paralysed, respiration hurried, the pulse frequent, small, and trembling, the forehead covered with a cold sweat, and the lower jaw depending. Occasionally the child made a slight noise by protruding its tongue from the mouth. No urine had been voided, nor had any fæces been passed since the first occurrence of the symptoms. The child had no longer the power of sucking, but it could swallow without difficulty.

It was too late to think of attempting to remove the poison from the stomach. Some teaspoonfuls of coffee were given to it, with camphorated tea; and enemata of camphorated vinegar were used. In addition to these means, the child was placed as soon as possible in a warm camphorated bath. The bad symptoms gradually disappeared, and the child perfectly recovered.

[Cases of this description are unfortunately of very common occurrence, and the results are seldom so favorable. The reporter seems to have placed great confidence in the use of camphor in all its forms, giving it internally and at the same time using it in injections, and even as an external application in the bath. We are not aware that any particular benefit has been hitherto found to follow the use of camphor in such cases, and probably we may ascribe recovery in this instance as much to a vigorous reaction in the constitution as to treatment. The dose of poison, besides, must have been small, or the child would not have lived thirty-six hours after taking it, the time which elapsed before it was seen by Dr. Pupke. Cases are reported, which show that infants may be killed by two teaspoonfuls of a decoction of two or even one poppy-head in water.—Ed.]

* *Med. Zeit., and Annales d'Hygiène*, Janvier, 1845.

SIR JAMES GRAHAM'S BILL AS IT AFFECTS CHEMISTS.

To the Editors of The Chemist.

GENTLEMEN,—

PETITIONS have lately been sent here for signature by the Pharmaceutical Society, complaining of the great injustice of the clause in Sir James Graham's Bill, which imposes penalties on any unregistered person practising as an apothecary.

From a fear that Chemists and Druggists may suffer from a tortuous application of its meaning, do you not think it would be as well, to quiet the timid portion of your readers, to publish the decision of the House of Lords in what, perhaps, may be thought a similar case?

The College of Physicians had many times prosecuted the Apothecaries for visiting patients, and prescribing medicines for the relief of those persons who came to *their shops* for advice; contending that so to visit and prescribe was acting as a Physician, and

consequently infringing their charter, which entitled them alone to *practise physic?*

The House of Lords, however, thought otherwise, for when the case of the King *v.* Rose came before them in appeal, they decided that an Apothecary had a right to go out to take orders for his medicines, and to advise his customers in their use, as well as any other tradesman.

Upon that decision rests the legal right of Apothecary, Druggist, and every other vendor of medicine, to advise his customers respecting the use of the article he sells.

How, therefore, can a Druggist be exposed to the fines of the proposed clause for advising his customers in the use of his medicines any more than for dispensing them?

If, however, to prescribe a remedy be to act as an Apothecary, then are the Physicians, Surgeons, and every one who does not possess an Apothecary's certificate, as liable to prosecution as the Chemists and Druggists.

Why not, to remove all doubt upon the matter, petition for a protective clause similar to that inserted in the Act of 1815, commencing with "and nothing in this Act shall interfere with the art, trade, or calling of a Chemist and Druggist?" &c.

Should you think these remarks worth a place in your widely circulated journal, I think by such means they made be made of some use to your readers.

I remain, Gentlemen,
Yours respectfully,
HENRY DUNN.

South Molton, June 18, 1845.

There could be no objection to a Chemist and Druggist visiting and prescribing, if he were competent to do so, and if he have satisfied a body of examiners of his possessing the requisite knowledge. We have always contended that Chemists should be properly educated, and examined by persons appointed by Government. Until Chemists obtain for themselves a legal qualification, they will never be recognised by the Legislature or by the other branches of the profession. In fact, they should become Apothecaries. The idea of the diploma of the Pharmaceutical Society being sufficient would be too laughable to be entertained for a moment by any one not insane. By the way, the Pharmaceutical Society appear to be as late as they are powerless in taking their measures. We know for a fact that Government has no confidence in this Society, and will not pay any attention to it.

We have long observed silence as regards the Pharmaceutical Society, but we have been at work.

ARSENIATE OF QUININE.

LETTER FROM R. N. M'DERMOTT, ESQ.

HAVING read in the *Lancet* a letter from Mr. R. N. M'Dermott, in which he claims the preparation and use of this salt prior to M. Bourieres (in fact so far back as October, 1843), an article by whom appeared in our last number, we wrote to Mr. M'D. on the subject, and we now subjoin the reply with which we have been favored by that gentleman. We shall be very glad to learn the results of Mr. M'Dermott's experiments, and to lay them before our readers if he will kindly communicate them, at the same time regretting the causes of their temporary discontinuance.

Athboy, Ireland.

To John Watt, Esq.

Sir—Your letter did not reach me till the 20th inst., on my return to Dublin from this place.

I regret you should infer my not claiming my prior right to having prepared and used the arseniate of quinine through the medium of your valuable periodical arose from want of courtesy on my part, or confidence in the impartiality of THE CHEMIST, in whose pages I have always found much valuable information.

I had been (till illness and severe domestic affliction interfered) much interested in an inquiry into the action of arsenious and bichloride of mercury, &c., as therapeutic agents, and reason, observation, and experience, lead me to conclude that their (I may say at the present day) indiscriminate use is neither safe nor justifiable.

In persons suffering under cutaneous affections, a deranged condition of the digestive, depurative, and assimilative functions invariably coexists with that of the skin, and my experience goes to satisfy me that any improvement following the use of the usual arsenical preparations in the latter case, is more than counterbalanced by the aggravation of the former.

In the number of the *Dublin Medical Press* for March 13, 1844, I published an account of a new salt, the proto-chloride quina and mercury, which I proposed to substitute for the much-used solution of the bichloride of mercury in tincture of cinchona. Mr. Hamilton, of the Richmond Hospital, Dublin, has since found it a valuable medicine in lupus, &c.—Vide *Dublin Journal of Medical Science, &c. &c.*, also *Braithwait's Retrospect.*

I have not, from the causes before alluded to, been able fully to test the value of either salt, but when I do I shall feel much pleasure in communicating the result.

In conclusion, I shall feel much obliged by your claiming for me, in your journal, the priority of having prepared and used arseniate of quinine.

I have the honor to be, sir,
Your obedient servant,
RALPH N. M'DERMOTT,
&c., &c., &c.

REMARKS ON STRYCHNIA AS A REMEDY IN CHOREA.

BY A. ROSS, M.D., BOULOGNE-SUR-MER, LATE PHYSICIAN TO THE SUFFOLK GENERAL HOSPITAL, BURY ST. EDMUNDS.

SOME years have now elapsed since, reasoning on the supposed state of the brain and nervous system in chorea, on the frequent want of success attending the usual mode of treatment of the disease, and on the well-known effects of strychnia on the nervous system, I was induced to make trial of this remedy in a few cases which came under my care, and in which the usual mode of treatment, by aperients, tonics, &c., had been unsuccessful.

It was my intention to wait until I should have collected a greater number of cases before I communicated to the profession the result of my experiment; but I am induced to anticipate this intention, in consequence of the report, in the *Lancet* of the 17th of May, of a recent discussion on this disease, at a meeting of the London Medical Society, the result of which shows that a remedy for this frequently intractable affection is still much wanted.

The first occasion on which I made trial of the remedy in this disease was in the beginning of 1839, in the case of a delicate girl of twelve or thirteen years of age, who came under my care as an hospital patient, with many of the eccentric symptoms of this singular disease most distinctly marked. From having been very expert with her needle, she was rendered incapable of using it, and her attempts to thread it were almost ludicrous. I prescribed for her the eighth or tenth of a grain of the alkaloïd, to be taken twice a day. On the second or third day of the treatment, through a mistake of the nurse, she had an overdose of the medicine, which produced more violent effects than I intended—viz., convulsive twitches, which however, quickly subsided, on the medicine being intermitted, and with them all symptoms of the disease. In a day or two after this I saw her thread a fine needle with a band perfectly steady, and she was dismissed cured at the end of a week. I saw her more than a year afterwards; she was quite well, much improved in appearance, considerably grown, and had had no return of chorea. I ought to mention that she first came under my care after having been already some weeks in the hospital, under the care of my esteemed predecessor, Dr. Bayne, whose treatment of her was continued, without any improvement in her symptoms, until she commenced the use of the strychnia.

My next case was a girl of the same age and constitution, who came under my care as an hospital patient, about the same period. In her case I adopted the same treatment. No violent effects were produced by the remedy, and after a few days' treatment the symptoms of chorea began gradually to disappear, and she was also dismissed cured at the end of a fortnight.

I cannot speak so favorably of the effects of strychnia in some cases of paralysis in which I have considered myself warranted in trying its use, and in which class of diseases under certain circumstances, its advantages have been favorably recognised by some authors; but in chorea, in the limited number of cases in which I have given it a trial, I have been much satisfied with the result.

Should any of my professional brethren feel disposed to try the effects of the remedy in this frequently unmanageable disease, I should feel gratified if they would make known the result of their trial, as I feel that my own opportunities of trying the remedy have not been sufficiently numerous to establish its true value.

THE GRAPE CURE IN RUSSIA,*

(BY SIR G. LEFEVRE)

WOULD seem to be the original of the dictum, "Live on a shilling a day, and earn it."

Those who have practised long in Russia will have been made conversant with the *cure du raisin*. I had an opportunity of becoming so when in the south of the empire, and in a grape country. It is necessary to state in what this cure consists, and for what class of diseases it is recommended. The latter may be dismissed at once, by stating that all those functional nervous affections which resist the routine of treatment generally employed, are the cases which may be so benefitted, seeing that the discipline is more intolerable than the disorders for which it is instituted. A lady of rank leaves her bed of down and cushioned canopy, and migrates into the country,—turns a poor family out of their habitation,

* *Medico. Chir. Review.*

(not without making them an ample recompence,) and becomes the tenant of a filthy hut. This is part of the cure—viz , to forego all luxury, to sleep in the peasant's crib, to sit upon his bench, and to avoid anything in the shape of comfort. The grape alone for meat—the grape for drink ; a small quantity of dry bread is perhaps allowed. This is continued for the space of three weeks, and it is no wonder, if all circumstances are taken into consideration, that a cure is effected. I have known people of the highest rank subject themselves to such discipline, and have full faith in its results. It is homœopathy and hydropathy in another shape, and, as the Italians say of all the varieties of form in which they make their pastes, "*c'est toujours du macaroni.*"

TABLES FOR DISCOVERING THE NATURE OF SALINE DEPOSITS IN THE URINE BY CHEMICAL REAGENTS AND BY THE MICROSCOPE.*

BY DR. G. BIRD.

I. BY CHEMICAL REAGENTS.

1.	Deposit,	white	2.
	,,	colored	5.
2.	,,	dissolves by heat	Urate of ammonia.
	,,	insoluble by heat	3.
3.	,,	soluble in liquor ammoniæ	Cystine.
	,,	insoluble in	4.
4.	,,	soluble in acetic acid	Earthy phosphates.
	,,	insoluble	Oxalate of lime.
5.	,,	visibly crystalline	Uric acid.
	,,	amorphous	6.
6.	,,	readily soluble by heat	Urates.
	,,	slowly dissolves by heat	,, stained by purpurine.

II. BY THE MICROSCOPE.

1.	Deposit,	white	2.
	,,	colored	5.
2.	,,	an amorphous powder	Insoluble by heat — Phosphate of lime. Soluble by heat—Urate of ammonia.
	,,	in defined crystals	3.
3.	,,	in prismatic crystals	Triple phosphate.
	,,	in octahedral or tubular crystals	4.
4.	,,	in octahedra	Oxalate of lime.
	,,	in simple or compound tables	Cystine.
5.	,,	in transparent crystals	Uric acid.
	,,	amorphous, or in spherical masses	Urates of ammonia or soda.

IV. REVIEWS AND NOTICES OF BOOKS, &c.

THE LONDON MEDICAL GAZETTE, OR JOURNAL OF PRACTICAL SCIENCE.

THIS medical periodical has recently come out under a new form, considerably increased in size, and diminished in price, and, high as its character has ever deservedly stood, that character is now greatly improved.

The nine numbers which have appeared since the alteration, have contained lectures delivered by some of our most eminent Professors, medical and surgical, with a great number of valuable original communications from men of the highest stand in in the profession. The leading articles are ably written ; the reviews learned and impartial ; the selected articles evince good judgment. There have also appeared some valuable papers on Toxicological subjects, some of which are by Mr. Alfred S. Taylor, the able lecturer on Medical Jurisprudence at Guy's Hospital. The proceedings of the various medical societies are also given a some length.

* *Med. Chir. Review.*

Altogether, it is a journal which no member of the profession, be he chemist, general practitioner, or physician, should omit to take. We consider it one of the most carefully edited medical periodicals of the day, and as such we heartily wish it success.

THE CYCLOPÆDIA OF PRACTICAL RECEIPTS AND COLLATERAL INFORMATION IN THE ARTS, MANUFACTURES AND TRADES; including Medicine, Pharmacy, and Domestic Economy. Designed as a compendious Book of Reference for the Manufacturer, Tradesman, Amateur, and Heads of Families. By ARNOLD JAMES COOLEY. 2nd Edition. London: John Churchill. 1845. 8vo. pp. 808.

AN extremely useful compendium, meriting a place in every library. The editor, Mr. Cooley, who is well known to our readers as a contributor to THE CHEMIST, has bestowed much care on its compilation. He appears to have collected its contents from a great number of works—Foreign as well as English. It is altogether highly creditable to his industry, and we hope that this second edition will meet with the support it so well deserves.

CONSUMPTION AND ASTHMA, THE NEW CURE. By W. H. KITTOE, M.D. Second Edition. London: Sherwood.

CONSUMPTION has been termed "the English malady;" and certainly the very large proportion of deaths caused by it amongst us gives it ominous right to the title, and claims for every sound writer upon it a fair hearing. During some years past the labors of Laennec, Louis, Clark, Scudamore, and others, have amply proved the possibility of curing consumption, and of completely arresting it in its early stages; and that consumption is necessarily and unavoidably fatal, has sunk only into a popular error. The deaths from consumption, though still frightfully large, have of late decreased, and much of this is owing to the altered views of treatment entertained by enlightened practitioners in abandoning the abuse of the lancet, purgatives, and mercury, in diseases of the chest, and the excessive use of which the author of this work well and strongly denounces. The work before us, without being technical, affords a full and useful summary of the structure of the respiratory organs, and of the nature and causes of their diseases, and contains just that kind of knowledge of the subject which every parent and guardian of youth and every well-informed man ought to possess, whilst by omitting prescriptions it offers no temptations to the too common and dangerous tamperings of domestic quackery The views of the writer are sustained by cases much to the purpose, authenticated by the names and addresses of the patients. Interspersed are many remarks that will repay the reading of most professional men even; and indeed we regret that, without giving the formulæ, he has not more fully indicated the consecutive treatment of his cases, particularly those of asthma, in which he seems to have been successful far beyond most of his brethren. We could also wish that the pathology of asthma had been somewwhat more fully treated and elucidated. But the work is of a highly useful and practical kind.

GRAHAM'S ELEMENTS OF CHEMISTRY.

As a fresh proof how much chemistry is cultivated and real merit appreciated in Germany, we may be allowed to adduce the fact that, notwithstanding the numerous works on the above science, compiled by eminent writers of that country, the translation of *Graham's Elements of Chemistry* was most enthusiastically received there. Scarcely has a year passed since this book was first in the hands of the German public, and yet it is already out of print. A second edition is advertised; but to satisfy the great demand the publishers have had recourse to extraordinary means, and have printed the first and third volumes simultaneously (the German translation appeared in three volumes). Considering that the translation has been sooner sold than the original, and that the former is sold even at a higher price than the latter, we deem this case to be without a parallel. —nt—

BOOK RECEIVED.

"Gregory's Outlines of Chemistry," Part II.—A notice of this excellent work will appear in our next.

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THE CHEMIST.

I. CHEMISTRY.

OBSERVATIONS ON THE SESQUIOXIDE OF CHROMIUM AND ITS ISOMERIC MODIFICATIONS.*

BY HENRY LÖWEL.

By dissolving in water, at the ordinary temperature of the atmosphere, the crystallised double sulphate of chrome and potassa ($3\,SO^3$, $Cr^2O^3 + SO^3$, $KO + 24\,H^2O$), called chrome alum, a solution of this salt is obtained, whose color is blue, verging on purple or violet, by transmitted solar light, and purple-red when placed between the eye and the flame of a candle.

By submitting this violet-blue solution to spontaneous evaporation, without the aid of heat, the chrome alum which it contains crystallises in octohedrons.

If the violet-blue solution of chrome alum be heated beyond 150° or 160° F., and especially if it be boiled, it becomes green. The intervention of heat not only changes the color of the solution of the salt, but it also deprives it of the property of crystallising. By evaporating the green solution to a syrupy state, the salt does not crystallise by the cooling of the liquor; the latter is ultimately dried into an amorphous residue of a deep green.

If, on the contrary, the solution of chrome alum, turned green by a few minutes' ebullition, be abandoned to itself, in a corked flask, it gradually becomes more and more blue, and, after about two or three months, it resumes the violet-blue color which it had before being heated; then, by submitting it to spontaneous evaporation, the salt which it contains is completely crystallised in octohedrons.

To explain these phenomena, Berzelius and most chemists think that, by the effect of heat on the saline solution, the oxide of chromium which it contains undergoes a change in the grouping of the atoms which constitute its molecule; they admit that, in these two solutions of chrome alum, one of which is green and the other violet-blue, the oxide of chrome exists in the state of two different isomeric modifications, which Berzelius has designated under the names of *green modification* and *purple-red modification*. (The latter, on account of the color which the solution presents, when viewed by candle-light, and which should rather be called the *violet-blue modification*). To my knowledge, no work has yet been published with a view of elucidating this part of the chemical history of chromium, and of proving whether there really exist different modifications of its sesquioxide, in the combinations which it forms with acids. I think that the existence of these two modifications has been admitted, only because all the soluble salts of chrome, hitherto described, give solutions which present either the green or the purple or violet-blue color. Moreover, Berzelius's almost universally admitted explanation has even been disputed. M. Schroetter, in a memoir on the sulphates of chrome,* expresses his opinion that the change of color from blue to green, which occurs on heating a solution of sulphate of chrome, arises simply from a loss of water which the salt undergoes, and not from a modification of the oxide. Berzelius, in speaking of this work, in his *Annual Report* of March 31, 1842, rightly observes that the experiment on which Schroetter founds his opinion, and of which I shall speak further on, is by no means conclusive.

In a work on chromium, with which I have long been occupied, I have carefully studied the properties of the salts formed by the sesquioxide of this metal. Not having yet been able to complete this work and publish it entire, I will extract from it the results of a series of experiments, which seem to me to throw some light on this still controverted question. From the facts which I have observed, and which I have the honor of submitting to the Academy, I think I may conclude that, not only is the sesquioxide of chromium found in the state of two different isomeric modifications in the salts whose solutions are green, and in those

* *Journal de Pharmacie*, May, 1845.

* *Poggendorf's Annalen*, vol. liii.

whose solutions are violet-blue, but that it also presents in its saline combinations other modifications, which have not been noticed and described by chemists.

DISCUSSION OF M. SCHROETTER'S OPINION.

I. In his work on the sulphates of chrome, M. Schroetter describes a blue sulphate which he obtains by dissolving 8 parts of hydrate of chrome in 9 parts of sulphuric acid, abandoning the green liquor obtained to itself, until it has become blue, and then precipitating it by alcohol.

Further on, I will describe a process for preparing this blue sulphate in a very short time. It is this salt which, in combining with sulphate of potassa or sulphate of ammonia, forms chrome alum.

This simple sulphate, thus prepared, is in small crystalline bractese, and of a light, silvery, ashy lilac. According to Schroetter's analysis, the composition of this salt is expressed by the formula $3SO^3, Cr^2O^3 + 15HO$.

It is very soluble in water. It cannot be crystallised, like chrome alum, by the spontaneous evaporation of its solution. By exposing in a small capsule a solution of a few grammes of this salt in a little cold water, under a bell-glass, by the side of another capsule containing concentrated sulphuric acid, the solution, on concentrating by spontaneous evaporation, becomes covered after a short time with a pellicle, and then the liquor remains for a long time in the syrupy state under this saline pellicle. If this pellicle be removed, a new one is formed, and by continuing to draw to the edges of the capsule the pellicles which are successively formed, the salt is finally obtained in hardened masses, formed by the pellicles successively removed, but there is no proper crystallisation.

The solution of this salt, made without heat, is of a blue color, verging on purple or violet, like that of chrome alum, and, like it, it becomes green, when heated to 150° or 160° F. The following is what Berzelius says, in his *Annual Report* already mentioned, relative to this change of color:—"Schroetter does not regard these changes as isomeric modifications, but simply as combinations at different degrees of hydratation; he regards the following circumstance as a proof of this:—If, in a tube closed at one end, alcohol be poured on the solution of the blue salt, and if the liquids be left thus in contact, without being mixed, the liquid gradually becomes green, beginning from the top, and, finally, the alcohol may be mixed with the saline solution, without any precipitate being produced. It cannot be conceived why the alcohol removes water from the salt, because it can find abundance in the solution; nor why the alcohol with which the precipitated blue salt is washed does not immediately convert it into green salt, if the conversion depends on the affinity of alcohol for water."

I have repeated M. Schroetter's experiment several times, and I have found—

1. That, by pouring alcohol into a tube, on a dilute solution of simple blue sulphate, without mixing the two liquids, and leaving the tube for several weeks, the blue liquor first became lighter at the top, then gradually passed entirely to green, beginning at the top, as M. Schroetter says.

2. If, instead of simple blue sulphate, a blue solution of chrome alum be used, and if it be treated in exactly the same manner with alcohol, the blue liquor first turns light, and is then gradually decolored, beginning at the top; some crystals of chrome alum are, at the same time, formed at the bottom of the tube. These crystals increase in volume in proportion as the decoloration of the liquid proceeds, and they are finally formed at the bottom of the liquid, which is perfectly colorless, and holds no salt in solution.

It is evident that, in this experiment, the chrome alum, which has the property of crystallising easily when it is in the blue state, is totally expelled from its solution by alcohol, without passing to the green state.

3. If a very considerable quantity of alcohol be added to a blue solution of simple sulphate or of chrome alum, and, if the liquors be well mixed by stirring, the salts are entirely precipitated in bractese or in a crystalline powder of a light ashy lilac color. These salts, thus precipitated and washed with alcohol, are always in the blue state, and they remain so, even if kept a long time in absolute alcohol.

4. If a great quantity of alcohol be added in the same way, to the same solutions of simple sulphate, or of chrome alum, first rendered green by a few minutes' ebullition, and cooled, and if the liquors are stirred in order to mix them thoroughly, the salts are also precipitated, but most in the crystalline state; in each the precipitate forms a thick matter, which ultimately hardens in the alcohol into an amorphous brittle mass, of a deep green.

5. In making a solution of blue simple sulphate, in cold water, dividing it into two equal parts, rendering one of them green by boiling, then allowing it to cool, and adding equal quantities of alcohol by small portions at a time, to both these solutions, and stirring the mixtures, no precipitate is at first formed in either liquor. By continuing thus

to add alcohol to the two solutions, and stirring well with glass rods, the blue solution soon begins to become turbid, and almost all the blue salt is precipitated, whilst, with the same quantity of alcohol added to the green solution, no precipitate is formed.

It results from this experiment that the salt in the green state is much more soluble in dilute alcohol than that in the blue state.

With these data, it appears to me that that which occurs in Schroetter's experiment may be explained in the following manner, admitting the modification of the oxide:—Alcohol attracts water from the saline solution on which it is superposed, but, at the same time, the latter also attracts the alcohol, is gradually impregnated with it, and passes into the state of solution of blue salt in dilute alcohol. The absorption of alcohol continuing, a period arrives at which the salt can no longer remain in solution, and there is a tendency in the liquor to expel it. Now, we have seen above that the blue simple sulphate has no disposition to crystallise; it appears, therefore, that in the medium of this tranquil liquor, the saline molecules instead of combining and grouping to form crystals, are modified, and gradually pass into the green modification, which permits them to remain still in solution, since the sulphate, under this modification, as we have observed above (experiment 5), is more soluble in dilute alcohol, than it is under the violet-blue modification.

With respect to M. Schroetter's remark, that alcohol may be mixed with the green saline solution, without any precipitate being produced, this effect depends entirely on the relative qualities of saline solution and alcohol; for, when the latter is in sufficient quantity, a deep green thick precipitate is formed.

Experiment 2, made with a solution of chrome alum seems to confirm this explanation of the phenomenon, for, the blue double salt, crystallising with the greatest facility, is gradually expelled from its solution, by the absorption of alcohol, without passing to the green modification.

Does the change of color really arise from a modification of the oxide of chromium? The following experiments appear to me to prove it:—

1. If from 60 to 75 grammes of concentrated liquid ammonia be added to 100 grammes of a saturated solution of chrome alum in cold water (containing about 12 or 13 grammes of crystallised salt), and if the flask in which the mixture is made be briskly shaken, the greenish-gray hydrate precipitated takes a rose tint, deep in color and is gradually redissolved. By shaking the flask from time to time, the precipitate is almost entirely dissolved at the end of five or six hours. By filtering an ammoniacal liquor, of a deep amaranthe red is then obtained, and there remains on the filter only a small quantity of hydrated oxide of chromium not redissolved, amounting to scarcely one-tenth, of that contained in the salt; therefore, more than nine-tenths are redissolved.

2. If from 60 to 75 grammes of ammonia be added in the same way to 100 grammes of the same solution of chrome alum, rendered green by a few minutes' ebullition, then cooled, the light azure gray precipitate which is formed is only feebly redissolved; if it be filtered at the end of six hours, or even after 24 hours' reaction, an ammoniacal liquor is obtained, also colored of an amaranthe red, but much less deep than that of the foregoing experiment; and the greenish-gray azure hydrate, which is not redissolved, and which remains on the filters, amounts to nearly two-thirds of that contained in the salt, only about one-third of the latter being dissolved. It is scarcely redissolved in greater quantity, if the quantity of ammonia be increased or even doubled.

The same results were obtained by operating with two solutions, each of 10 or 12 grammes of blue simple sulphate in 90 grammes of cold water, one of which was rendered green by ebullition, by treating them with the same quantities of ammonia.

It is evident, from these two experiments, that hydrate of chrome, precipitated from the green simple or double sulphate, is redissolved in much smaller quantity in the same excess of ammonia than the hydrate precipitated from the same blue sulphates.

3. By dissolving four grammes of crystallised chrome alum in 40 grammes of cold water, adding to this liquor a solution of five grammes of chloride of barium in 20 grammes of water (a quantity sufficient for precipitating all the sulphuric acid, and for leaving a considerable excess of chloride of barium in the liquor), and throwing immediately on a filter, after having well stirred the liquor with a glass rod, sulphate of chrome remains on the filter: the filtered liquor is limpid, and has a violet-blue color. If it be heated to ebullition, it becomes green, but does not become preceptibly turbid, and does not precipitate sulphate of baryta.

4. By rendering a similar solution of four grammes of chrome alum in 40 grammes of water, green, by a few minutes' ebullition, and precipitating this solution, when cold, by five grammes of chloride of barium dissolved in 20 grammes of water and filtering immediately, sulphate of baryta remains on the filter; the liquid which passes through

is limpid and green at first, but, after a few minutes, that which has passed through becomes foul and turbid, and that which is still filtering also passes through turbid. When all the liquor has passed through, if we try to filter it again, it also passes turbid through the filter. If it be boiled for a few minutes, the quantity of the precipitate increases, and on being filtered, the green liquid then passes through limpid, does not become turbid, and a very great quantity of sulphate of baryta remains on the filter. If immediately after having precipitated and filtered the liquor, the first portion which has passed limpid through the filter be heated to ebullition, it becomes turbid in the same manner and gives a precipitate of sulphate of baryta.

Two solutions, each of three or four grammes of blue simple sulphate in 30 or 40 grammes of cold water, one of which was first rendered green by an ebullition of a few minutes, precipitated in the same way, each by a solution of five grammes of chloride of barium in 20 grammes of water, acted in the same manner as the blue and green chrome alums above mentioned.

It is evident from these experiments that, by precipitating sulphate of chrome in the green state by the salt of baryta, even added in considerable excess, all the sulphuric acid is not immediately precipitated; a portion of this acid still remains in the liquor, and forms, with baryta and oxide of chromium, a soluble combination, but which has little stability; this effect does not occur with the same sulphate in the violet-blue state, when it is pure and very free from green salt. It results from all these experiments that the solutions of the sulphates of chrome ($3SO^3$, Cr^2O^3) in the violet-blue and green states, differ, not only in their physical properties, but that they also present considerable differences in their chemical reactions; the oxide precipitated from the one, acts in a different manner with an excess of ammonia, from the oxide precipitated from the others, all other things being equal. These differences can be attributed, it appears to me, only to an isomeric modification which the oxide undergoes, to a change which is operated in its molecular state, when the violet-blue solutions of the salts which it forms become green by heat, and if, really, as Schroetter says, the sulphate in the blue state is more hydrated and loses water in passing to the state of green sulphate, this loss of water cannot be regarded as the *cause* of the phenomenon, but indeed as an *effect*, or an indication of the change which is operated in the oxide contained in the salt.

II. Chromate of potassa being the salt generally employed by chemists for procuring not only chrome alum, but all the other salts of chrome, I will now describe some of the experiments which I have made with this salt, and whose results relate, more particularly, to the subject of which I treat in this memoir. When a certain quantity of alcohol and of an inorganic acid, either sulphuric, hydrochloric or nitric, is added to a solution of bichromate in water, there is a reaction; the alcohol removes from the chromic acid half of its oxygen, and reduces it to the state of sesquioxide of chromium, Cr^2O^3, which combines with the acid as well as the potassa of the bichromate. In order that all the chromic acid may disappear, it is necessary, besides a sufficient quantity of alcohol, to add at least four equivalents of acid to the solution of one equivalent of bichromate of potassa. After the reaction, one equivalent is found combined with the potassa, and the other three equivalents with the oxide of chromium.

If the acid and alcohol be made to act on bichromate of potassa in coarsely-powdered crystals, with less water than is required for dissolving it, much heat is developed by the reaction; in this case, if the heat developed exceeds 150° or 160°F., the liquors obtained with sulphuric acid and hydrochloric acid, that obtained with nitric acid is of a purple or violet-blue after cooling, whatever may have been the degree of heat during the action. If, on the contrary, we operate with solutions of bichromate sufficiently diluted in water, the reaction occurs more slowly, the liquors are little heated, and we then obtain with sulphuric and with hydrochloric acid, violet-blue solutions. I will now indicate the proportions which I employed for obtaining these blue solutions with each of the three acids.

WITH SULPHURIC ACID.

III. Fifty grammes of bichromate of potassa were dissolved in 850 grammes of water; 80 grammes of concentrated sulphuric acid were added to it, and, when the mixture was cool, from 20 to 25 parts of alcohol. The bottle was shaken in order to mix them. The reaction commenced immediately; the liquor was heated to about 110°to 120°F.; there was no disengagement of gas. At the end of a few hours, the fine green liquor was cooled; next day, on viewing it by transmission in thin layers, it appeared of a fine blue, reflecting purple and green; by the light of a candle, the transmitted light was purple-red. This liquor had a peculiar ethereal odor. By submitting it to spontaneous evaporation at the ordinary temperature, it crystallised en-

tirely in octohedrons; the bichromate of potassa had been totally converted into chrome alum by the reaction.

Besides this salt, and a small excess of sulphuric acid, there was in the blue liquor a certain quantity of acetic acid, &c., products of the oxidation, which were volatilised by spontaneous evaporation.*

Crystallised chrome alum dissolves in about seven times its weight of water at the ordinary temperature; the solution is of a purple or violet-blue, and it becomes green by heat, as we have said above. In almost all chemical works it is said that, in becoming green by heat, the double salt is decomposed, and the two sulphates separated: this assertion is not correct. By dissolving with the aid of heat 50 grammes of this crystallised salt in only 40 grammes of water, and boiling the solution for 15 or 20 minutes, so as to reduce it to the weight of 60 grammes, it assumes the syrupy state, containing 50 parts of crystallised salt, dissolved in only 10 parts of water; by cooling, from one day to the next, it does not crystallise. At the end of four or five days some crystals are found in it; by disengaging them from their green syrupy mother-liquor, washing them with a little cold water, and pressing them between folds of blotting-paper, these crystals are found to be only chrome alum in octohedrons. I have repeated this experiment several times, and I have never obtained the smallest quantity of sulphate of potassa. I think that Fisher, who announced the decomposition of the double salt by ebullition, must have operated with a solution of this salt containing much free sulphuric acid; for I have found that, by adding to the above indicated quantities of salt and water 20 grammes of sulphuric acid, and boiling the whole for 20 or 25 minutes, the green syrupy liquor gives next day a very abundant crystallisation of acid sulphate of potassa.

WITH HYDROCHLORIC ACID.

IV. Fifty grammes of bichromate of potassa are dissolved in 780 grammes of warm water; when the solution is cooled, 150 grammes of concentrated fuming hydrochloric acid and 20 grammes of alcohol are poured into it; the flask is shaken well in order to mix them. The action takes place as in sulphuric acid; the liquor is heated to from 110° to 120°F.; next day, regarding it by transmitted light, it is of a fine blue, reflecting green and violet: by the light of a candle, it appears purple-red. If it be heated to from 150° to 160°F., and especially if it be boiled, it becomes green, and a very long time is then required for the restoration of the violet-blue color. By submitting this blue liquor to spontaneous evaporation at the ordinary temperature of the atmosphere,

* Spontaneous evaporation requiring much time, we may, by modifying the process, operate in the following manner, for promptly procuring very large quantities of crystallised chrome alum. 150 grammes of bichromate of potassa, in crystals, with 1 quart of water, and 250 grammes of sulphuric acid, are put into a flask on a sand-bath, until all the chromate is dissolved. When the solution is cooled to about 81°F., it is poured into a large capsule, and 15 grammes of alcohol was added, the liquor being stirred in order to mix it well. The temperature of the liquor rapidly rises to about 130°F., then it gradually descends. At the end of three-quarters of an hour, or an hour, when it has descended to about 90°F., 15 grammes of spirit of wine are again added. The liquor is again heated to about 130°F. At the end of an hour, when the temperature has descended to about 100°F., about 30 grammes of spirit of wine are added. The temperature again rises to about 120°F.; then, being left to itself till next day, a crystallisation of 170 or 175 grammes of chrome alum is then found. The mother-liquor is poured into a flask on 50 grammes of powdered bichromate of potassa, and 80 grammes of concentrated sulphuric acid and 20 grammes of alcohol are added to it. The flask is often shaken to facilitate the solution of the salt. The temperature of the liquor is raised by the reaction to about 120°F. At the end of an hour, when all the chromate is dissolved, the liquor is poured into the capsule, and it gives, by the next day, a second crystallisation of about 120 grammes of chrome alum. The mother-liquor, again treated in the same manner, with the same quantities of powdered bichromate of potassa, sulphuric acid, and alcohol, gave a third crystallisation of about 120 grammes of chrome alum. The mother-liquor of the latter, of a fine green color, then contains a very large quantity of chrome alum in the green, uncrystallisable state; but if it be left by itself for some time, a large quantity of crystallised salt is again obtained. To obtain pure chrome alum in fine large crystals, the salt obtained as above is redissolved in about three times its weight of water, heated to 120°F., by the cooling of the liquor, from one day to the next, nearly half the salt dissolved crystallises. The mother-liquor may be used instead of pure water for redissolving and recrystallising these new quantities of salt, taking care not to heat it beyond 120°F.

a crystallisation of chloride of potassium is formed, in the midst of a green syrupy mother liquor, containing hydrochlorate of chrome, which does not crystallise. The same thing takes place by evaporating by heat until crystals are formed on its surface. The two salts separate, and do not form a double salt, as takes place in the treatment of bichromate by sulphuric acid.

If the liquor be evaporated by heat to dryness without separating the chloride of potassium, a porous saline residue of a purple-violet color is obtained. This residue powerfully attracts moisture from the air, and liquefies into a green syrupy liquor. I shall return to this purple-violet residue further on.

WITH NITRIC ACID.

V. 50 grammes of bichromate of potassa are dissolved in 790 grammes of water; 140 grammes of concentrated nitric acid and 20 grammes of alcohol are added to it. The flask is shaken in order to mix them; by the reaction the liquor is heated to about 100° or 110°F.; next day it is of a blue color, verging on purple or violet. By boiling this liquor, its color is of a fine green while it is boiling, but after cooling it is again of a violet-blue. Submitting to spontaneous evaporation, or evaporated by heat, it gives a crystallisation of nitrate of potassa. The mother-liquor contains nitrate of chrome, and appears of a deep green when it is concentrated to the syrupy state. Here, again, as, however, might be expected, the two salts separate and do not form a double salt. We cannot evaporate this liquor to dryness; by heat, nitrate of chrome begins to be decomposed and to disengage binoxide of nitrogen as soon as it is reduced to the pasty state, and chromate of chrome is formed.

It is seen that, by heating the solution of nitrate of chrome obtained by this reaction of alcohol and nitric acid on bichromate of potassa, it appears quite green so long as it is boiling, but it does not retain this color long, like that of the sulphate, and especially that of the hydrate of chrome treated in the same manner; it immediately becomes violet-blue by cooling. The same thing occurs with the solution of nitrate of chrome, which is obtained by combining 1 equivalent of hydrate of chrome with 3 equivalents of nitric acid, as we shall see further on.

I have availed myself of the property which nitric acid possesses of immediately forming with hydrate of chrome a blue salt which is soluble in alcohol, for preparing in a very short time Schroetter's blue simple sulphate, by operating in the following manner:—

A solution is made of 50 grammes of dry gray-green hydrate of chrome in 125 grammes of concentrated nitric acid, diluted with 125 grammes of water, and the whole is boiled for a quarter of an hour. When it is cold, 75 grammes of concentrated sulphuric acid, previously diluted with 150 grammes of water, and cooled, are added to it. This mixture is put into a large flask, and, the flask being kept well shaken, 1,200 grammes of spirit of wine are poured into it. Small pearly bracteæ are formed by agitation, and in a short time the quantity is so considerable that the liquor becomes almost a pap. The sulphuric acid decomposes the nitrate of chrome and forms a sulphate, which, being insoluble in rather dilute alcohol, is precipitated.

It is allowed to react for one or two hours, the flask being frequently shaken; then the whole is put into a very large filter. The alcoholic liquor passes through colored of a blueish-green; when it has entirely passed through, and when the voluminous precipitate is somewhat heaped up, by small shocks given to the funnel, 500 grammes of spirit of wine are poured on it, which also pass through somewhat colored. By repeating this washing twice more, the last portion of spirit of wine passes through colorless. The filter is then removed from the funnel, gently pressed between several folds of blotting-paper, replaced by others as soon as they are saturated with alcoholic liquor, until the salt finally remains under the form of a dry cake, which does not moisten the paper under high pressure. This cake is formed of small crystalline bracteæ, of a light, silvery, ashy-lilac. We thus obtain about 65 or 70 grammes of this dry sulphate of chrome, in the state of violet-blue modification, and whose formula is $3SO^3\ Cr^3\ O^3 + 15HO$.

(To be continued in our next.)

ON OXIDATION.—STUDY OF THE PRINCIPAL OXIDISING RE-AGENTS.*

BY M. P. C. BOUDAULT.

M. BOUDAULT, who is already known to chemists by his important investigations on the distillation of dragon's blood, and on the catalytic force, has presented to the school a thesis worthy of its honorable predecessors.

He has not confined himself, indeed, to

* *Journal de Pharmacie et de Chimie*, June, 1845.

tracing an excellent monograph of the principal phenomena of oxidation; he has been able to append to this subject new observations on cyanogen regarded as an agent of oxidation; and these observations, which are his own, give to a part of his work all the interest of an original work. We now present, in few words, a view of the progress followed by the author in the development of this subject, and the peculiar ideas which he has expressed concerning some points of theory. We will afterwards copy *verbatim* all that relates to oxidation by cyanogen.

Taking as a point of departure the remarkable discoveries of Lavoisier, M. Boudault successively considers the principal oxidising reagents, air and oxygen, water, binoxide of hydrogen, the metallic oxides, sulphuric acid, nitric acid, nitrate and chlorate of potassa; then chlorine, bromine, and iodine, which determine oxidation only in an indirect manner; he adds to this cyanogen, in which he has discovered a new trait of resemblance to the haloïds, in proving that this body is also a very energetic agent of oxidation.

In the first chapter, the author passes in review the very new and interesting circumstances in which the oxidation of organic and inorganic matters takes place under the influence of the air and oxygen. He recalls, on this occasion, the beautiful investigations of M. Frémy on the metallic acids; then, turning all his attention to organic matters, he quotes numerous examples of simple oxidation, such as those of the essences of bitter almonds, cinnamon, cumin, &c. He afterwards considers the complex substances, when they present, under the influence of the air and oxygen, the phenomena of fermentation, putrefaction, and combustion.

"These phenomena," says he, "belong to the class of simple oxidations; oxygen acts indifferently on carbon and hydrogen: these are true combustions, more or less complete, of organic substances.

"In organic chemistry, no distinction is established between oxidations and combustions; these two phases are united under one denomination. Liebig has given the name of *slow combustion* or *eremacausie*, to complex reactions, as well as to simple reactions. Should not a distinction be made between reactions which give definite compounds and these slow decompositions of complex bodies into their elements? Can the action of air on essence of turpentine, which gives formic acid, and on the essential oils, producing their respective acids, be compared to their decomposition of ligneous substances, which is converted into a brown matter, to which the name of rot is given? Certainly the word eremacausie is adapted to the slow combustion of ligneous matter, which burns in contact with the air; but why not leave the word oxidation for the simple reactions of oxygen on definite compounds?

"The word combustion is well applied to the last phase of the reaction of oxygen; it indicates, perfectly, the disunion of all the elements of the body.

"The action of oxygen on organic matters may be divided into four phases:—1st, simple oxidation, or absorption of oxygen; 2nd, the hydrogen of the matter may be removed in the state of water; 3rd, action of oxygen on carbon; and, in the 4th phase, fermentation, putrefaction, and combustion are found united."

Further on, the author, after having studied oxidation under the influence of the catalytic force, foretells, in some measure, the discoveries which the attentive study of this force, as regards oxidation, promises to science.

"Under its influence," adds he, "we see the same combinations operated at lower temperatures than in the ordinary conditions.

"Is it not, so to speak, certain that intermediate oxygenous combinations may be obtained at the moment when, by diminishing their temperature, the formation of the ultimate products of the reactions may be prevented?"

Oxidation by oxygenised water, the metallic oxides, the acids, chlorine, bromine, and iodine, also furnish to the author opportunity for judicious remarks. After having established that the latter three bodies react in the same manner, but with greater or less energy, he examines the substitution of chlorine for oxygen in an oxygenous compound, the action of chlorine on an alkaline solution, and on certain metallic oxides in the presence of water, which yields to them its oxygen, whilst the metalloïd removes the hydrogen. He also notices the action of chlorine on alcohol, and devotes himself particularly to the oxidations which have given to MM. Fordos and Gélis new acids of sulphur, and enabled M. Frémy to prepare ferric acid in the humid way.

"Chlorine, bromine, and iodine," says he, in conclusion, "act in two ways: 1st, they decompose the oxygenous bodies and remove the oxygen; 2nd, they serve as oxidising agents by an indirect action.

"Chlorine, bromine, and iodine, are not the only bodies which may serve as indirect agents of oxidation. I shall demonstrate that cyanogen may be ranked, from this remarkable property, in the same group as the haloïds.

"OXIDATION BY CYANOGEN.

"Since M. Gay-Lussac's discovery, numerous investigations have been made concerning cyanogen. The experiments have always shown that this compound could act the part of a simple body; indeed, it combines, like chlorine, bromine, and iodine, with oxygen and hydrogen, to form acids, and with the metals to form cyanides. In a word, almost all the properties of cyanogen coincide with the properties of haloïd bodies.

"We have examined the action of chlorine on the alkalies; the action of cyanogen is also very clear; it forms cyanuret of potassium and cyanate of potassa. In the same circumstances, iodine forms iodides and iodates, &c. These two reactions are, therefore, identical. It was necessary to ascertain whether the reactions may be continued with cyanogen as with chlorine: would water be decomposed in presence of the oxides or of certain organic compounds? Would the alkalies, under the direct influence of cyanogen, yield oxygen to two intermediate bodies? The instability of cyanogen in water has always appeared to me an obstacle to the investigations which I attempted; the products which I obtained by repeating the same reactions, showed me sufficiently that free cyanogen could never give very certain results. It was necessary to replace cyanogen by a compound in which this body was in very unstable combination, and, if I may so speak, in the state of mobility, in order that it might react on oxygenous compounds in such a manner as to remove their oxygen.

"I thought that the ferridcyanide of potassium might be the body which I sought for.

"In the first place, let me say one word concerning the preparation of this salt and its characters. Gmelin obtained this beautiful red salt by passing chlorine into a solution of ferro-cyanide of potassium, until the salts of peroxide of iron did not give Prussian blue with this liquor. The reaction is very simple; it may be interpreted as an oxidation, admitting, however, the theory of the double cyanide—

$$2(Cy\ Fe + Cy^2\ P^2) + Cl = Cl\ P + Cy^6\ Fe^2 + Cy^3\ P^2.$$

"The character which it is especially important for me to bear in mind is the easy decomposition of this salt, and the products which result from it. Under the influence of heat, and out of contact with the air, the ferrid-cyanide is decomposed into ferro-cyanide of potassium, cyanide of potassium, carburet of iron and nitrogen, and it gives free cyanogen; but, if the decomposition be carried on in the air, instead of nitrogen and carburet of iron, peroxide of iron and a more abundant disengagement of cyanogen are obtained. It appeared to me evident that this compound, which so easily yielded cyanogen by heat, and at a very moderate temperature might, in certain reactions, yield a part of the cyanogen which it contains.

"When ferrid-cyanide of potassium is treated with sulphuretted hydrogen, ferro-cyanide of potassium is formed; the reaction appears very complicated: nevertheless, this fact still confirmed my supposition: hence, I employed this double salt as a substitute for cyanogen, which was not adapted for all the reactions which I proposed to make.

"I first examined the action of potassa on Gmelin's salt, to endeavor to restore to it the equivalent of potassium removed by chlorine in its preparation. At the ordinary temperature there is no reaction, even during the space of a month; the potassa remains in contact without altering the solution of ferrid-cyanide. When I boiled this same solution there was no perceptible reaction until it arrived at a very concentrated state; peroxide of iron was precipitated; at the same time the formation of ferro-cyanide of potassium and of cyanide of potassium were noticed: I have never been able to collect oxygen in this reaction. However, these decompositions of ferrid-cyanide into ferro-cyanide gave me the certainty that the cyanogen might act as if it were free; hence, I thought of availing myself of the intervention of a body having a powerful affinity for oxygen, and I placed myself in circumstances similar to those which determine the reaction of chlorine or potassa in presence of an oxide. I then considered the double cyanide as acting like free cyanogen in presence of potassa and of a metallic oxide. I heated a solution of red salt and potassa in presence of oxide of lead, and immediately obtained a brown precipitate, which was only pure oxide of lead; the liquor contained only ferro-cyanide of potassium. I was already able to deduce this consequence, that the cyanogen of the ferrid-cyanide might replace free cyanogen; cyanogen was already known to be an energetic oxidising agent.

"Before endeavoring to give the explanation of these phenomena, I will quote a number of facts which confirm this mode of oxidation.

"SUPER-OXIDATION OF METALLIC OXIDES.

"The metallic oxides have a tendency, in general, to pass to a higher degree of oxidation; a certain number of these undergo this modification with the greatest facility, under the oxidising influence of cyanogen, either at the

ordinary temperature, or at the temperature of ebullition, according as the oxide is more easily super-oxidisable.

OXIDE OF MANGANESE.

When a salt of protoxide of manganese, or protoxide of manganese precipitated out of contact with the air, is put in contact with a solution of ferrid-cyanide of potassium and potassa, peroxide of manganese and ferro-cyanide of potassium are immediately obtained, without the aid of heat. I have obtained with great quantities of potassa crystallised peroxide of manganese.

If, instead of adding an excess of ferrid-cyanide, we put, on the contrary, an excess of salt of manganese and also much potassa, a brown-red precipitate is obtained, which is probably manganeso-manganic oxide: I have not yet analysed this oxide.

OXIDE OF NICKEL AND COBALT.

Protoxide of nickel, put into the alkaline solution of the red salt, does not appear to be super-oxidised, and if it be boiled for some time there is a decomposition which has no connexion with the phenomena of oxidation. It is the same with cobalt; however, there is a period at which the oxide of cobalt seems to be super-oxidised, but it is probably reduced by the ferro-cyanide which is formed.

OXIDE OF LEAD.

The super-oxidation of lead, under the influence of the oxidising reaction, is the clearest fact which I have been able to obtain at the temperature of ebullition; a salt of protoxide of lead, or of peroxide in solution in potassa, is super-oxidised; the pure oxide is obtained in the crystalline state. I think, however, that this fact depends on the quantity of potassa employed.

I have sometimes obtained minium by adding a salt of lead, and by heating the liquor gently.

OXIDE OF COPPER.

Ferrid-cyanide of copper, or oxide of copper, is super-oxidised in presence of the solution of ferrid-cyanide and potassa, and we have a black precipitate, and always some ferro-cyanide.

OXIDE OF TIN.

Salts of protoxide of tin give a reduction of the ferrid-cyanide, which indicates super-oxidation; nevertheless, I have not been able to ascertain the nature of the oxide which is precipitated. It might be the stannate of protoxide of tin discovered by M. Frémy.

OXIDE OF CHROMIUM.

The oxidising action of ferrid-cyanide of potassium on the oxides may be still more energetic than we have hitherto remarked. The example of the oxidation of chromium should present a certain degree of interest. A solution of oxide of chromium in potassa, treated by ferrid-cyanide of potassium, at the boiling temperature, is converted into chromate of potassa, and if sufficient oxide be added all the ferrid-cyanide is converted into ferro-cyanide. It is evidently a very energetic oxidation, for hitherto we have not been able to obtain chromate of potassa by the humid way.

OXIDES OF GOLD AND SILVER.

We have already met with oxides which cannot be super-oxidised by means of ferrid-cyanide and potassa; these two oxides are among the number. If a solution be used without the aid of heat, nothing occurs; but at a temperature approaching ebullition, a precipitate of peroxide of iron is formed; in the liquor ferro-cyanide of potassium and cyanide of gold are found in solution in cyanide of potassium.

The salts of silver, submitted to the same reaction, present the same phenomenon: ferro-cyanide is formed, peroxide of iron is precipitated, and in the liquor from which I precipitated the yellow cyanide by alcohol, I found cyanide of silver and cyanide of potassium. These facts result from simple oxidation; they are true decompositions.

SULPHUR.

Sulphur may also be oxidised under this same influence; it passes to the state of sulphuric acid.

ACIDS AND SALTS.

Certain mineral acids may be super-oxidised, either in the free state, or in the state of combination. Phosphorous acid and the soluble hypophosphites are converted into phosphates. Sulphurous acid gives sulphuric acid, and the sulphites are formed into sulphates. Oxalic acid and the oxalates, put into the alkaline solution, almost instantaneously give carbonates.

After having examined a great number of bodies which, under the influence of ferrid-cyanide of potassium and potassa, are converted into compounds containing more oxygen, I will now explain the phenomena of the reaction. Ferrid-cyanide of potassium is regarded as a double salt, that is to say, a compound of percyanide of iron and cyanide of potassium. Now, this percyanide of iron does not exist in the free state; it is a very unstable body; it exists only in Gmelin's red salt, in which it is retained by the cyanide of potassium. As soon as a reaction is determined in presence of this salt, there will be a tendency to decomposition. May not the explanation of this reaction be

based on this instability. Indeed, if we express this idea in a formula, we find—

$Cy^3 Fe^2 + Cy^3 P^6 = Cy^3 Fe^2 + Cy^3 P^3 + Cy.$

By adding the potasta in presence of oxide of lead, we have—

$$Cy^2 Fe^2 Cy^3 P^6 + Cy + PO + PbO$$
$$= Cy^2 Fe^2 Cy^3 P^3 + CyP + PbO^2$$
$$= 2Cy\ Fe\ Cy^2 P^2 + PbO^2.$$

Thus, from this formula, we find 2 equivalents of ferro-cyanide of potassium and pure oxide; this is what was met with in the reaction.

Ferrid-cyanide of potassium, which I regard in this case as ferro-cyanide minus potassium, and consequently as possessing an equivalent of free cyanogen, may be substituted for cyanogen. It is, therefore, demonstrated that cyanogen possesses another point of analogy with chlorine, bromine, and iodine —that of being, like them, an energetic oxidising agent.

If the whole of this work be examined, it will be found to be composed of two parts: in one, I endeavor to collect and discuss the different modes of oxidation known, and to present the different points of view under which these oxidations should now be regarded; in the second part are found the results of my own investigations, which induce me to regard cyanogen as an energetic oxidising agent comparable to chlorine.

ON RUTINIC ACID.*

BY A. BORNTRAGER.

A CRYSTALLINE body is contained in the ruta graveolens, which was first observed by Dr. Weiss, and called *rutin* by the same chemist.† At the request of Professor Wöhler, I have subjected this substance to a nearer examination. Though of but a feeble reaction, it is decidedly an acid, and I will call it *rutinic acid*; I thus avoid the termination *in*, which is exclusively applicable to organic bases.

In preparing rutinic acid, I followed the process adopted by Dr. Weiss, with some modifications, however, which will be noticed in the sequel, and by which alone I succeeded in obtaining the substance in a pure state. The ruta graveolens, after being cut into shreds and dried, is boiled with common vinegar for half an hour, the liquid expressed and allowed to stand for several weeks. During this time the rutinic acid, with an admixture of a great many other substances, separates partly as a sediment, partly as microscopic crystals. By removing and evaporating the supernatant liquid, an additional quantity may be obtained.

The impure acid deposited is washed with cold water, boiled with about four times its own quantity of pure acetic acid, previously diluted with four parts of water, and the solution is filtered. In a few days the greatest portion of the rutinic acid is again separated in a crystalline state. By a fractioned evaporation of the remaining solution, after some time, a fresh quantity is again deposited. The whole of the acid obtained is washed with cold water, dissolved in about six times its weight of boiling alcohol, and on an addition of well-prepared animal charcoal, it is to be filtered. The solution is mixed with one-eighth water, the alcohol entirely distilled off, and the residue allowed to stand for some days in a cool place, until the acid is crystallised out. A great deal, however, remains dissolved in the mother-liquor, hence this must be concentrated again by evaporation. In all cases the acid is only very gradually deposited, and this takes place with a facility proportionate to the low degree of temperature employed. Among the other advantages of this process a saving in alcohol is effected; because, though rutinic acid, if pure, is easily soluble in alcohol, when it is mixed with other substances, great quantities of the latter liquid are required for its solution.

The quantity obtained is very inconsiderable; the first acid extract deposits, indeed, a voluminous mass of impure acid, but this is light, and contaminated with a great many foreign matters, and finally but little acid in a pure state remains. The rutinic acid thus prepared, and purified, is distinguished by the naked eye as a green-yellow crystalline powder. Viewed by the microscope of 200 powers, it is found to consist of concentrically united four-sided prisms, with very acute terminal sides. The color is manifestly peculiar to it, as on being separated from its combinations by bases, it was invariably obtained with the same color. It is tasteless, but its solution in alcohol, on being tested with litmus paper is decidedly of an acid reaction. On being heated in the open air it fuses, exhaling an odor resembling that of heated sugar, and burns with a flame. Heated in an oil-bath, it fuses at 356°F.; without losing water it fuses into a yellow tenacious liquid. It solidifies on cooling, being partially crystallised; at 428° F. a small portion of it sublimes, forming yellow drops; at 468°F. it is converted into charcoal.

Rutinic acid is only sparingly soluble in

* Liebig's *Annalen der Chemie und Pharmacie*, No. 3, 1845.

† *Pharmaceutisches Centralblatt*, p. 903, 1842.

cold water, less soluble than in hot water; but the hot saturated solution deposits nothing on cooling; nay, the solution may be evaporated to one-sixth of its original bulk, without depositing anything, even after having been allowed to stand for some time. Only when further concentrated, after some days, crystallisation begins, and proceeds gradually. The solution is of a pale-yellow color.

It is sparingly soluble in cold absolute alcohol; it is easily soluble in boiling alcohol of 76°. But even this solution deposits nothing on cooling, which takes place only after great concentration, and evaporation. The solution is then converted into a pulpy mass, in which no crystals are observed, rutinic acid being obtained from alcohol in a crystalline state, if the solution is mixed with one-sixth of water previous to evaporation.

The slowness with which the rutinic acid is deposited from its solutions, is perhaps attributable to the circumstance, that the same substance in these solutions has a different constitution in its crystalline state. Perhaps in the latter it contains one more atom of water, which separates on the action of the hot menstruum, but which is afterwards very gradually recovered by the process of crystallisation. The composition of its lead combination, in which two atoms of water are replaced by one atom of oxide of lead, induce me to suppose that a similar action takes place.

Rutinic acid is completely insoluble in ether, even when boiling. It is easily soluble in a dilute solution of potassa, with a red-yellow color. The same takes place with caustic ammonia, carbonate of soda, baryta, strontia, and lime-water. On exposure to air, the solution in potassa rapidly becomes darker, and the rutinic acid, especially if allowed to evaporate, is destroyed, and converted into a brown substance resembling humus. It acts in the same manner in a solution of hydrate of potassa in alcohol. Carbonic acid, passed through this solution, throws down carbonate of potassa, and pure rutinic acid remains dissolved, and it is generally separated in an unchanged state, by other acids, from its solution in the alkalies, so long as it has not been altered by the air; but even in the latter case, it is only very gradually deposited.

The solution in caustic ammonia, on being evaporated, leaves behind pure rutinic acid, free from ammonia.

A solution of chloride of calcium in alcohol, having been mixed with a solution of rutinic acid in alcohol, a dark green precipitate was thrown down, which, however, being prepared in fractioned portions, exhibited so heterogeneous a constitution, that it was not qualified for an elementary analysis. Salts of copper gave precipitates, but these varied in color. The rutinic acid was decomposed by nitrate of silver, and the silver was reduced. Oxide of lead was the only base with which a definite combination could be obtained. By mixing a solution of rutinic acid with that of acetate of lead, an orange-yellow precipitate was thrown down, of the same purity and brightness of color as chromate of lead. I prepared this combination for analysis, with the precaution, which seemed to me necessary, of avoiding the formation of an admixed basic combination, and to prevent the access of air. I conducted the precipitation in a close vessel, mixing the alcoholic solution of rutinic acid with an alcoholic solution of acetate of lead. I found that the precipitate, on being washed and dried, contained neither carbonate nor acetate of lead.

The elementary analysis of rutinic acid, dried at 212° F., which was separately examined, in an atmosphere of well-dried oxygen, free from carbonic acid, showed that it contained no nitrogen.

I. 0·288 grs. gave 0·146 $\dot{H}$ and 0·534 $\ddot{C}$.

II. 0·2705 ,, ,, 0·136 ,, 0·4965 $\ddot{C}$.

Hence results the following constitution:

	I.	II.	Calculated according. $C^3\ H^2\ O^2$
C	50·34	50.27	50·04
H	5·55	5.54	5·54
O	44·11	44.19	44·42

The lead compound gave the following result:—

I. 0·421 grammes rutinate of lead gave 0·465 $\ddot{C}$ and 0·093 $\dot{H}$

II. 0·4005 ,, ,, ,, 0·448 ,, 0·095 ,,

III. 0·3465 ,, gave 0·2215 Pb $\ddot{S}$ = 0·163 Pb

IV. 0·3935 ,, ,, 0·2515 ,, = 0·184 ,,

From this it appears, that in the lead salt, for one atom of oxide of lead, an amount of rutinic acid is contained, which includes 12 atoms of carbon, 6 equiv. of water, and 6 equiv. of oxygen, viz.:—

	I.	II.	Calculated according. Pb+C^{12} H^{6} O^{6}
C	30·20....	30·37	30·34
H	2·46....	2·63	2·52
O	20·32....	19·05	20·19
Pb	47·02....	47·05	46·94

Hence, the acid, combined with the oxide of lead, contains two equiv. of water less than the crystallised acid, the constitution of which must, therefore, be expressed by the following formula :—

$$C^{12}\,H^{8}\,O^{8} \text{ or } C^{12}\,H^{6}\,O^{8} + 2\dot{H}$$

—nt—

CONFERVA DEVELOPED IN A SOLUTION OF ARSENIC.

In the *Comptes Rendus* for April there is a note from M. Bory de Saint Vincent "Concerning Conferva developed in a Solution of Arsenic," in which he claims the discovery of such a curious fact for a M. Boutigny, who, it appears, accidentally observed it in 1835 or 1836. That a substance possessing all the characters of conferva should grow and flourish in the solution of a mineral which is generally found to be so destructive to vegetable as well as animal life, must always be considered as a curious fact; but, that it was a *novel* one in 1835 is very doubtful, for no one who has ever kept arsenious acid in solution in distilled water, could have failed to detect this confervoïd growth, any more than he would in the simple water itself. Not so, however, if the arsenic has been dissolved in spring or river water; in this case we have never found any such growth, even after the arsenic has been in a solution for years.

ON THE DETERMINATION OF HYDROCHLORIC ACID IN A LIQUID, CONTAINING FREE CHLORINE.*

BY DR. KOENE.

If we have to determine a lower stage of oxidation than a substance is capable of attaining, we generally have recourse to chlorine, oxidise the same, and form hydrochloric acid. To arrive, however, by this process at an accurate result, it becomes necessary to determine the acid, unmixed with any free chlorine; it, however, frequently happens that we cannot obtain this end unless by proceeding in the following manner:—

The substance to be examined, and a sufficient quantity of sulphate of potassa are dissolved in water, that by the hydrochloric acid thus formed, and this salt, a chloride, and bisulphate of potassa may be produced. Afterwards, preventing daylight from having access, chlorine is passed through, and when the same has ceased to act, the excess is expelled by a current of air. Finally, the chlorine of the chloride is determined by nitrate of silver.

—nt—

INVESTIGATIONS OF SEVERAL NATURAL AND ARTIFICIAL COMBINATIONS OF PHOSPHORIC ACID.*

BY C. RAMMELSBERG.

(*Continued from page* 294.)

PHOSPHATE OF ALUMINA.

The real constitution of the artificially prepared combinations of the phosphate of alumina has hitherto been but little known. The elementary works generally distinguish two salts; 1, a *neutral one*, produced by the precipitation of a neutral alumina salt with $\dddot{Na}^{2}\dddot{P}$, to which consequently the composition $\dddot{Al}^{2}\dddot{P}^{3}$ is considered to answer; and 2, a *basic one*, which is formed when the former is dissolved in an acid, and is precipitated from it by an excess of ammonia. It is designated by $\dddot{Al}^{4}\dddot{P}^{3}$, though the analyses of neither have ever been made known.

The other properties of these salts having been rather accurately examined, I have taken great pains to ascertain their composition, which is precisely the most difficult task met with in the establishing of their characters.

It is known that a combination of phosphoric acid and alumina cannot be decomposed by calcination with an alkali, because by the washing with water a considerable portion is dissolved with the excess of the alkali. We are indebted to Berzelius for a method of analysing the wawelithe,† according to which the decomposition is operated by calcining with a mixture of an alkaline carbonate with silica. Fuchs, however, in his turn, employed basic silicate of potassa, in which the principle of the method is the

* Poggendorff's *Annalen der Physik und Chemie*, No. 3, 1845.

* Poggendorff's *Annalen*, March, 1845.

† *Ann. Chem. Phys.*, vol. xii., p. 19., and Schweigger's *Journ.*, vol. xxvii., p. 36.

same as in the preceding.* Though Berzelius's method may be employed in most cases, yet it is too circumstantial, and a portion of the silica added always remains dissolved in the alkaline liquid, which by an addition of carbonate of ammonia, and evaporation cannot be completely removed; we may certainly better succeed in obtaining the last mentioned end by dissolving oxide of zinc in the carbonate of ammonia employed, but to remove the zinc, the process becomes more complicated still. I have frequently employed this method, when operating with artificial phosphate of alumina, and several minerals described in the foregoing, which contain these two bodies. I have since succeeded in contriving another method requiring less time, and, with certain precautions taken, yields as accurate results as any, and deserves to be noticed, even if it were only because it may serve as a controling process. The substance is first dissolved in a sufficient quantity of sulphuric acid previously diluted with an equal amount of water, and then a sufficient quantity of sulphate of potassa is added, which by heating is likewise dissolved.† The whole is mixed with a large quantity of strong alcohol (80 per cent.) and allowed to stand for some hours. By this treatment alum and sulphate of potassa are separated, whereas the phosphoric, and sulphuric acids remain dissolved. The liquid mass is then filtered, washed with alcohol, the residue dissolved in water, and the alumina precipitated with ammonia. Water is added to the alkaline filtered mass, which on being evaporated again to remove the alcohol, is supersaturated with ammonia, when generally a few milligrammes (50 milligrammes = one grain) are thrown down, owing to a trace of alumina, and finally precipated by chloride of calcium. In filtering the precipitate it is not necessary to keep the air from it; as it invariably contains sulphate of lime, it is washed only for a short time, dissolved in hydrochloric acid, then diluted with water, to which a small quantity of alcohol has previously been added. On filtration the sulphate of lime is left behind, and then pure phosphate of lime is precipitated by ammonia.

A. THE SO-CALLED NEUTRAL PHOSPHATE OF ALUMINA.

On adding to a solution of alum a solution of common phosphate of soda, until it ceases to produce a precipitate, the filtered mass, though it contains an excess of the alkaline precipitate, acquires a *strongly acid* reaction, is free from alumina, but contains much phosphoric acid. We may, therefore, anticipate that the process going on in this case will be similar to that taking place with silver, in which, as is known, $(\dot{N}a^2, \dot{H})\ \dddot{P}$ throws down $\dot{A}g^3\ \dddot{P}$.

The gelatinous precipitate requires a continued washing; and, by drying in the air, becomes a white, very loose, hygroscopic powder. Wherefore, it is scarcely possible to say how much water it essentially contains. (*a*) One portion air-dried, that is, dried in a tube of a furnace, lost on calcination 38·17 per cent. (*b*) Other portions were partly kept for some time under a bell-jar over sulphuric acid, and partly by means of sulphuric acid dried in vacuô. The loss of water by the calcination of the salt was in both cases almost the same. It amounted in four experiments to 30·63, 31·64, 31·75, 33·05 per cent.

The further results of the analysis were as follow:—

	(*a*)	(*b*)		
	With $\dddot{S}$ and $\dot{K}\ \dddot{S}$.	With $\dddot{S}i$ and $\dot{N}a\ \dot{C}$.	With $\dddot{S}$ and $\dot{K}\ \dddot{S}$.	The same.
Phosphoric acid..	35·07 .. 37·55	41·85	38·62	36·92
Alumina ..	not determined 26·34	26·14		24·50
Water	39·17	33·05		
		101·04		

The quantities of the alumina and phosphoric acid are almost invariably as 3 : 5, the combination is therefore for certain as $\dddot{Al}\ \dddot{P}$, but the amount of water varies to an extent that the air-dried salt contains 9 atoms; that dried over sulphuric acid contains partly 8, partly 7, and 6 atoms. It is not improbable that the number 6 expresses the atoms of water of a determinate hydrate; and that 7, 8, and 9 are intermediate stages of mixtures of this salt.

* Schweigger's *Journ.*, vol. xxiv., p. 126.

† Fusing with bisulphate of potassa would likewise answer the purpose, but by calcining a portion of the phosphoric acid might be lost.

The composition resulting from calculation is the following :—

	$\dddot{Al}\dddot{P}+9\dot{H}$	$+8\dot{H}$	$+7\dot{H}$	$+6\dot{H}$
Phosphoric acid	35·03 ..	36·65 ..	38·43 ..	40·39
Alumina	25·22 ..	26·39 ..	27·66 ..	29·07
Water	39·75 ..	36·96 ..	33·91 ..	30·54
	100·	100·	100·	100·

B. THE SO-CALLED BASIC PHOSPHATE OF ALUMINA.

To prepare the body in question, the foregoing salt was washed, dissolved in hydrochloric acid when still in a moist state, and precipitated with ammonia. This precipitate is still more diaphonous and gelatinous than the preceding, and is more difficult to be washed, as it finally obstructs the filter and forms a kind of emulsion with the water.

(*a*) Air-dried substance gave—

	With Si and Na C.	
Phosphoric acid,...	34·57	
Alumina	36·78	
Water	29·04	28·22
	100·39	

(*b*) Dried at 212° F. in a water-bath—

	With $\dddot{Si}$ & Na C.	With $\dddot{S}$ & K $\dddot{S}$.
Phosphoric acid..		37·04
Alumina.....	37·23	37·36
Water	23·79	

The combination, therefore, is = $\dddot{Al}^4\dddot{P}^3$, and contains air-dried 18 atoms, and at 212° F. only 15 atoms of water. Calculation gives—

	$\dddot{Al}^4\dddot{P}^3+18\dot{H}$	$+15\dot{H}$
Phosphoric acid..	36·81	38·61
Alumina.....	25·34	37·06
Water	27·85	24·33
	100·	100·

Vauquelin stated that if phosphate of alumina is digested with a solution of carbonate of potassa it is converted into pure alumina. On repeating the experiment, by first allowing to digest for fourteen days and then boiling the whole, it was found that a small portion of acid only was subtracted from the salt A, and seemingly as much as was necessary to convert it into B, that is one-fourth of the whole.

Of all the methods devised for testing a precipitate of alumina, especially after calcination, for the amount of phosphoric acid, a case of frequent occurrence in similar experiments, I consider the one proposed by Otto to answer best. For a quantitative determination, however, it was inadequate, as it gave some per cents. of phosphoric acid less than was obtained by other processes, and in addition to this, by the presence of tartaric acid, the determination of the alumina becomes circumstantial.

VIVIANITE.

We are not in want of investigations of the crystallised phosphate of iron, to which the name of blaueisenerz, or vivianite, was given. Analyses of that substance were communicated by Laugier, Vögel, Stromeyer, Thomson, and Segeth, which, however, coincide but little, and do not indicate the true composition of the salt, as they suppose the whole amount of iron to be contained in a state of protoxide, though, in addition to this body, the oxide of iron is likewise a constitutional part of the same salt.

We reduced the analyses we possess into the following table :—

	From Isle de France, by Laugier.	From Bodenmais, by Vögel.	From St. Agnes in Cornwall, by Stromeyer.	From New Jersey, by Thomson.	From Kertsch, in the Krimm, by Segeth.
Phosphoric acid	21·	26·4	31·1825	26·06	24·95
Protoxide of iron....	45·	41·6	41·2266	46·31	48.72
Water	34·	31·0	27·4843	27·14	26·26
	100·	98·4	99·8934.	99·51	100·

I employed in my experiments a vivianite in indistinctly crystallised needles, from the Mulica Hills, in New Jersey (probably the same variety which Thomson analysed, called by himself the mullicite), and the known crystallised variety from the Bodenmais. The first I obtained from the I. R. mineral collection of Vienna, and the latter from the R. mineral collection of Berlin.

I. *Vivianite from New Jersey.*—Its specific gravity I found to be = 2·68 (temperature 59° F.).

(*a*) 2·117 after being dissolved in hydrochloric acid left behind 0·036 quartzy grains (it occurs in a ferruginous quartzy conglomeration). The solution was precipitated by ammonia and hydrosulphuret of ammonia, the sulphuret of iron dissolved in aqua regia,

and precipitated with ammonia, and in that manner was obtained 1·935 oxide of iron, = 49·74 per cent. The phosphoric acid precipitated by chloride of calcium gave 1·41 phosphate of lime, in which 0·5906 phosphoric acid = 28·4 per cent. were found.

(b) 2·008 of the finely-powdered mineral were calcined in a current of dry hydrogen gas. The water collected in a receiver containing chloride of calcium was = 0·512, and the gray residue amounted to 1·466. By dissolving this in hydrochloric acid, no gas is disengaged, and 0·146 quartz are left behind. The solution, after being evaporated to dryness, and the residue fused with carbonate of soda, gave 0·919 oxide of iron.

(c) 1·8 were boiled with diluted hydrochloric acid and a weighed quantity of pure copper until the whole iron was reduced to the state of protoxide. 0·168 of copper was dissolved, and 0·04 quartzy sand left behind.

II. *Vivianite from Bodenmais.*—1·962 were treated as stated at *c*, when 0·174 of copper was dissolved, and 0·068 foreign substances left behind. The liquid was freed from copper by means of sulphuretted hydrogen gas evaporated to dryness, the residue calcined with carbonate of soda and washed with water. The oxide of iron being redissolved in hydrochloric acid and precipitated by ammonia, amounted to 0·97; whereas the alkaline liquid, treated with the common precautions, yielded 1·185 phosphate of lime, in which 0·54956 of phosphoric acid was contained.

Considering the whole amount of iron as being present in the state of oxide, the analysis gives—

	New Jersey. (*a*)	(*b*)	Bodenmais.
Phosphoric acid	28·40		
Oxide of iron	49·74	49·56	29·01
Water	..	27·49	51·21

Or if the iron is considered as in the state of protoxide, the following will the results:

	(*a*)	(*b*)	
Protoxide of iron	44·76	44·42	46·09*

It appears that the analyses of Laugier and Thompson coincide most with the above, but the latter only also relative to phosphoric acid.

The quantity of oxide of iron amounts in the vivianite of

New Jersey (*c*) to 12·06 per cent.
Bodenmais .... 11·60 ,,

Hence the true composition is as follows:—

	V. of New Jersey. (*a*)	(*b*)	V. of Bodenmais.
Phosphoric acid	28·40	..	29·91
Protoxide of iron	33·91	33·98	35·65
Oxide of iron	12·06	12·06	11·60
Water	..	27·49	..

or as a mean—

Phosphoric acid	28·60	16·02
Protoxide of iron	34·52	7·67
Oxide of iron	11·91	3·57
Water	27·49	24·43
	102·52	

On comparing the quantities of oxygen they are found to be approximatively in the ratio of 5:2, 1:7. This, however, seems not to be accurate; for, though it may be assumed that the oxide of iron contains double the amount of the oxygen of the protoxide (as an intermediate proportion between $\dot{\dot{F}}e$ and $\dot{\dot{F}}\dot{e}$ is not likely to exist), yet the phosphoric acid manifestly contains only 4½ times the amount of the oxygen of the oxide of iron.

On calculating the formula for the vivianite, it must not be forgotten, that this mineral is isomorphous with the *kobalt blüthe*, of which Kersten's latest analysis proved to evidence the following formula to be correct:—$\dot{C}o^3\ddot{A}s + 8\dot{H}$. The vivianite is therefore, in its original state likewise $\dot{F}e^3\ddot{P} + 8\dot{H}$; we, however, find it commonly, in the state of spurious crystals, yet offering all the characters of real crystals (for instance, complete faltbarkeit). A portion of the salt of the protoxide became higher oxidated; hence the blue color of the mineral, which, as the impure earthy variety shows, was originally white. Completely the same was the case with the artificially prepared phosphate of protoxide of iron, which, owing to its loose aggregated state, more rapidly still passes over into the ferroso-ferric double salt; and, as will appear from the investigation below, has quite the same composition as the vivianite, with the sole exception that it contains a greater amount of the oxide salt. The formula of the vivianite, therefore, must manifestly account for the process having taken place in the said oxidation, which is fully obtained, if the mineral is composed of 3 atoms of oxide of iron, 18 atoms of protoxide of iron, 8 atoms of phosphoric acid, and 56 atoms of water. The quantity of the latter liquid is, in both the phosphates, found to be alike, and is also identical with that of the artificially prepared salt. From the motives

* Here, and in the following, the calculations are according to the late corrected atomic weight of iron = 34·98.

just stated, the following is the expression of the vivianite :—

$$6(\dot{Fe}^3\dddot{P}+8\dot{H}) + (\dot{Fe}^3\dddot{P}^2+8\dot{H})$$

and the calculated composition is—

Phosphoric acid	29·10
Protoxide of iron.....	33·00
Oxide of iron	12·22
Water	25·68
	100·

The excess of the analytical medium is, therefore, to be attributed to the protoxide of iron, which may be owing to imperfections of the analysis, and to the circumstance, that in the interior of the substance the change has not been completely operated.

Since, as will soon be found, the artificially prepared combination differs from the vivianite merely by the fact that it contains but two atoms of the salt of the protoxide, and yields perhaps but this *one* blue combination, which is likewise contained in the vivianite, its formula may be expressed as follows :—

$$4(\dot{Fe}\dddot{P}+8\dot{H}) + [2(\dot{Fe}^3\dddot{P}+8\dot{H}) + (\dddot{Fe}\dddot{P}^2+8\dot{H})]$$

From this it will appear that the phosphate of iron, $\dddot{Fe}^3\dddot{P}^2+8\dot{H}$, originated from $\dot{Fe}^3\dddot{P}+8\dot{H}$. Two atoms of the last combination exchanged half of their water for three atoms of oxygen, as shown by the following :—

$$2(\dot{Fe}^3\dddot{P}+8\dot{H}) = \dot{Fe}^6O^6+2\dddot{P}+16\dot{H}$$

$$\dddot{Fe}\dddot{P}^2+8\dot{H} = \dot{Fe}^6O^9+2\dddot{P}+8\dot{H}$$

FERROSO-FERRIC PHOSPHATE.

As is known phosphate of soda $(\dot{Na}^2,\dot{H})\dddot{P}$, throws down from a solution of protoxide of iron a white precipitate, which on exposure to the air becomes blue. If for two atoms of the crystallised sulphate of iron one atom of phosphate of soda is employed, that is in the proportion by weight of 3:4, the liquid separated from the precipitate is found to be colorless, but of a strong *acid reaction*, whereas the iron salt is much less acid, and the soda salt highly alkaline. The liquid contains much phosphoric acid, but at the same time some protoxide of iron.

This behavior shows that the precipitate cannot be composed, as was believed hitherto, of $\dot{Fe}^2\dddot{P}$; but that it consists of $\dot{Fe}^3\dddot{P}$, and it is the same in this case as in that of the silver salt, which was known long ago.

On the filter the precipitate becomes very rapidly blue, and after being dried in the air it represents a vivid blue powder, which cannot be distinguished from the so-called blaueisenerde (vivianite).

In the water-bath it loses already a portion of its water, and becomes green.

(*a*) 1·573, after being dried in sulphuric acid, were dissolved in hydrochloric acid, precipitated with ammonia and hydrosulphuret of ammonia, and digested. The sulphuret of iron furnished 0·763 oxide of iron = 48·5 per cent. of the combination. By chloride of calcium 1·125 phosphate of lime were precipitated, which contained 0·5121 phosphoric acid = 32·55 per cent. of the combination.

(*b*) 1·773, after being dissolved in the above manner, and observing the usual precautions, were precipitated by sulphuretted hydrogen, when 0·096 sulphur = 0·4669 oxide of iron was obtained, corresponding to 26·33 per cent. of the salt.

(*c*) 1·541, after being boiled with hydrochloric acid, and a weighed quantity of pure copper, until the reduction was operated, dissolved 0·326, which correspond to 0·41176 of oxide of iron = 26·72 per cent.

(*d*) 2·019, after being calcined in the air, lost 0·427 = 21·15 per cent., and were converted into a brown powder. The latter being calcined with carbonate of soda gave 1·009 oxide of iron = 49·97 per cent., and 1·322 phosphate of lime, in which 0·60177 phosphoric acid = 29·85 per cent. of the combination.

(*e*) 1·2, treated as stated in *c*, yielded 0·243 of copper dissolved = 0·30693 oxide of iron = 28·58 per cent.

This gives the following composition, assuming 25·58 per cent. oxide of iron as the most probable number :—

	(*a*)	(*b*)
Phosphoric acid.....	32·55..	29·85
Protoxide of iron	20·63..	21·95
Oxide of iron.....	25·58..	25·58

or as a mean—

Phosphoric acid.....	31·20..	17·48
Protoxide of iron	21·29..	4·73
Oxide of iron.....	25·58..	7·67
Water	21·93..	19·50
	100·	

The quantities of oxygen are almost in the ratio of 20 : 6 :: 9 : 24, hence the combination is formed of 4 atoms of phosphoric acid, 6 atoms of protoxide of iron, 3 atoms of oxide of iron, and 24 atoms of water, which may be expressed by—

$$2(\dot{Fe}^3\dddot{P}+8\dot{H}) + (\dddot{Fe}^3\dddot{P}^2+8\dot{H}),$$

which, therefore, must contain—

Phosphoric acid	29·83
Protoxide of iron	22·55
Oxide of iron	25·06
Water	22·56
	100·

This artificial combination differs from the vivianite, by the fact that the latter, with an equal amount of the salt of peroxide, contains three times as much of the salt of the protoxide.

Both salts, on being moderately ignited in hydrogen gas, yield nothing but phosphate of protoxide of iron.

If phosphate of soda be added, drop by drop, to a solution of sulphate of protoxide of iron, some time elapses before a permanent precipitate is thrown down. On filtration, and on being allowed to stand exposed to the air, the liquid, containing a great excess of the iron salt, becomes turbid, and deposits a yellowish-white pulverulent precipitate, which, after being washed, suffers no alteration of color, and presents the salt which will presently be described, and which may be expressed by $\dddot{Fe}\,\ddot{P}$, as appeared by the quantitative determination of both, and of that of the oxide of iron by metallic copper. Perhaps in the beginning $\dot{Fe}^2\,\ddot{P}$ are formed, which, in the air, combines with 1 atom of oxygen, and passes over into $\dddot{Fe}\,\ddot{P}$.

PHOSPHATE OF OXIDE OF IRON.

Though the properties of this salt have likewise been ascertained with accuracy, yet, relative to its composition, erroneous notions have been entertained hitherto. If, therefore, a neutral solution of oxide of iron—for instance, that of the beautifully crystallised sulphate of oxide of iron—and ammonia be precipitated by phosphate of soda $(Na^2\,\dot{H})\,\ddot{P}$, the colorless filtered mass is of an acid reaction, notwithstanding the excess of phosphate of soda contained in it, from which it appears that the precipitate, contrary to what has been generally believed in its composition, does not correspond to phosphate of soda, thatis, $\dddot{Fe}^2\,\ddot{P}^3$, but must be basic to a greater extent.

The precipitate was very hygroscopic, and was dried in vacuô over sulphuric acid. 1·272 on calcination lost 0·257 water, and left behind an anhydrous salt with a brown-red color. This was decomposed by calcination with carbonate of soda, and the process continued in the usual way, by which were found 0·533 oxide of iron, and 1·02 phosphate of lime, and in the latter 0·4643 phosporic acid.

In 100 parts were, therefore, contained—

		Oxygen.
Phosphoric acid..	36·50	20·45
Oxide of iron....	41·90	12·57
Water	20·20	17·95
	98·60	

As these quantities of oxygen are in the ratio of 5 : 3 : 4, the salt is

$$\dddot{Fe}\,\ddot{P} + 4\,\dot{H},$$

and its composition calculated—

Phosphoric acid	38·10
Oxide of iron	32·69
Water	19·21
	100·

It is not improbable that the salt has lost some water in vacuô, though the amount of the latter without previously drying, in hygroscopic powders, like this, cannot be accurately determined, if, as is most probable, 5 atoms of water are supposed, the combination must contain—

Phosphoric acid	36·35
Oxide of iron	40·73
Water	22·92
	100·

BASIC PHOSPHATE OF OXIDE OF IRON.

By the action of alkalies, one portion of acid is subtracted from the phosphate of oxide of iron, and thus two basic salts originate.

A. The salt is dissolved in the least possible quantity of hydrochloric acid, supersaturated with ammonia, and the whole heated for some time: in this manner a bright brown basic salt of a mucilaginous appearance is obtained, which after being dried presents a powder of the same color.

This combination was analysed in a similar manner to that described above.

1·517 lost on calcination 0·415 of water; the brown-red residue, on being fused with carbonate of soda furnished 0·687 of oxide of iron. The phosphoric acid in this case could decidedly be best calculated by the loss.

100 parts, therefore, contain—

		Oxygen.
Phosphoric acid..	27·36	15·33
Oxide of iron....	45·29	13·58
Water	27·35	24·31
	100·00	

The proportions of oxygen are as 10 : 9 : 16, hence the salt is

$$\dddot{Fe}^3\,\ddot{P}^2 + 16\,\dot{H}, \text{ or } 2\,(\dddot{Fe}\,\ddot{P} + 5\,\dot{H}) + \ddot{Fe}\,\dot{H}^6.$$

The calculated composition, therefore, must be

Phosphoric acid	27·11
Oxide of iron	45·55
Water	27·34
	100·

In the foregoing salt, therefore, one-third of the acid has been subtracted by the action of ammonia.

If the salt just described, or the phosphate of iron obtained by precipitation, be for some time digested with a diluted solution of potassa, and boiled, they offer the appearance of the hydrated oxide of iron, without, however, as is known, being freed from phosphoric acid. The result of the analysis seems to indicate that this substance is a basic salt, of a definite composition, which, by an excess of potassa, undergoes no further decomposition.

I investigated them only in the calcined state, hence in the anhydrous condition.

0·21 fused with carbonate of soda, the residue being washed and dissolved in hydrochloric acid, and then precipitated by ammonia, gave 0·198 oxide of iron, hence the combination contains—

		Oxygen.
Phosphoric acid..	5·71	3·2
Oxide of iron....	94·29	28·3
	100·	

The oxygen of the acid and base in this case is as = 1 : 9, the substance contains, therefore, 1 atom of acid combined with 15 atoms of base; it is $\dot{Fe}^{15}\dddot{\underline{P}}$, and its calculated composition is—

Phosphoric acid	5·68
Oxide of iron	94·32
	100·

PHOSPHATE OF LIME.

In the above described experiments I had, of course, frequent opportunities afforded me of throwing down the phosphoric acid from a liquid previously acidulated by hydrochloric acid, containing soda and phosphoric acid, by means of a mixture of chloride of calcium (a solution of the crystallised salt) and ammonia, this mixture being kept for some time, and the ammonia employed rather in a considerable excess; the precipitate was to be calcined, and then dissolved in hydrochloric acid, decomposed with alcohol and sulphuric acid, to ascertain the amount of phosphoric acid contained. By this process its composition was invariably found to be the same; it gave in 14 experiments the following amounts of phosphoric acid :—

45·54 per ct.	45·55 per ct.	46·20 per ct.
45·19	45·64	45·07
46·14	46·37	45·63
45·09	46·03	45·37
44·09	47·10	

Mean ..... 45·71 per ct.

It therefore clearly appears that it was always $\dot{Ca}^3\dddot{\underline{P}}$, a combination which contains 54·48 Ca + 45·52 $\dddot{\underline{P}}$, and in the determination of phosphoric acid instituted in the manner above, the lime precipitate needs not be separately analysed.

An experiment instituted with this view showed that this salt, in the air-dried state, contains in two atoms of water: 2·06 grms. lost, after calcination, 0·208 water, and gave 2·442 sulphate of lime = 1·0142 lime, or in 100—

		$\dot{Ca}^3\dddot{\underline{P}} + 2\dot{\underline{H}}$.
Phosphoric acid..	40·67	40·83
Lime	49·23	48·88
Water.....	10·10	10·29
	100·	100·

Notwithstanding this composition has invariably been found to be the same, yet the phosphoric acid could not be determined with the same degree of accuracy, which circumstance is owing to the solubility of the lime salt in liquids containing salts of ammonia. I found that 0·979 of the calcined precipitate, after solution in hydrochloric acid, and precipitation with ammonia, gave only 0·931; by treating the liquid with oxalic acid, 0·05 carbonate of lime = 0·028 lime was obtained, and after evaporation, &c., a residue was left behind of 0·018 phosphoric acid; both these added to the precipitate, gave 0·977, almost exactly equal to the original amount. Hence nearly five per cent. of the precipitate remained in the liquid, from this experiment; however, we can draw no conclusion relative to the solubility in general.

But the result is found to be different, if a pure solution of $\dot{Na}^2\dddot{\underline{P}}$ (pyrophosphate) is precipitated by a mixture of chloride of lime and ammonia. The precipitate is then $\dot{Ca}^2\dddot{\underline{P}}$.

2·463 grammes of calcined phosphate of soda treated in this manner, furnished 2·362 lime salt (in a calcined state), and the latter on being decomposed with alcohol and sulphuric acid gave 2·543 sulphate of lime = 1·0461 lime. Hence the soda salt contained 53·42 per cent. phosphoric acid ($\dot{Na}^2\dddot{\underline{P}}$ =

53·3 per cent. phosphoric acid); and the lime precipitate consisted of—

		$\dot{Ca}^2\,\dddot{P}$.
Phosphoric acid..	55·71	55·62
Lime	44·29	44·38
	100·	100·

Berzelius showed long ago that $\dot{Ca}^2\,\dddot{P}$ is likewise produced, if chloride of calcium be added drop by drop to a soda salt. If it be, however, proceeded with in a manner the reverse of this, and if it be precipitated only so long as the liquid continues to be of an alkaline reaction, or if the lime salt be added to a solution of phosphate of ammonia mixed with free ammonia, there will be produced—

$\dot{Ca}^8\,\dddot{P}^3 = 2\,\dot{Ca}^3\,\dddot{P} + \dot{Ca}^2\,\dddot{P}$, which combination contains 48·45 per cent. of acid. Berzelius has corroborated these facts by some recent experiments, and has found, that the just mentioned salt, when once formed, is by an excess of chloride of calcium, only with great difficulty converted into $\dot{Ca}^3\,\dddot{P}$, invariably yielding by analysis above 45 per cent. of phosphoric acid. Berzelius obtained $\dot{Ca}^3\,\dddot{P}$ by throwing a mixture of ammonia and phosphate of ammonia into an excess of chloride of calcium. $\dot{Ca}^2\,\dddot{P}$ is crystalline, the two basic salts are gelatinous, and $\dot{Ca}^3\,\dddot{P}$ is particularly mucilaginous, like the precipitated fluoride of calcium. —nt—

ON THE PREPARATION, PROPERTIES, AND COMPOSITION OF VALERIANIC ACID AND ITS SALTS.*

BY DR. G. C. WITTSTEIN.

By introducing valerianic acid into medicine the store of medicinal substances has received a valuable addition, and the old fame of the indigenous baldrian confirmed again. With reference to the preparation of the said acid, as is known, we may not only extract it as a natural constituent from the baldrian, but also obtain it as an artificial product in various chemical reactions on organic bodies—for instance, by the action of caustic potassa on the fousel-oil of potatoes. Taking it, however, for granted, that, in a therapeutical point of view, the artificially prepared acid is identical with the natural one (which is yet to be demonstrated), it still seems to me that, until now, it is rather preferable to prepare the acid from the natural material.

Preparation.—I introduce 20 pounds (1 pound = 18¾ ounces) of the officinal baldrian root, previously dried, and cut into fine shreds, with 100 pounds of water, into a copper distilling vessel, and distil off 30 pounds. A florence flask may serve as a recipient not to lose the ethereal oil, which almost always is obtained from 20 pounds of root to the amount of 3—3¼ of an ounce. On the residue in the distilling vessel I again throw 30 pounds of water, draw off as much, and repeat this alternate addition and subtraction once more. The root is now exhausted to such an extent that the mass obtained from it by a fresh distillation reddens litmus, either only as a trace or not at all. The three first distillations, therefore, may suffice for obtaining the whole of the valerianic acid. On examining the contents of the distilling vessel, it is found, indeed, that the brown extract-liquor contained in it is of a strong acid reaction; but I have ascertained that this property is not owing to the valerianic acid, but that it originates from another fixed (organic) acid, which is also met with in baldrian. It appears from this, that even though the valerianic acid may be contained in the root, fixed by a base, no particular addition of an acid is required on distilling it (as has lately been recommended by a French chemist*), as the root by itself may set free the acid.

The three aqueous distilled liquors, after the removal of the ethereal oil, are saturated with carbonate of soda, of which, according to the quality of the root, from 3½ to 6 ounces are required, and then evaporated in a copper pan until a few pounds remain, afterwards filtered, and evaporated to dryness in a weighed porcelain capsule, in a water-bath. The capsule is weighed again, to ascertain the quantity of the valerianate of soda; the latter is dissolved in an equal weight of water; the solution poured into a retort, which may contain from six to eight times that amount; and to this a mixture of one portion of sulphuric acid and two portions of water is added, in a manner that to five parts of dry valerianate of soda four parts of concentrated acid correspond. The soda immediately combines with the sulphuric acid, and forms the acid sulphate of soda; whereas the liberated valerianic acid floats like a brown oil on the surface of the aqueous solution of the soda salt. On distilling in a water-bath almost to dry-

* Buchner's *Repertorium*, xxxvii. 3.

* Rabourdin, *Journal de Pharm. et de Chimie*, October, 1844, p. 310.

ness, the valerianic acid in conjunction with the greatest part of the water passes over into the receiver. The latter vessel is evacuated into a separating funnel, or a high small cylinder, and after having been allowed to stand for some time, the oily acid is separated from the watery portion. The oily acid thus obtained amounts to from 13 to 24 drachms; the watery portion, being a concentrated solution of the acid, is likewise preserved.

The acid obtained in this manner contains 3 M. G. water (*vide* below), and is not yet of the highest degree of concentration. To arrive at this, it must be distilled again, and as soon as the milky liquid ceases to pass over, limpid drops are seen to fall; the receiver is then changed. That which now passes over is the simple hydrate of the acid. For medicinal purposes this rectification seems to be superfluous. That which will be stated in the following with reference to valerianic acid relates to its terhydrate.

Properties.—Valerianic acid is a colorless, pale, yellow liquid, similar to the ethereal oils, of a penetrating odor, resembling the ethereal oil of valerian, yet differing from it, by being still more disagreeable, and reminding one of the odor of mouldy cheese, offering a very acrid, acid, nauseous taste, similar to baldrian. It floats on water, $\frac{1}{28}$th, however, is dissolved by the latter liquid. The solution is of a strong acid reaction; the taste is in proportion to the concentrated acid contained, but mild, and at the same time somewhat saccharine. Its combination with several bases—for instance, with soda—has a sweet saccharine taste, with an admixture of that of baldrian. In strong alcohol and ether it dissolves in all proportions. At a higher temperature it is entirely volatilised. By heating it in a platinum spoon it may easily be inflamed, and burns then with a luminous flame. Its elementary constitution in the anhydrous state, and in that of its combinations with bases, has already been satisfactorily ascertained by Ettling and Tromsdorff, and answers to the formula—C^{10} H^9 O^3. The most concentrated, free acid, is the simple hydrate; hence—C^{10} H^9 $\overline{O^3}$—HO. It remained, therefore, to examine the quantity of water of the unrectified acid—hence, containing the greatest possible amount of water—and which is obtained by the distillation of a solution of valerianate of soda with dilute sulphuric acid.

With this intention I separated the oily liquid from the water, and immediately after supersaturated 60 grammes of the former with a solution containing 30 grammes of the calcined carbonate of soda, evaporated the liquid to dryness in a small weighed capsule, and exposed the salt to a heat below of 210°F., until its weight ceased to lessen. Owing to the eagerness with which this salt attracts moisture, it was weighed while still hot. I obtained 65·5 grammes. On treating with alcohol of 90 per cent., 3·4 grammes carbonate of soda remained; hence, 62·1 grammes were dissolved, and present neutral valerianate of soda. These 62·1 grammes contained 46·63 grammes anhydrous valerianic acid, and 15·57 grammes soda, and answer to 26·56 grammes anhydrous carbonate of soda. Hence, the 60 grammes of the acid employed in this operation are composed of 46 grammes acid and 13·37 grammes water, and its composition is—

	In 100 parts.	Atoms.	Calculated.
Acid	77·718	1	77·659
Water ..	22·282	3	22·341
	100·000		100·000

Formula—C^{10} H^9 $\overline{O^3}$ + 3HO atomic weight 1504.

Of the valerianates, the following have been received into medical practice:—The valerianate of quinine, valerianate of iron, and valerianate of zinc.

VALERIANATE OF QUININE.

Preparation.—One part of the oily valerianic acid is to be dissolved in 60 parts of water in a glass flask, and then 3 parts of pure quinine added (freshly precipitated); the whole is heated until it nearly reaches the boiling point, filtered while still hot, and the mass thus obtained allowed to cool. The crystals formed after some days are freed from the supernatant liquid, which latter is again evaporated at a temperature, however, not higher than 122°F., and by applying a gentle heat the whole salt is allowed to dry. About 5 parts of the salt are obtained in this manner. The aqueous solution of the acid receives the quinine with great facility—namely, 2 M.G. of the latter.

If, by evaporating, the temperature is raised above the point stated, oily drops are formed, which solidify on cooling, representing amorphous, resinous masses, containing less water than the crystallised salt, and become almost insoluble in water.

Properties.—The salt crystallises in colorless, oblique rhomboïdal tables of the lustre of mother-of-pearl, or in white opaque needles, grouped in the form of stars, exhaling a feeble odor of valerianic acid, with a bitter taste, leaving behind a sensation reminding one of baldrian. It is constant in the air. On applying heat it becomes tenacious, fuses very easily into a

colorless liquid, and, after yielding a portion of its water, it is converted into the hydrate, which is observed to appear in oily masses by boiling the aqueous solution; on raising the heat, white vapors are still formed; it becomes inflamed and burns, without leaving any residue. 1 part of the crystallised salt is dissolved in 110 parts of cold and 40 parts of boiling water, and in 6 parts of cold and in 1 part of boiling alcohol of 80 per cent.; and very easily in ether. Of the amorphous variety of the hydrated salt water dissolves scarcely $\frac{1}{1000}$th part; it, however, may impart to this latter liquid a bitter taste; but alcohol and ether act with the same facility on the amorphous as on the crystallised salt.

To ascertain the quantitative proportions of the constituents, 20 parts of the well-formed crystals were dissolved in water, and precipitated by caustic soda ley. The precipitate (hydrated quinine) weighed 10·84 grammes, containing 10·271 of grammes pure quinine. The alkaline ley was separated from the precipitate, and a current of carbonic acid gas directed through it, to convert the caustic soda contained in it into a carbonate, and the liquid was evaporated to dryness. The remaining salt weighed 6·20 grammes; after having treated with alcohol of 90 per cent., 220 grammes of carbonate of soda were left; hence, 4·0 grammes of valerianate of soda were dissolved. These 4·0 grammes consist of 2·996 grammes of valerianic acid and 1·004 grammes soda. If, after deducting the acid and the quinine from 20 grammes the remainder be considered as water, we obtain 6·733 grammes for the latter liquid, or 100 parts of the salt consist of—

	Found.	Atoms.	Calculated.
Quinine.....	51·355	2	51·411
Valerianic acid	14·980	1	14·719
Water	33·665	24	33·870
	100·000		100·000

Formula: $2\,\overset{+}{C}H+\overline{V}a+4\,\overline{H}O+20$ Aq.
Atomic Weight: 7936.

The 20 grammes of water, will be found to be water of crystallisation.

The resinous body was separated from the aqueous solution of the salt by boiling, then well rubbed and dried at a gentle heat. 20 grammes of it treated in this manner, furnished 15·16 grammes of hydrate of quinine = 14·371 grammes of pure quinine; moreover, 5·4 grammes of valerianate of soda = 4·045 grammes valerianic acid. Hence remain 1·584 grammes for the water; and 100 parts of the resinous body consist of—

	Found.	Atoms.	Calculated.
Quinine.....	71·855	2	71·629
Valerianic acid	20·225	1	20·505
Water	7·920	4	7·866
	100·000		100·000

Formula: $2\,\overset{+}{C}h+\overline{V}a+4\,\overline{H}O$.
Atomic weight: 5696

The two analyses of the crystallised and the amorphous valerianate of quinine, with regard to their relation between base and acid resemble the officinal sulphate of quinine, but do not agree with the formulæ,

$\overset{+}{C}h+\overline{V}a+\overline{H}O+$Aq. and $\overset{+}{C}h+\overline{V}a+\overline{H}O$,

which have been assigned to these combinations by the Prince de Canino.* I could not at first otherwise account for this difference but by supposing that the salt employed in his experiment was stoechiometrically pure; but this seemed unlikely, as both our salts exhibited the same physical properties. Now, even though I admit a difference in the amount of water (which in this case is rather considerable) it is still true that valerianic acid may combine with 2 M. G. quinine, to form a soluble salt; and, supposing that the Prince de Canino assumed the M. G. of the quinine (according to Regnault) double as high as has been done by myself, viz., $\overset{+}{C}h = C^{40}\,H^{24}\,N^2\,O^3$ (instead of $C^{20}\,H^{12}\,N\,O^2$). The quantities of water however, which he found, are certainly too low.

VALERIANATE OF IRON.

Preparation.—Five parts of oily valerianic acid and 60 parts of water are put into a porcelain capsule, and saturated with carbonate of soda; the liquor is to be boiled until the whole carbonic acid is expelled, and allowed to cool. After this three parts of the crystallised chloride of iron ($Fe^2\,Cl^3+6\overline{H}O$) are dissolved in 100 parts of water, in a glass cylinder, and then of the previously well cooled above solution of valerianate of soda so much is added as produces a turbidness. The precipitate is washed in a filter with a very little water, and dried in the air, at a temperature under 68°F. It will weigh about 5 parts. It must neither be precipitated nor dried at a higher temperature, as otherwise the precipitate would lose part of its acid; nay, the combination has so little stability, that if the precipitate be for some time boiled with the supernatant li-

* *Vide* Buchner's *Repertorium*, vol. lxxxvi. p. 387.

quid, the whole acid is subtracted, and nothing but oxide of iron remains. For the same reason a long-continued washing must be avoided.

The liquid filtered off from the precipitate contains still a portion of valerianic acid, which may be re-obtained by distillation.

Properties.—It is of a dark brick-red color, presenting a very loose, amorphous powder, which emits a feeble odor of valerianic acid, and slightly tastes like the said acid. On being gently heated without fusing, it loses the whole acid; on raising the heat rapidly, however, it fuses, and the acid is volatilised in the form of thick inflammable vapors, without undergoing any decomposition, as the vapors exhale scarcely any odor of valerianic acid, but they diffuse the characteristical odor of butyric acid; in the residue remains pure oxide of iron. By cold water, like lycopodium, it does not become moistened; by boiling water the whole of its acid is gradually extracted, and pure oxide of iron is left. In hydrochloric acid the preparation is easily soluble; the diluted solution, if it contain not too much acid, is not rendered blue by ferrocyanide of iron; the oxide of iron is, therefore, not partially reduced to protoxide of iron, by the valerianic acid.

To analyse the combination, 20 grammes were boiled with a solution of 10 grammes of the anhydrous carbonate of soda. The precipitate thus obtained on being washed, dried, and calcined, weighed 5·40 grammes. The filtered liquid, being of an alkaline reaction, was evaporated to dryness, and the salt treated with strong alcohol, when 1·9 grammes remained undissolved. Hence 8·1 grammes carbonate of soda were converted into valerianate of soda; these answer to 78·95 grammes of valerianate of soda, in which 14·2 grammes of acid are contained. If, on deducting from 20 grammes of the oxide of iron the valerianic acid the loss be considered as water, the precipitate consists, in 100 parts, of—

	Found.	Atoms.	Calculated.
Oxide of iron..	27·00	3	26·315
Valerianic acid	71·00	7	71·720
Water.....	2·00	2	1·965
	100·00		100·000

Formula: $3\,\dddot{\underline{Fe}} + 7\overline{Va} + 2\dot{\underline{H}}$ or $7\,(\dddot{\underline{Fe}} + 3\overline{Va}) + 2(\dddot{\underline{Fe}} + 3\dot{\underline{H}})$. Atomic weight: 11·400 or 34·200.

The precipitate is, therefore, a basic salt, of which 2 M.G. acid, required to render the salt neutral, passed into the solution, and have been displaced by 2 M.G. water.

VALERIANATE OF ZINC.

Preparation.—One part of valerianic acid is dissolved in 180 parts of water in a glass flask, one-half part of carbonate of zinc is added (in the freshly precipitated state), digested for some hours, at a very gentle heat, filtered after having been allowed completely to cool, and the filtration promoted by applying a gentle heat until crystals are obtained. During evaporation the salt separates on the surface in white snowy aggregated masses, which may easily be removed by the means of a spoon. If the liquor is allowed to boil, the whole salt is not obtained in this beautiful loose state; but then one portion of it tenaciously adheres to the bottom of the capsule. The quantity obtained amounts to somewhat more than the acid employed.

Properties.—It crystallises in snowy white lamellæ, resembling borax, of the lustre of mother-of-pearl, being very light, stable in the air, emitting a feeble odor of the acid, it possesses an acrid metallic taste, which at the same time reminds one of that of valerianic acid. On being heated in a platinum spoon, it fuses, disengages thick white vapors, becomes ignited, burns with a bluish-white flame, and finally leaves pure oxide of zinc, but not the whole quantity employed, as one portion of it is carried off by the reduction.

At the common temperature it is dissolved in 160 parts of water, and in 60 of alcohol of 80 per cent.; the solutions are of an acid reaction, are rendered turbid by the application of heat, but on cooling become limpid again. The salt, therefore, possesses the rare property of being easier soluble in water and alcohol when cold than it is when these liquids are hot. Ether, however, differs in that respect, when cold it dissolves $\frac{1}{1000}$, but when boiling it dissolves $\frac{1}{20}$th of the salt.

The analysis of the salt was instituted wholly in the same manner as the valerianate of iron. Twenty grains furnished 5·9 grains oxide of iron and zinc and 14·0 grains of valerianic acid; 100 parts of the salt consist, therefore, of—

	Found.	Atoms.	Calculated.
Oxide of zinc ..	29·50	1	30·08
Valerianic acid..	70·00	1	69·92
	99·50		100·00

Formula: $\dot{Zn} + \overline{Va}$. Atomic weight: 1672. The salt contains, therefore, *no* water.

—*nt*—

SACCHARIC ACID.*

BY M. HEINTZ.

He has succeeded in preparing this acid, without any difficulty, in a state of purity, by treating one part of sugar with three parts of nitric acid of 1·25 sp. gr. and not heating it higher than 122° F. By attending closely to the temperature, no trace of oxalic acid is formed. After the reaction is completed, the mixture is saturated with carbonate of potash, then acetic acid is added until the mass smells of it, when the slightly soluble bisaccharate of potash is left undissolved, and if dried between blotting paper, and recrystallised, it can be obtained perfectly pure; from this the saccharate of cadmium is formed, and decomposed by hydrosulphuric acid to furnish the saccharic acid (the lead salt will not answer for this purpose). The acid can be obtained as a brittle mass, by evaporation and desiccation in a vacuum with sulphuric acid.

ON AQUA REGIA; ON HYPOSULPHURIC ACID AS A MEANS OF OXIDATION; ON THE CONSTITUTION OF THE SAME ACID AND THE PART IT ACTS ON BEING PUT IN CONTACT WITH ORGANIC SUBSTANCES.†

BY DR. KŒNE,

PROFESSOR OF CHEMISTRY, AT BRUSSELS.

1. Aqua Regia may serve as one of the most powerful agents either for oxidising, acidulating, or dissolving a great number of simple and compound bodies. Important, however, as this substance is, on account of the above uses, and interesting as are the phenomena attending its action, the nature of the same is still but incompletely known.

2. Berthollet attributes the properties of aqua regia to the formation of chlorine and nitrous acid.

3. This hypothesis was adopted by Sir H. Davy, who, after having mixed hydrochloric acid with nitrous acid, obtained a liquid, which did not possess the properties of aqua regia.

But in the days of Davy hyposulphuric acid was not known. The English chemist, therefore, may have operated with this acid, which seems the more likely, as several French chemists, at the instance of Professor Dumas, regard this acid as a compound radical, as an oxidising agent less powerful than nitric acid, capable of replacing hydrogen in some organic substances, which contain more than one equivalent of this metalloïd (hydrogen).

4. Finally, Millon considers nitrous acid as the most powerful oxidising agent of all the stages of oxidation of nitrogen—an hypothesis which leads to the assertion that the hydrogen of hydrochloric acid may destroy the nitrous acid, as the same metalloïd may deoxidise nitric acid.

5. We have, therefore, to ascertain whether hydrochloric acid reduces nitric acid to hyponitric acid, to nitrous acid, or to binoxide of nitrogen.

6. If it could be proved that nitrous acid, of all the three oxyacids of nitrogen, is the most powerful oxidising agent, we might give preference to the latter hypothesis. But, recollecting the observations made by Davy (3), and considering the oxidising power of the hyponitric acid, the formation of which, by the reaction of oxide of nitrogen on nitric acid, precedes the production of nitrous acid, we arrive at the conclusion, that hydrochloric acid can reduce nitric acid only to the state of nitrous acid.

7. The constitution, however, of oxysulphuric and analogous acids leads us to suppose that hyposulphuric acid is a radical; but, on the other hand, the formation of hydrochloric and nitric acids by mixing water with a solution of chlorine in hyponitric acid is rather in favor of the assertion, that aqua regia is the result of a *mutual action* between the elements of the hydracid and those of the oxyacid — $(H^2Cl^2)+(N^2O^4+O)=(H^2+O)+(N^2O^4+Cl^2)$. According to which, therefore, water and oxychloro-nitric acid are formed, the latter of which is little stable, and, like analogous acids, is decomposed in the presence of water. The latter hypothesis having been preferred to the two others, it was contemplated to allow an excess of gaseous hydrochloric acid to act on concentrated nitric acid. But by the mutual action of the said acids water is formed, the quantity of this liquid, however, gradually augments and is opposed to the complete decomposition of the acids; wherefore, in order quantitatively to determine the hydrochloric acid, the elements of which combine with those of the nitric acid, it was necessary to protect the acids from the influence of water during the time of their mutual action—that is, to employ a liquid which combines with water the moment it is formed, taking care at the same time that this hygroscopical substance be of such a nature as not to exert any injurious influence on the reaction, by which the latter might be rendered incomplete.

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* *Beric Königl. Preuss. Akad.*

† Poggendorff's *Annalen der Physik und Chemie*, March, 1845.

8. Sulphuric acid may answer this purpose. This combination, *if present in great excess*, possesses, in addition to this, the property, at the temperature of the disengagement of chlorine, of retaining in solution the hyponitric acid and the nitrous acid, a property indispensable for the demonstration, as an excess of hydrochloric acid is necessary to replace the whole of the nitric acid employed, and this excess, therefore, must be determined.

9. This determination requires that the influence of the chlorine be rendered inefficient, the quantity of which in a free state, under these circumstances, cannot be determined, as the chlorine in its free state, before it can be completely disengaged, under the influence of heat, from its solution in sulphuric acid, red vapors are formed. These vapors, on entering into an aqueous solution of chlorine, would reproduce hydrochloric acid and nitric acid.

10. In the present case, therefore, the hydrochloric acid must be determined by the process which I have communicated in another article. (See p. 348).

After these preliminaries, I might pass on to the description of the apparatus I employed, the process adopted, and an infinity of precautions taken, in order accurately to determine the amount of hydrochloric acid, which was not acted upon by the nitric acid, the amount of real acid of which having been previously ascertained; the analyses, however, instituted with various quantities of substances have led to results disagreeing with the theory indicated; this description would, therefore, be useless. Although it is true that in similar analyses very considerable losses are unavoidable, it is nevertheless a fact, that if the loss amounts to more than half an equivalent of hydrochloric acid, the cause must be sought after, not in the complication of the apparatus, or of the analysis, but in some other circumstance.

11. As this cause may be supposed to be the deoxidation of the hyponitric acid induced by the action of the hydrochloric acid on it, five grammes of well-dried nitrate of lead were heated, and the vapors of hyponitric acid thus disengaged were directed into 100 grammes of pure and concentrated sulphuric acid. The gaseous hydrochloric acid gas was then passed into the acid liquid. A large quantity of chlorine was disengaged from a solution of sulphate of potassa (10). To ascertain whether the nitric acid in its turn was decomposed by hydrochloric acid, the following experiment was made:—

12. Into a solution of 100 grammes of pure sulphuric acid, and 2·1 grammes of hyponitric acid a current of hydrochloric acid was passed for half an hour, and afterwards by the application of heat the hydrochloric acid and chlorine were expelled; half of this was then again treated with a current of hydrochloric acid, but this time no chlorine was produced. A portion of the other half was mixed with one-sixth of its volume of water freed from air. This water was introduced into a flask filled with carbonic acid.* The water was acidulated with the acid, and as soon as the flask became filled it was hermetically closed. On allowing the acid to cool, two drops of hydrochloric acid were added, the vessel was then closed and shaken. After half an hour the vessel was opened, when a distinct odor of chlorine was perceived.

In this experiment, care must be taken, 1st, that the acid be added to the water, that, immediately on the addition, nitric acid may be produced; 2nd, that about six parts of acid to one of water be contained in the flask, to prevent an excess of water from counterbalancing the action of the hydrochloric acid on the nitric acid, at the moment of its formation (7).

13. The sulphuric acid previously employed in the concentration of nitric acid was heated until it arrived at a constant boiling point. The acid, on being treated as above, with water and hydrochloric acid, exhibited the same phenomena, but by an addition of hydrochloric acid alone chlorine was never formed.

14. To 150 grammes of the concentrated sulphuric acid of experiment 13, 10 grammes of red nitric acid were added. By this solution, the latter lost its intense red color; however, on heating the acid mixture in a retort, the neck of which was drawn out, and bent into a rectangle, the red vapors reappeared. Between 338 and 356° F. an effervescence took place, which seems chiefly to be derived from a disengagement of oxygen. At 374° F. the disengagement of gas became so powerful that the liquid seemed to boil; but at higher temperatures the formation of oxygen considerably diminished, and at 482° F. it ceased entirely to be disengaged. Together with the oxygen, red nitric acid was distilled over, amounting almost to one-third of the sulphuric acid added.

The moment the temperature of the acid was found to be 620° F., the neck of the retort was introduced into a vessel in which carbonic acid was continually disengaged, and the flame of the spirit lamp was gradually diminished. The acid, which, when

* The non-existence of sulphate of oxide of nitrogen not being ascertained, it was necessary to take the above precaution.

boiling, was of an intense yellow, after cooling became completely colorless.

15. Cooled down to the common temperature, the acid formed no chlorine with hydrochloric acid.

Being treated with water, as in experiments 12 and 13, on an addition of hydrochloric acid, the acid gave chlorine.

Another portion of the acid being mixed with alcohol, the air being excluded, formed nitric ether.

Copper dissolved in it, with a violet color, and a disengagement of binoxide of nitrogen.

After having adapted to the tubulure of the retort, a funnel provided with a stopcock, through this were first introduced carbonic acid, and then 26 grammes of acid. Heat was afterwards applied, and when the water was boiling the cock was opened. The temperature of the water was considerably raised by the addition of the acid. At the same time, red vapors were disengaged, and received into water with carbonate of lime suspended in it; heat was then applied until the temperature of the acid arrived at the boiling point of sulphuric acid. The liquid separated from the carbonate of lime was evaporated to dryness, and the residue was exhausted with alcohol. The alcoholic liquid, on being evaporated, in its turn, gave a residue, which, on calcination, left 0·21 grammes of lime.

These facts being established, we mean to apply them to aqua regia and hyposulphuric acid, in order more fully to ascertain the nature of these liquids.

ON THE NATURE OF AQUA REGIA.

16. Aqua regia is very peculiar in its characters and functions. Its formation is especially influenced by the temperature, by the degree of concentration of the two acids from which it is formed, and by the presence of a body capable of combining with chlorine.

17. By mixing hydrochloric with nitric acid, both being concentrated, and the temperature very low, no reaction is caused. The cold liquids are mixed without coloration or disengagement of chlorine. If, however, the mixture is removed from the refrigerating medium, and put in a place in which the temperature is 32°F., it gradually becomes colored, and chlorine is disengaged.

18. A mixture of two volumes of hydrochloric acid of 37·60 per cent., and one volume red nitric acid, forms chlorine only at a temperature above 9°F. Nitric acid, containing 74·5 per cent. of real acid, being treated with the same hydrochloric acid, is colored, and forms chlorine, at a temperature above 21°F.

The presence of a greater amount of water may still further retard the reaction. Finally, if the acids be very much diluted, even by the application of heat, no reaction is caused; if, however, in the latter case, a metal be thrown into the liquid, a reaction follows, and water is formed. Soon afterwards, this liquid diminishes the affinities to such an extent, that the reaction would entirely cease, if a metal of the sixth class,—for instance, gold—were made use of.

19. These phenomena prove that the formation of aqua regia is by no means the effect of the mutual reaction between the elements of hydrochloric and nitric acids (the latter being considered as composed of $N^2O^4 + O$), but that is to be attributed to the action of the hydrogen of the one upon the oxygen of the other. To produce the effect, it is indeed requisite that, either by the application of heat, or the use of a body having great affinity for chlorine, or these two agents jointly, the affinity of the chlorine for the hydrogen of the hydrochloric acid be reduced, and that the affinity of the same metalloïd for the hydrogen of the water be counterbalanced, to augment the dishydrating power of nitric acid. *If a double affinity predominated in this reaction* (7), *these phenomena would not be perceived, the chlorine would not be disengaged, or enter into a combination, the moment of its formation, to combine with the hyponitric acid.*

In making these remarks, we have not lost sight of the various circumstances in which a chemical action commences, the moment the affinities by which it was called forth are annihilated. We even have premised the hypothesis by which hyponitric acid is considered as a radical. We attempted to support the hypothesis, by allowing hydrochloric acid gas to act upon concentrated nitric acid, at a low temperature, protecting it at the same time against the influence of water. Instead, however, of obtaining the combination ($N^2O^4 + Cl^2$), we perceived, and demonstrated, that these acids were reduced to water, chlorine, and nitrous acid (11, 12, 14). These are the products furnished by an active aqua regia.

The causes by which these are produced may be, 1st, the striving of the chlorine to pass into the gaseous state, or to combine with metals; 2nd, the instability of the nitric acid and its dishydrating power; 3rd, the tendency of nitric acid for becoming volatilised or forming combinations.*

* M. Baudrimont says that, by putting together hydrochloric acid of commerce and nitric acid of 86° C., he obtained a gas of a

peculiar nature. This gas, which he considers as the active principle of aqua regia, would be that which corresponds to nitric acid, and be represented by the formula $N^2O^3C^2$. I know not whether M. Baudrimont, in asserting this, was not influenced by the hypothesis of Mr. E. Davy, according to which the product obtained by the action of nitric acid on fused sea-salt, would be the active principle of aqua regia.

Certain it is, that if the chloronitric acid exists, it must be possessed of peculiar properties, among others—to yield nitrous acid to sulphuric acid (*vide* the details of my experiments); *not to combine with the elements of water;* but to furnish nitrates and chlorides with the metalloïds; to attack gold, and to explode with silver reduced to powder; to act, *but slowly*, however, *on potassium;* with the powder of antimony or arsenic to produce intense phenomena of light and heat, *without producing similar phenomena with fused phosphorus (Annalen der Chemie und Pharmacie*, xlviii. p. 202, and *Traité de Chimie Générale, &c.*, par A. Baudrimont, i. p. 616).

If the French chemist, instead of having employed the raw products of commerce, had operated with dry hydrochloric acid, and if he had taken care to dry the chloronitrous gas, and to ascertain the absence of hydrochloric acid, it might have been possible to form an opinion on the merits of his work, the data of which require more minute researches.

ON HYPONITRIC ACID CONSIDERED AS AN OXIDISING AGENT.

20. We have just seen that the action of hydrochloric acid upon nitric acid becomes embarrassed as soon as the latter is reduced to the stage of nitrous acid. This is manifestly owing to the circumstance that the oxygen is in a more intimate state of combination in the latter acid, than in nitric and hyponitric acids. Nitrous acid is, therefore, the most stable, and, consequently, of the three stages of oxidation of nitrogen the least powerful oxidating agent.

This inference, drawn from the reaction between hydrochloric, nitric, and hyponitric acids placed in concentrated sulphuric acid, is far from coinciding with that which Millon concluded from his experiments. According to this chemist the process of oxidation by nitric acid is in general the following:—nitrous acid is formed first, and with this nitrite of copper, mercury, silver, &c., produced; these salts are again destroyed by the nitric acid, and this destruction giving rise to the formation of oxide of nitrogen, the latter body on coming into contact with the nitric acid reforms the nitrous acid, the metal is then attacked again by this acid, and the salt formed destroyed, and so on in succession.*

We know not, whether in the action of the metal on the nitric acid, the formation of a nitrite always precedes that of a nitrate; but it does not appear to me that the production of a nitrite by the action of an alkali on a saturated solution of oxide of nitrogen in nitric acid, permits us to accede to the hypothesis of M. Millon,† because the formation of hyponitric acid precedes that of nitric acid, and because, if the nitric acid, as is generally the case, be present in excess, only hyponitric acid is formed. Nor do I conceive how nitrous acid, one of the most powerful oxidising agents can combine with protoxide of mercury. The presence of nitrite of protoxide of mercury, the circumstances attending its formation, the property of the phosphorous acid to resist the oxidising power of the nitrous acid,‡ the formation of nitrous acid by the action of hydrochloric gas on nitric acid and hyponitric acid; the production of hyponitric acid by the action of oxide of nitrogen on an excess of nitric acid, the circumstances attending the formation of nitrite of oxide of ethyl, and the influence of heat on alkaline nitrates are all facts wholly incompatible with the hypothesis of M. Millon; and the important observations made by the author himself, prove to evidence, that of all the stages of oxidation of nitrogen the hyponitric acid is the most powerful oxidising agent.§

If the reverse were the case, if the hyponitric acid were a less powerful oxidising agent than the nitrous acid, an anomaly would occur in the oxidising powers of these stages, for the greater the amount of oxygen they contain, the more powerful oxidising agents they are. In a manner, that oxide of nitrogen, which, in the presence of iron, zinc, or phosphuretted hydrogen, is converted into protoxide of nitrogen, resists the deoxidising power of copper. Nitrous acid, which yields one-third of its oxygen to copper, in the presence of phosphorus and hydrochloric acids, undergoes no change, though either of them may convert the hyponitric acid into nitrous acid. With reference to the nitric acid, it appears, beyond doubt, to be a stronger oxidising agent than hyponitric acid, if it could only exist by itself.

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(To be continued.)

* *Comptes Rendus*, xiv. p. 912.

† *Ibid.*, p. 911.

‡ Berzelius, *Jahresbericht*, 1841, p. 31.

§ The nitric acid imagined to be N + Aq.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

NEW METHOD OF PREPARING A PHOTOGENIC PAPER.*

BY M. GAUDIN.

A SHEET of white paper is to be exposed for a minute to the vapor of hydrochloric acid, after which a nearly saturated neutral solution of nitrate of silver is to be passed over its surface by means of a brush (which must not be fastened with metal): the paper is then allowed to dry. The dry sheet is then placed in the focus of a camera obscura; on being removed no traces of the image will be visible, but upon wetting the paper with a nearly saturated solution of sulphate of iron, slightly acidified with sulphuric acid, the image will appear immediately.

In order to fix the image, it must be well washed, first with pure water, and afterwards with water containing 10 per cent. of caustic ammonia. If the parts which ought to be white have a yellow tinge, the paper must, before being allowed to dry, be washed in water acidified with hydrochloric acid.

The proofs thus obtained are reversed, as is the case with Mr. Talbot's paper; the sensitiveness of this paper is also similar to his.

In order to reproduce the reversed images thus obtained, in their true position, the same paper must be used without passing it through the sulphate of iron, and the operation must be continued until the edges of the paper surrounding the reversed image have become black.

If ammoniacal salts of silver are employed instead of the ordinary nitrate of silver, a more sensitive paper is produced; but the solution is more volatile. Nevertheless, for the production of reversed images, M. Gaudin uses a paper prepared by passing ammoniacal acetate of silver over paper impregnated with hydrochloric acid gas.

Excellent nitrate of silver may be produced with a piece of silver coin.

The inventor exposed a reversed image to the sun for two days, without its undergoing any change.

In order to reproduce the reversed images, they must be varnished, which removes the inequalities from the paper, and materially assists the operation.

The paper must be prepared by the light of a taper, but after passing the sulphate of iron over it, the operation may be concluded by daylight.

* *Bulletin de la Société d'Encouragement.*

ON THE ROTATION OF CROPS.*

AT a late meeting of the Royal Society, Professor Daubeny read a paper on this important subject. The learned professor was first led to undertake the researches by the expectation of verifying the theory of De Candolle, in which the deterioration experienced by most crops on their repetition was attributed to the deleterious influence of their root excretions.

For this purpose, he set apart, ten years ago, a number of plots of ground in the Botanic Garden at Oxford, uniform as to quality and richness, one half of which was planted each year, up to the present time, with the same species of crop, and the other half with the same kinds succeeding each other in such a manner that no one plot should receive the same crop twice during the time of the continuance of the experiments, or, at least, not within a short period of one another. The difference in the produce obtained in the two crops under these circumstances, would, the author conceives, represent the degree of influence ascribable to the root excretions.

The results obtained during the first few years from those experiments, as well as from the researches which had in the meantime been communicated to the world by M. Braconnot and others, on the same subject, led him in a measure to abandon this theory, and to seek for some other mode of explaining the falling off of crops on repetition. In order to clear up the matter, he determined to ascertain for a series of years, not only the amount of crop which would be obtained from each of the plants tried under these two systems, but also the quantity of inorganic matters extracted in each case, from the soil, and the chemical constitution of the latter, which had furnished these ingredients.

The plants experimented upon were spurge, potatoes, barley, turnips, hemp, flax, beans,

* *Agricultural Gazette.*

tobacco, poppies, buckwheat, clover, oats, beet, mint, endive, and parsley.

From a chemical examination of the crops, Dr. Daubeny concludes—

1st. That the falling off of a crop after repetition depends, in some degree, on the less ready supply of certain of the inorganic ingredients which it requires for its constitution, not but that two crops equally well supplied by the soil with these ingredients may take up different quantities of them, according as their own development is more or less favored by the presence of organic matter in the soil in a state of decomposition.

2nd. That it is possible that a field may be unproductive, although possessing abundance of all the ingredients required by the crop, owing to their not being in a sufficiently soluble form, and therefore not directly available for the purposes of vegetation; so that in such a case the agriculturist has his choice of three methods—the first, that of imparting to the soil, by the aid of manure, a sufficient quantity of these ingredients in a state to be immediately taken up; the second, that of waiting until the action of decomposing agents disengages a fresh portion of those ingredients from the soil (as by letting the land remain fallow); and the third, that of accelerating this decomposition by mechanical and chemical means.

3rd. That it is probable that in most districts a sufficient supply of phosphoric acid and of alkali for the purposes of agriculture, lies locked up within the bowels of the earth, which might be set at liberty, and rendered available by the application of the artificial means above alluded to.

4th. That the aim of nature seems to be to bring into this soluble, and, therefore, available condition, these inorganic substances by animal and vegetable decompositions, and therefore that we are counteracting her beneficial efforts when we waste the products of this decomposition by a want of due care in the preservation of the various excrementitious matters at our disposal.

5th. That although we cannot deny that plants possess the power of substituting certain mineral ingredients for others, yet that the limits of this faculty are still imperfectly known, and the degree in which their healthy condition is effected by the change is still a matter for further investigation.

6th. That the composition of various plants, as given in this paper, differs so widely from that reported by Sprengel and others, that we are supplied with an additional argument in favor of the importance of having the subject of ash analysis taken up by a public body, possessed of competent means and facilities for deciding between the conflicting authorities, and supplying us with a more secure basis for future calculations.

ON THE ASHES OF WHEAT.*

ANOTHER paper was also read "On the Ashes of Wheat," by W. Sharp, Esq. The experiments recorded were undertaken principally with the ultimate view of ascertaining with exactness what quantity of inorganic matter is removed from the soil by the seeds of a crop of wheat. The author first inquires what is the average amount of the inorganic, or incombustible portion of wheat; a question to which no satisfactory answer has yet been given. The result of his experiment is, that wheat yields by slow combustion a residue of from 1½ to 1¾ per cent. He then proceeds to determine by experiment the degree in which this result is influenced by previous drying at different temperatures, varying from 230° to 260°F., and finds that a heat of 245° is not sufficient to expel all the moisture contained in wheat; for, while the loss of weight is then about 8 per cent., by a heat of 260° the amount of this loss is 10 per cent. When the heat is so great as to occasion decomposition, the saline matter contained in the wheat fuses, and a portion of the carbon becomes so entangled, or firmly adhered to it, as to be incapable of separation by burning. Hence he recommends, in order to obtain greater uniformity in the results, that the wheat subjected to these experiments should be dried at a low temperature, such as that of a room in summer, and be allowed to remain a few days under its influence. The author tried the effect of the addition of nitric acid, with a view to save time, by accelerating the combustion, but found that the results could not be relied upon when this plan was adopted, and he was therefore obliged to relinquish it. He next directed his inquiries to the ascertaining whether the quantity of inorganic matter was in proportion to the specific gravity of the grain; that is, to its weight per bushel, and this he found in general to be the case. The conclusion he deduces from this investigation is, that the mean amount of inorganic matter removed from the soil by the grain of a crop of wheat is exactly 11b. per acre. [This only amounts to one-tenth per cent. on a crop of 20 bushels per acre. One per cent. of ash in the grain of such a crop would amount to nearly 10lbs. per acre.]

* *Agricultural Gazette.*

ON THE APPLICATION OF GEOLOGY TO AGRICULTURE.

Mr. Mac Adam, assistant secretary of the Belfast Academical Institution, lately delivered, at the Belfast Museum, the first of three lectures, on the application of geology to agriculture. The subject was, the mineral substances, most interesting to the agriculturist, which are found in the crust of the earth. The lecture commenced with a description of the elementary bodies which have been brought to light by the discoveries of modern chemistry. Of these elementary bodies, only a small number are immediately interesting to the agriculturist, viz., oxygen, hydrogen, nitrogen, chlorine, fluorine, iodine, carbon, sulphur, phosphorus, silicon, aluminum, calcium, magnesium, potassium, sodium, manganese, and iron. These substances are scarcely ever found in nature occurring in a separate form, but almost always in combination with one another. Iron is found, in great quantities, generally combined with oxygen. In the state of oxide, it occurs in almost every rock, in greater or less proportion; where strata contain much of it these are called ores, and from them the iron is procured by smelting, for the purposes of commerce. It is a necessary component of every fertile soil; and from the soil it is taken into the bodies of plants, and from them into animals, in whose systems it is a necessary ingredient, as without it it is impossible that the blood can be in a healthy state; and it is also a component of bones and hair. In the soil, oxide of iron has the property of attracting ammonia from the atmosphere, and thus assisting in the supply of that important substance to vegetation; and it also favors many unseen chemical actions that are always going forward, by which both organic matters are furnished to plants. The lecture alluded to the importance of an agriculturist possessing some knowledge of metals, as it might, in many cases, enable him to detect the existence of ores in his land; and he pointed out the variety of metals which exist in Ireland, the number of localities of which would be doubtless increased, were the requisite knowledge possessed by the occupiers of land. The metallic bases of the earth and alkalies were then mentioned; these are found combined with oxygen, so that there must be an enormous quantity of oxygen locked up in the animal crust of the globe. The importance of phosphorus was also stated. It is a necessary ingredient of animal bodies, entering largely into this composition of the bones and of different secretions; it is, therefore, a necessary ingredient of plants, and in them it abounds in many seeds, which explains the nutritious qualities of bread that is made from the seeds of grain-bearing plants. Large quantities of sulphur are dispersed through soils in a state of minute division combined with iron, forming sulphuret of iron, which is acted on in various ways, and indirectly assists greatly the progress of vegetation. There is always a constant deposition of carbon in the soil, from the decay of plants; but, independent of this, there is a large quantity of carbon locked up in the great masses of limestones, that are of such general occurrence. After enumerating these simple elementary bodies, the lecturer then described several simple minerals, previously pointing out the necessity of attending to certain external characters by which they can be recognised. He stated that, although there is a great number of simple minerals that claim the minute attention of the mineralogist, yet that the greater proportion of these are of rare occurrence, and that but a few are found in abundance, and, indeed, in such abundance as to compose nearly the whole of the earth's crust. These are quartz, from the disintegration of which sands are formed; mica, talc, felspar, and hornblende; all of which in their decomposition supply the materials of fertile soils—carbonate of lime, sulphate of lime, and a few others of indirect importance. These minerals form the large rocky masses on the surface of our globe, and from these rocky masses originate soils, which are the objects of the agriculturist's primary interest and attention. In order, therefore, to have a proper knowledge of soils, it is requisite to be acquainted with the phenomena presented by rocks; and accordingly the lecturer announced that, in the next lecture, he would enter upon the description and classification of the great mineral formations on the surface of the earth.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

MEMOIR ON THE ETHEREAL TINCTURES.*

BY M. EMILE MOUCHON.

SECOND PART.

Second Series—Alcoholic Extracts.

Experiment I. Alcholic Extract of Aconite.—Sixty centigrammes of alcoholic extract of *aconitum napellus*, collected after the preparation of the ethereal tincture, were dissolved in a few grammes of water, and the solution was given to a hen. Signs of indisposition speedily manifested themselves. The animal had much difficulty in keeping on its legs; it cackled frequently, alternately opened and shut its beak, and seemed to make efforts to reject the poison; however, one hour after taking the poison it ate tolerably well, and seemed not to suffer.

Next day, the same hen was made to take 60 centigrammes of aconite, resulting from the evaporation of the juice of the plant in an oven. Symptoms resembling a kind of strangulation were immediately manifested. As before, the animal made alternate motions of inspiration and deglutition, which indicated extreme constraint in the respiration. An hour after, the symptoms of indisposition existed in their full force, and food was refused. It was not until several hours later that it tried to eat a few grains of barley, and it was also some hours before the influence appeared to cease. In the middle of the day the aliments were taken as usual, and nothing indicated the slightest derangement in the health of this hen.

Having long been convinced that the extract of aconite, obtained from the recent plant, with the aid of the heat of an oven, is perceptibly more active than the ordinary alcoholic extract, I should give it the preference, in order to establish a rational comparison in favor of the alcoholic product, whose physiological action I was studying.

Some months after the date of the two foregoing experiments, I introduced into the stomach of a cock four grammes of the alcoholic extract of aconite which had been tried on the hen. Formidable symptoms were manifested immediately after it was swallowed, and in less than two hours the animal had ceased to live, after having uttered several sharp cries, expressive of fright. After an extreme and apparently convulsive agitation, accompanied by manifest signs of a haletante saccadee and highly accelerated respiration, a violent convulsive motion terminated this scene, impressing on the animal a kind of general and apparently tetanic raideur.

After this result, it would have been superfluous to have tried the extract of the juice of aconite, since no doubt could exist concerning its poisonous action, in the dose of four grammes, in such a case as that which I have just detailed, for that which can do more can do less.

Experiment II. Alcoholic Extract of Belladonna.—Sixty centigrammes of this extract had no influence on an adult hen; 2 grammes had such an effect on it for several hours, that I thought it was poisoned; however, these few hours having passed away, the animal, which hitherto had been affected with a kind of indisposition produced by 60 grammes of aconite juice, reduced to the extractive consistence, recovered, and ate almost as usual. No less than four grammes of this extract were required for producing mortal symptoms, nearly analogous to those which deprived the cock of his life, with this difference, that they appeared to me less formidable, and that they did not cause death until after four hours and a half. The convulsive state was less violent, but the general raideur which supervened in its last moments was nearly the same. The sharp cries which the animal also uttered at intervals, appeared to me to be occasioned by a kind of spasmodic contraction, simulating a true strangulation.

Experiment III. Alcoholic Extract of Aconite.—This extract, in the dose of two grammes, had no perceptible influence on a rabbit. The animal ate quite as well a quarter of an hour after having swallowed the extract as at a few hours later. Four grammes, administered a few days after, sensibly affected the same rabbit, which appeared enfeebled, but which, however, without having his ordinary allures, ate moderately, occasionally. The influence of the poison was felt all the next day, but not on the day after.

The same quantity (four grammes) of inspissated juice of the same plant produced general perturbation, manifested by an uninterrupted motion of the jaws and abundant baveuse secretion, a continual change of position, unequivocal signs of suffering, the almost absolute impossibility of keeping long on his legs, abdominal shocks, and efforts to

* *Journal de Chimie Médicale*, June, 1845.

reject the poison, the prolonged refusal of the aliment presented to him, &c. This state of suffering continued for 48 hours, after which time the animal began to eat, without having resumed his allures. It was not until three days after the swallowing of the poisonous substance that reëstablishment was completed, at least to all appearance.

The alcoholic extract of the shops, in the dose of four grammes, did not produce, on another rabbit, a more powerful effect than a similar quantity of alcoholic extract collected after the ethereal treatments. The remarks were absolutely the same. Thus, then, there is an identity of action between the two alcoholic extracts.

According to the foregoing, it is still the extract of the recent plant, resulting from the concentration of the juice in an oven, which showed itself most energetic.

Be it as it may, I do not doubt that 10 or 12 grammes of one of the three extracts would have caused death, the ravages caused by each of them, and especially by the second, being of a nature to render this certain, which other facts will fully justify.

Experiment IV. Alcoholic Extract of Belladonna.—Two grammes of this extract, representing nearly 16 grammes of dried plant, were introduced into the stomach of a rabbit. There was little or no poisonous effect.

It was absolutely the same with two grammes of aqueous pilula extract of belladonna.

In both cases the rabbit relished the food presented to him, and his habits remained unaltered.

Some days after, we tried to introduce into the stomach of a rabbit 12 grammes of alcoholic extract of belladonna, extracted from the plant after the passage of the ether, but he could scarcely be made to swallow half, the necessary precautions for repairing this loss not having been taken. The other half was rejected by the animal, which makes, in all cases of this kind, strange efforts to prevent the introduction of the poison. The signs which characterise poisoning are the following:—Continual motion of the jaws, perceptible and repeated shocks of the two fore legs, which it carries towards the nose, as if to expel the mortal agent which torments him. This manifest sensation of illness lasted the whole day. At night the disorder seemed to have somewhat abated, and next morning some food was taken in small quantities, although the animal was still tormented at intervals, some momentary tremors, some strokes of the teeth against the bars of the hutch, and frequent disorderly motions, denoting transient sufferings, which dissipated towards the decline of the day, and did not return.

It may be presumed that the animal would have died if it had taken the 12 grammes of extract. This is a fact which I should have been curious to verify, if I had had at my disposal a sufficient quantity of this agent, but, unfortunately, my small provision was nearly exhausted; I depended, besides, sufficiently on the preceding experiments not to be tempted to try others.

Experiment V. Alcoholic Extract of Hemlock (Conium maculatum).—A rabbit swallowed 6 grammes of extract of hemlock. Immediately after taking this poison symptoms of poisoning manifested themselves: these consisted of violent contractions of the abdominal viscera; sudden and jerking motions, to which succeeded, at moments, a state of torpor or somnolency, very great prostration, when certain signs of suffering did not determine agitation. After 12 or 13 hours, the animal passed sanguinolent urine, or, more correctly speaking, almost pure translucent blood, of a fœtid odor, resembling that of hemlock, as in the case in which 10 grammes of ethereal extract of hemlock were administered.—(See Experiment VIII. of the First Series, *The Chemist*, No. lxvii., page 327.) The night having passed away, the rabbit appeared better, and it was the same a day afterwards, at least in the morning, for in the evening it began to receive some little nourishment. The return to health did not appear nearly complete until the fourth day after this powerful experiment.

12 grammes of the same extract of hemlock caused horrible illness in another adult rabbit, which, for the time, appeared not to remember it. The same symptoms were carried to a higher degree; the fits, also, followed one another with more rapidity. The progress was such, that death appeared more and more imminent; however, it did not occur until seven hours after the poison had been taken, the effects of which were violent, and the animal died in extreme agitation. It must, however, be remarked that M. Orfila has rarely caused death with conium maculatum, although he has experimented on a great number of animals, and with large doses. What will this celebrated toxicologist think when this fact comes to his knowledge? He will, perhaps, doubt the truth of my assertions; however, nothing is more positive than what I have just related, and it proves nothing but that my alcoholic extract of hemlock is as energetic as it appears to me a product of this nature should be, whether it results from the plant

already treated by ether, like the latter, or from the same vegetable exhausted only by weak alcohol.

Experiment VI. Alcoholic Extract of Digitalis Purpurea.—4 grammes of alcoholic extract of *digitalis purpurea* perceptibly inconvenienced an adult rabbit. A few vomitings supervened a quarter of an hour after it was swallowed; a little later, a species of convulsive enfeebled state was remarked in the animal, but nothing else. An hour after swallowing these 4 grammes, a similar dose was administered; a short time after, vomitive efforts supervened; the agitation was extreme and convulsive; this agitation ceased after about an hour, giving place to a kind of momentary prostration; then the convulsive state returned, and a slight vomiting took place, and others succeeded shortly afterwards. The illness appeared extreme; however, the antiperistaltic motion appeared to give way, and the animal again fell into a state of prostration, or rather into a state of coma somnolentum, which ceased only when fresh efforts at vomiting manifested themselves without vomiting taking place. A third dose of 4 grammes was introduced into the stomach of a rabbit, four hours after the first. Then new convulsions and new symptoms, denoting considerable illness, were observed. Vomitings followed soon after the introduction of the poison; they were more frequent, but they were very sparing. To these expulsive efforts also succeeded signs of general prostration, another comatosed state, and a kind of stupefaction which denoted that the brain was considerably congested. This state lasted several hours; it increased perceptibly towards night, and evidently indicated that the animal would soon die. Indeed, 10 hours after the administration of the first dose, it died without giving any signs of agitation.

While all this was being done, another strong rabbit was submitted to the same influence, but with similar doses of the alcoholic extracts of the shops arising from the same provision of digitalis. The little difference which I remarked in the phenomena consisted of two or three vomitings more, and in the shorter duration of these phenomena, to which death put an end nine hours after the first administration of the poison.

It is probable that, if we had prevented the vomitings, the violence of the poison would not have permitted these animals to live so long.

I forgot to say that the urine was passed frequently and in abundance, and was rather lactescent during both experiments. This fact, although of very little importance, deserves to be noted.

Experiment VII. Alcoholic Extract of Hyosciamus Niger.—A rabbit having experienced, from 8 grammes of hyosciamus, only symptoms nearly analogous to those which 6 grammes of extract of belladonna produced in the rabbit which formed the subject of the third experiment in No. IV. of this series, 16 grammes were administered to it a few days afterwards. Then there was reason to think that death would follow this administration, for the quadruped was immediately out of breath, cruelly tormented, very agitated, and attacked with vomit uretibus, which caused it to reject a small quantity of the matter swallowed, and which made me fear vomiting, so frequent and violent were the spasmodic motions of the stomach. However, although the disorders caused went on evidently increasing during the first hour, no vomiting occurred. It was only remarked that the respiration appeared more and more accelerated, and that the nostrils were continually in motion, a remark which I have made in all cases in which the poison had exerted a powerful influence. It was remarked, moreover, that the animal always carried its head high, a sign of difficult respiration; that it, from time to time, agitated its two fore-paws, and sometimes raised them towards the nostrils with precipitation; that the regard was fixed and melancholy, the eyes dull, &c. The same symptoms were sustained in nearly the same state throughout the day; however, at night, they seemed to improve a little, although there existed a kind of torpor, characterised by the infrequency of locomotive movements.

Next day, no change for the better occurred; the animal was even more debilitated; it seemed unable to resist the poisonous influence of the extract. Towards the decline of the day an endeavor was made to make it take some nourishment, but it was in vain.

On the third day all chance of cure was for ever destroyed; a complete prostration denoted an early end. The animal was plunged in a profound state of drowsiness, from which it could only with difficulty be aroused; its respiration was much less accelerated; the urinary secretion, hitherto suspended, was somewhat re-established, and the urine was normal, at least in appearance. The day passed by without anything worthy of remark: it was only perceived that the debility became greater towards night, and that death was approaching; next morning it was found quite dead, cold, and stiff.

Then, taking 16 grammes of alcoholic extract of hyosciamus, prepared with the same plant, according to the ordinary process, I

submitted another rabbit to the influence of this product.

To describe what I observed in this case would be completely useless, the foregoing remarks being perfectly applicable here, with this difference, that death occurred on the third day in the evening, and that I saw the limbs of the animal convulsed in its last moments, without anything very remarkable having announced the approach of death; for, according to the observations of the foregoing experiments, I might have supposed that life would be prolonged until at least next day.

These are two nearly identical facts, which appear in favor of the alcoholic extracts under consideration, although death, which may depend on the idiosyncracy of the animal, occurred some hours earlier in the second case.

These facts accomplished, I should have been able to direct my experiments to other alcoholic extracts obtained from vegetable powders treated by ether, especially the extracts of strammonium and nicotiana; but I confess that I have delayed to finish these cruel experiments, which have been fatal to so many animals, at the same time that they have cost my feelings much pain, which I am not afraid of avowing. It is not given to all men to devote themselves with impunity to such investigations; and, if it is permitted to tax with weakness the sentiment of pity which I cannot but feel in the presence of my suffering victims, which I have thought it my duty to sacrifice in the interests of science, I own that my courage has failed more than once in the course of my experiments; and I own, also, that, more than once, I have been obliged to renovate it in order to consummate the sacrifice. Thus, then, if I have been able to conquer my repugnance, so long as I have felt the necessity of combating them, I could not devote myself to other experiments of this nature, when I considered them completely useless. The facts which I have related are, moreover, more than sufficient, in my opinion, for destroying all kind of doubt concerning the nullity of the ethereal tinctures which we have studied. They are such that they carry with them more useful information; for, if we put ourselves in a situation to solve the question put, they may also, it appears to me, come in aid of physiological science, by furnishing to it new lights.

Indeed, it results from all this, independent of what I intended to prove, that we were grievously in error, if we thought that our organs are not more impressionable than those of animals in general. Is it not very evident, on the contrary, that man, from his own very peculiar nature—from his great nervous sensibility—from that universal concensus which forms the specific character of all human organisation—and which performs so important a part in all the vital actions proper to the nervous system; is it not in the highest degree evident, I ask, that man should be more powerfully subject to the influence of certain poisonous agents than animals which, after him, occupy the first rank among organised beings? If it were otherwise, how could we explain, for example, the very powerful influence in fractional doses of the *solamacea* of hemlock and of digitalis on our organs, when considerable quantities of these same plants have not always killed the animals devoted to my experiments? Moreover, could this great physiological truth be doubtful to us, in the face of the very remarkable facts detailed with so much precision by M. Orfila, in his *Traité de Médecine Légale*, in speaking of the salts of morphia? These salts, so energetic in their action on man, in the dose of a few centigrammes, have been mortal to dogs only in extremely high doses. "Thus," says M. Orfila, "if the dogs are strong and full grown, they may bear a high dose of acetate of morphia without being killed; if they are young and small, it is sufficient to kill them in the space of four or six hours, to administer to them from 40 to 60 grains of poison." Now, I ask, what man, however strong he may be supposed to be, could resist the poisonous action of such an agent, even supposing him to be under the influence of a much smaller quantity of this powerful poison? Certainly, there is none; for I know not that human nature can thus brave the power of opium or its derivatives, at least until long habit has insensibly accustomed the organs to the action of the poison.

Thus, then, if there be a great number of poisonous substances which exert on man and on certain animals a nearly similar action, there are also many, particularly among those which act more especially on the nervous system, which act more violently on man than on animals; and the plants, whose products we have just studied, should belong to the latter. It cannot be argued, moreover, on the bad nature of my extracts, from the very peculiar care which I always take in the choice of plants which I intend for the preparation of such products—the habitat, exposure, the most favorable period of the season, the gathering at the flowering time, and all the conditions proper for a perfect elaboration of the active principles, are so many considerations of which I fail not to avail myself, so far as I am able to do, and to which are necessarily joined those

which relate to the collection of these products. I am too deeply impressed with the importance and value of these conditions ever to have to blame myself with having neglected them; and a daily experience sufficiently proves to me, moreover, that all my extracts answer perfectly from the precautions which I take for making them that which they ought to be. I dwell upon them, not to satisfy a vain feeling of self-conceit, always out of place in such a case, but, indeed, to give value to the facts which I have just laid down in this memoir, as well as to the considerations to which they should give rise.

I should here remark, in passing, that, however impressionable our organs may be, we generally show ourselves much too timid with respect to the posology of these powerful medicines. There is no doubt that prudence should render practitioners very reserved in the employment of these agents, especially in our temperate climate; but this reserve should not be carried to such an extent as to nullify the effects which they ought to produce; and this is, I think, very generally done in the present time. Why? Because the materia medica is not sufficiently studied at bottom. If we frequently fail in our object, should we not sometimes attribute the cause to this timidity, carried to excess, which seems to me to characterise certain medical prescriptions of the present age. Let us remember that if Stork and his imitators have found the extracts of hemlock, for example, succeed wonderfully under their skilful direction, it is because they have not feared gradually to carry doses to one gramme and more of the first, and to 16 grammes of the second. Now, I ask, what physician of the present day would be hardy enough to adopt such a posology? None. Let homœopathics render themselves ridiculous at leisure with their infinitisimal doses; but, in order to be right in the few cases which we make of their doctrine, let us not ourselves fall into the ridicule of a too timorous therapeutical practice, for this would be the means of killing ourselves with our own arms, at the same time rendering humanity a disservice. I think, therefore, that, taking account of the influence of our temperate climate on the development of the active principles of the vegetables under consideration, we ought, without rushing into dangerous excesses, to show ourselves much more bold than we are.

I apprehend that I have digressed rather too much from my subject, but I dare hope that this digression will be pardoned on account of the motive; and it is with this hope that I pass on to the preparation of the ethereal tinctures.

(To be continued in our next).

ON THE PHARMACOLOGY OF ALCOHOL, ETHER, FOUSEL-OIL, AND ETHEREAL OILS.*

BY FR. APOIGER.

(Concluded from page 322.)

IV. FENNEL OIL (OLEUM FŒNICULI).

The ethereal oils of the umbelliferæ are particularly remarkable for their sweetish taste, which circumstance also indicates that their physiological as well as therapeutical qualities are of a gentler kind than those of aromatic and acrid ethereal oils. As such are known and especially estimated the oleum anisi and fœniculi; it is known that they easily pass from the stomach into the blood, exerting a mild and restorative stimulus on the mucous membranes, not only of the intestinal canal, but most particularly on those of the organs of respiration, hence they are employed as so-called expectorating medicines. It appears that physiological experiments with animals have not yet been instituted with these substances, hence Professor C. G. Mitscherlich, of Berlin,† has undertaken a similar experiment with a rabbit, into the stomach of which he injected six drachms of oleum fœniculi æthereum. Soon followed great restlessness, with accelerated pulsation of the heart. After one hour the animal appeared to be languid, it trembled, convulsions in the spinal muscles were noticed, and after thirty-six hours it expired. During the whole time much urine was repeatedly evacuated; but little and rather solid feces were excreted; the breath distinctly emitted the odor of fennel oil, the same odor was perceived on opening the abdominal cavity; in the blood the odor of the fennel oil was but indistinctly perceived, and was quite absent in the urine. The stomach was not externally reddened, in its inner sides a considerable layer of mucus was noticed; between the epithelium and vascular membrane a great many spots were noticed of the size of a pin's head, of a brownish-black color, and some others partly smaller, partly larger ones, forming protuberances, and containing blood globules. The epithelium of some of these spots was found to be excoriated, and blood extravasated in the stomach. The small intestines were externally white, and contained, likewise, much mucus, and were reddened on their inner sides. The inferior portion of the small intestines, the cecum, the rectum, the lungs, heart, &c., were in the normal state. This experiment, there-

* Buchner's *Repertorium*, xxxvii. 3.

† These experiments were first communicated in the *Med. Zeitung*, Nos. 44 and 45 for 1843.

fore, corroborates the long ascertained fact, that fennel oil has a very gentle action, and that great doses of it are necessary to destroy life.

V. CARAWAY OIL (OLEUM CARI CARVI).

This body is much similar to the preceding oil with reference to its physical, chemical, as well as medical properties; it was, therefore, to be anticipated that it would not be in a higher degree fatal in its effects. Mitscherlich injected, therefore, directly, half an ounce of this oil into the stomach of a middle-sized rabbit. The effects produced were similar to those of the preceding substance. The animal immediately appeared to be languid, the breathing became difficult, the pulsation of the heart much accelerated. It soon, however, turned insensible, and after one hour the pulsations of the heart could scarcely be numbered. A notable contraction of the pupils was observed, and the lungs emitted the odor of caraway. The animal finally suffered some slight convulsions, and after five hours calmly expired. Urine was often evacuated, but no feces excreted. The dissection having been instituted immediately after death, the peristaltic motion of the intestines was seen but feebly to continue; the abdominal cavity diffused a distinct odor of oleum carvi; in all other respects the stomach and small intestines exhibited changes similar to those produced by the fennel oil; except that a less quantity of blood was seen to be extravasated in the stomach. The odor of the blood was not distinctly that of oleum carvi, and the urine was quite free from any similar odor. One ounce of the oil in question was injected into the stomach of a large-sized rabbit. This time death followed after five minutes, under similar symptoms as above. The stomach, externally as well as internally, was found to be unchanged. It contained a little oleum carvi. The cecum and rectum in this, as in the first experiment, were normal. It appears from the above experiments that the oleum carvi as well as fœniculi, and fousel oil are absorbed by the stomach, and in part excreted again in the lungs, but not in the urine, that it produces a change in the structure of the stomach and duodenum, but that it causes no inflammation in these organs, and that it cannot be considered as a dangerous poison.

VI. OIL OF TURPENTINE.

With this oil experiments with animals have been instituted some time back by several physiologists, viz., by Lanzoni, Friend, Hertwick, Gaspari, and Schubarth, by which it appears that from 1 to 2 drachms of this oil may be introduced into the stomach of a dog, and one ounce of the same body into the stomach of a horse without proving fatal to either animal; nay, Copland ventured to swallow 10 drachms of this body previous to breakfasting. He observed after this, acceleration of the pulse, some vertigo, and shivering cold, in the epigastric region sensation of heat, and some uneasiness. After one hour the disagreeable sensation in the epigastric region was painful to a greater extent, chiefly on account of a contraction of the intestines; the pulse became small and hard, but continued to be accelerated; the physiognomy pale and distorted; to these succeeded a sense of anxiety, and finally hunger and thirst. The breath diffused a strong odor of oil of turpentine, and the urine that of violets. After 10 hours a state of coma and drowsiness were observed. After a little sleep Copland was so weakened that he felt obliged to have recourse to medical assistance.*

Purkinje took for three consecutive mornings one drachm of oil of turpentine each time, partly with and partly without sugar; after which he felt more hot and very drowsy.† Schubarth injected into the stomach of a dog, three years of age, 2 drachms of oil of turpentine, which was followed by restlessness, acceleration of the pulse, tetanus, and after three minutes death took place. From this termination we may conclude with great probability, that by forcibly administering the oil of turpentine, a portion of it has passed into the windpipe; an accident easily occurring if force is used in giving some substances to dogs, and against which we ought to take all possible precautions, to prevent false conclusions. Thus it happened that Schubarth, on repeating this experiment with two other dogs, obtained quite different results; one of them to which two drachms were likewise given, staggered, and, after a quarter of an hour, vomited twice; and somewhat later, ejected again some sanguine foaming matter. After three hours and a half the animal continued to be languid and sad. One drachm of oil of turpentine was then injected into the stomach of a third dog, without, however, injuring the health of the same.‡ Mitscherlich injected half an ounce of oil of turpentine into the stomach of a large-sized rabbit, whereon the animal soon

* *London Medical and Physical Journal*, by Fothergill, July, 1821, also in Wibmer's work, *Uber die Wirkungen der Arzneien und Gifte*, vol. iv., p. 211, and in Buchner's *Toxicol.*, 2nd edition, p. 312.

† *Neue Breslauer Sammberg*, 1829, p. 439.

‡ Horn's *Archiv.*, 1824, vol. i., p. 89.

became restless, the respiration being at the same time accelerated, the pulsation of the heart frequent; their frequency gradually diminished, and lasted for two hours. The exhalation of the lungs diffused the odor of oil of turpentine. The urine was repeatedly secreted, and in large quantities, emitting the odor of violets; feces were at first but sparingly evacuated, but soon a diarrhœa supervened, with weakness, which lasted 60 hours, after which the animal calmly expired without exhibiting symptoms of spasms. In the evacuated feces, oil of turpentine and mucus, with a great many tumefied cylindric cells, were found. The abdominal cavity having been opened, an odor of turpentine oil was diffused, which in the blood could not be distinctly perceived. The stomach and small intestines were not inflamed; the first, however, offered a great number of bluish spots with a whitish bordering, indicative of an alteration in the structure of the interior membranes of the stomach, similar to those produced by fennel and caraway oils. The epithelium situated beneath the thick mucous membrane of the stomach, was much thinner than commonly found, in some places inflated by vesicles of the size of peas, containing partly blood globules, partly resolved blood, which imparted a black-red appearance to the interior sides of the stomach. The small intestine was of its natural color, containing in addition to oil of turpentine, a great deal of mucus; in this case likewise the epithelium was found to be very thin; the vascular membrane overfilled with blood. The bladder repleted with urine, and not much reddened. The urine contained a great number of small round corpuscles, which seemed to be nucleuses of cells. Kidneys, lungs, and other organs were normal.

Into the stomach of another large-sized rabbit Mitscherlich injected one ounce of oil of turpentine; notwithstanding this great dose, death followed only after 44 hours. The phenomena preceding this fatal issue were, on the main, similar to the foregoing, except that now the diarrhœa was more abundant, and that towards the close of life only mucus with strongly inflated cells was excreted. In the stomach, instead of the described blood vesicles, were found from forty to fifty brown-black spots, most of which were of the size of a pin's head, and some of them larger; no epithelium was on them, but a drop of blood was on each, which, when removed, left an excavation, radicating in the vascular membrane. These spots were partly covered with mucus, which became brown by the extravasated blood; they had a white bordering, without a redness indicative of inflammation.

The results of these experiments were very remarkable; they show that the oil of turpentine makes part of those ethereal oils which may be taken in considerable quantities, without causing inflammation, and without causing death rapidly to take place. Like the fousel, fennel, and caraway oil, it is absorbed by the stomach, and conveyed to the lungs, being by the latter partially exhaled; but the said ethereal oils apparently not acting on the inferior portion of the intestines and urinary system, the oil of turpentine acts in a peculiar and known manner, wherefore it is employed as an anthelminticum and diureticum, and imparts to the urine a peculiar odor. The change of structure, and the formation of blood vesicles, with a considerable secession of the epithelium in the stomach are remarkable.

VII. OIL OF JUNIPER.

Dr. Fr. Simon, of Berlin, has instituted experiments relative to this substance, and found that the same acts in small doses, like oil of turpentine, that it may scarcely promote either appetite or digestion; that it, however, somewhat accelerates circulation, augments the sensation of heat, and the secretions, especially those of the kidneys, imparting to the urine an odor resembling violets, like turpentine oil; that on continuing it for a long time it causes strangury, dysury, ischury, and even hœmatury. Half an ounce of the ethereal juniper oil injected into the stomach of a rabbit has not proved fatal, but the animal became very ill.

One ounce proved fatal after 22 hours in a similar manner, as Mitscherlich observed to be caused by oil of turpentine. In this case he found the stomach not to be inflamed, but the secession of the epithelium was more considerable; the intestinal canal and kidneys were somewhat reddened. In the urine Simon discovered with the microscope a great quantity of cylindrical corpuscles, which seemed to consist of cells and cellular granules, and undoubtedly originated from an irritation of the kidneys.

VIII. OLEUM SABINÆ ÆTHEREUM.

It has been known since the time of Galenus that juniperus sabinæ is a more violent agent than juniperus communis, and that this effect becomes manifest not only by an accelerated circulation of the blood, the exhalations of the lungs, and augmented secretion of the urine, but also by an increased congestion of the blood towards the uterus.

Hillefeld administered to a large-sized cat

* *Med. Zeitung*, v. d. V. f. H. in Pr., 1844, No. 19.

1 drachm of oleum sabinæ, and observed salivation, anxiety, trembling, and an augmented evacuation of the urine.* Orfila administered to dogs from 4 to 6 drachms of pulvis herbæ sabinæ, and afterwards tied the œsophagus. These experiments caused death to take place after several hours, and on dissection Orfila found in the stomach indication of the action of irritating poisons. These facts not being decisive in a pharmacological point of view, Prof. Mitscherlich† instituted some fresh experiments.

2 drms. of oleum sabinæ were injected into the stomach of a middle-sized rabbit; the pulsations of the heart and the respiration were soon very frequent, all the muscles of the animal were seen to tremble, and after 25 minutes the pulsations of the heart could no more be numbered; the exhalations of the lungs emitted a strong odor of oleum sabinæ. Afterwards the rabbit became insensible, the trembling ceased after two hours, the urine was twice evacuated under violent exertions; the said excretion was dark-yellow, and likewise diffused the odor of sabina. The repeatedly-evacuated feces were hard. The breathing became gradually very difficult, and after great agony, and the lapse of 7½ hours, death followed; whereon immediately the animal was dissected. The muscles on being irritated continued to contract, and the motions of the heart persisted for a long time; the same is the case with the peristaltic motions of the intestinal canal. The blood exhaled a strong odor of sabina, and the same was emitted from the abdominal cavity. The urinary bladder contained a little dark-yellow urine, of the odor of the ethereal oil. The kidneys and urinary bladder contained more blood than usual, but were not inflamed; the exterior surface of the stomach was a little reddened; it contained a small quantity of food, but a great deal of the ethereal oil, and a thick layer of mucus; the same was the case with the whole of the small intestines which were externally more reddened than the stomach. The epithelium of the stomach and the intestinal canal was in its natural condition, the vascular membrane, however, of the intestinal canal was found overfilled with blood; the same was the case with the vessels of the heart; the lungs were natural.

Half an ounce of oleum sabinæ was injected into the stomach of a large-sized rabbit. The symptoms following the action, and the results of the dissection were, on the main, the same as in the former experiments. Death followed after 6½ hours. Neither feces nor urine were evacuated during this time. These experiments show that the oleum sabinæ, like other ethereal oils, are absorbed from the stomach; that its action is more violent than that of oleum juniperi and terebinthinæ; that, however, even after large doses, the fatal termination takes place only after several hours; that it acts strongly on the kidneys, in a manner that these become filled with blood, and that it is excreted partly with the urine, partly with the exhalations of the lungs; and, finally, that its effect on the intestinal canal consists in a considerable overfilling of the blood vessels, and augmented secession of the epithelium.

IX. OLEUM SYNOPIS ŒTHEREUM (ETHEREAL MUSTARD OIL).

It is known that mustard may irritate the stomach and intestines; that in large doses it causes over-excitation and vomiting; that it, moreover, possesses a diuretic action, wherefore in the latest days mustard oil has been recommended in doses of from 1 to 2 drops in a mucilaginous vehicle as a diuretic, and often prescribed. Physiological experiments on animals have not yet been instituted with the ethereal oil of mustard; hence, Mitscherlich undertook the following:—

1 drm. of oleum synopis was injected into the stomach of a large-sized rabbit. The animal cried not, it ceased, however, to be sensible for mechanical irritation; the pulsations of the heart were very frequent; after half an hour it appeared to be very languid and trembling, somewhat restless; the breathing was molested, the exhalations of the lungs, resembling the odor of mustard oil, gradually became stronger; the pulsations of the heart became feeble, scarcely numerable. After 80 minutes convulsions took place, and after 2 hours death followed. The dissection being directly instituted, the peristaltic motions of the intestinal canal were still found to be very strong, and continued for some time. The blood drawn from the veins was very dark, coagulated only gradually, and diffused a strong odor of mustard oil. The urinary bladder was filled with urine, which likewise exhaled the odor of mustard oil. Kidneys and bladder were inflamed. The stomach and superior half of the small intestines, though somewhat reddened, were not strongly inflamed; in the stomach a considerable quantity of the oil was still found. In the pericardium a considerable quantity of liquid exudation; the exterior surface of the heart reddened; the lungs normal.

* Experimenta circa venena; from this in Wibmer's work, *Uber d. Wirkurgen d. Arzneien u. Gifte*, vol. iii., p. 191.

† *Med. Zeitung*, v. d. V. f. H. in Pr., 1843, pro 44.

Half an ounce of mustard oil was injected into the stomach of another big-sized rabbit; violent symptoms instantly followed this time; strong convulsions with vehement cries; death after fifteen minutes. The dissection being immediately instituted, the pulsation of the heart was seen to continue for one hour; the other phenomena agreed with the above.

From these experiments we may draw the following conclusions. 1. That mustard oil in an equal dose acts much stronger, and becomes sooner fatal, than all the other ethereal oils mentioned above; it is, therefore, to be reckoned among poisons. 2. The local inflammation, which it excites in the stomach and intestines, is, however, not so strong as might have been anticipated from its strong odor and taste. 3. It is rapidly absorbed and conveyed into the blood. 4. It is excreted again by the surface of the lungs and urinary organs. 5. The irritation of the muscles, and the heart continue for a long time after death.

CONCLUSION.

That alcohol, ether, and the ethereal oils are rapidly absorbed by the mucous membranes, conveyed into the circulating system; that they are excreted again partially by the exhalation of the lungs, partially with the urine; and that at the same time an acceleration of the pulse, and of the pulsations of the heart takes place, has been fully proved by all experiments instituted up to the present day. Gmelin and Tiedemann found this in their experiments with garlic and assafœtida; Milne Edwards, Breschet, Magendie, Viborg, Purkinje, Copland, Hertwig, Schubarth, Fürst, Simon, Mitscherlich, &c., corroborated the same fact relative to other ethereal oils. It is remarkable that the fousel-oil is in a similar manner intoxicating as alcohol, but its action is less dangerous, as has been believed hitherto, and that in the manner of fennel and caraway oil, it acts not on the urinary organs like alcohol, turpentine oil, and juniper oil, &c. It is remarkable, moreover, that alcohol and ether cause an inflammation of the stomach, and small intestines to take place, whereas several ethereal oils, of which a similar inflammatory action might rather have been anticipated, as fousel-oil, turpentine, juniper, fennel, and caraway oils, produce only a change of structure in the membranes of the stomach, in consequence of which a blood exudation, but no real inflammation may be called forth, and even the inflammation of the stomach caused by the most acrid, and violently acting mustard oil, is not so vehement as might have been anticipated. Finally, that the effects of alcohol are peculiarly characterised by the subtraction of water from the mucous membranes, the contraction and secession of the cells of the epithelium; and those of the ether are distinguished by the rapid penetration of this liquid through the cellular tissues, and its gaseous expansion in the same.

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NUMBER OF APOTHECARIES* IN AUSTRIA.†

In the latest edition of the *Pharmacopœia Austriaca*, constituting one volume of the *Codex der Pharmacopœen*, published in 1844, in Leipzig, is contained the following table, showing the number of the pharmaceutical shops in the Austrian empire (Hungary not comprised) as it was in the year 1836:—

	Round Numbers of Inhabitants.	Number of pharmaceutical shops.	Proportion.
Austria unter der Ems..	1500000	99	1 : 15·000
Austria ob. der Ems..	850000	45	1 : 19·000
Tyrol and Voralberg ..	850000	79	1 : 10·700
Styria	950000	36	1 : 26·400
Illyria	750000	36	1 : 20·800
Dalmatia ..	390000	30	1 : 13·000
Lombardy..	2500000	782	1 : 3·200
Venice	2000000	751	1 : 2·600 (?)
Bohemia ..	4500000	193	1 : 22·000
Moravia and Silesia ..	2000000	76	1 : 26·000 (?)
Galicia	4500000	645	1 : 6·000

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* The German word apotheker does not convey the meaning of the English term apothecary, which alludes to a trade severely prohibited in Austria. It is not lawful in the Austrian dominions for any one individual to prescribe medicines and sell them himself; but as, on the other hand, the German apotheker being obliged to pass through a regular course of *university* education, we find no English word adequately expressing the position he holds in society.

† Casper's *Wochenschrift*, 31st May, 1845.

ON THE ACTION OF SUGAR UPON THE TEETH *

From researches made on this subject, M. Larrey has come to the following conclusions:—

1. Refined cane, or beet-root sugar, is prejudicial to the teeth more from its direct contact than from the evolution of gaseous matter during digestion.

2. If a tooth be allowed to macerate in a saturated solution of sugar, it is so decomposed as to acquire almost a gelatinous character, while the enamel becomes opaque and spongy, and crumbles down under the slightest pressure. Sugar ought not, therefore, to enter into the composition of tooth-powder.

3. The erosion of the teeth by this substance does not depend on an acid, for none is present in sugar; but on the affinity which this organic principle has to enter into combination with the calcareous base of the tooth.

4. If the enamel be less attacked than the osseous part of the tooth, the reason is that it contains fluoride of calcium, a body which resists chemical agency even more than the sulphate of lime.

[Remarks.—That sugar easily combines with lime, and forms a compound out of which the lime may be obtained crystallised as carbonate, has been long known; but it is something new that it should thus exert a solvent power over the phosphate and carbonate of lime. M. Larrey's last conclusion does not appear to us to be sound: the enamel contains only traces of fluoride of calcium, while the phosphate of lime forms 78 per cent., and the rest is chiefly animal matter. A better reason will be found probably in its hardness and close texture. It would be interesting to know whether these chemical results are borne out by observations made among those who are in the habit of taking large quantities of molasses and saccharine substances.—Ed. Gaz.]

ON THE ACTION OF THE PROTOXIDE OF NITROGEN INHALED UPON THE HUMAN ORGANISM.†

BY FRANCIS APOIGER.

After having prepared from pure nitrate of ammonia protoxide of nitrogen, and inhaled of it 108 ounces (estimated by its volume), I observed first a sweetish taste, and then some narcosis, which soon disappeared again, and two or three seconds after passed into a peculiarly agreeable sensation; my mind became exhilarated to a degree beyond description, which, according to the observations of the persons surrounding me, was distinctly expressed by my eyes. After two minutes this sensation reached its zenith; all here below, together with those about me, appeared to dwindle away. I seemed to dwell in higher regions, and wholly composed of mind. I had not lost the power of thinking; though it was not in my power to occupy my mind with anything made of matter. It was not a sensation like that perceived by a person intoxicated with wine or opium, in which the sensual desires act so prominent a part; no, it was the purest delight of the soul; I was filled by a blissful joy, and for a few minutes I felt myself unfettered from my corporeal frame. It would, therefore, have been untrue to say that the sensations perceived by myself resembled intoxication; nor did this ecstacy exhaust my forces, which is the case when intoxication takes place in consequence of excessive drink. Soon after this and the following days my bodily strength was not at all injured, and I felt as well as if nothing had passed, nor was my breast in the slightest inconvenienced. In most elementary works on chemistry it is stated that this gas produces intoxication, for which last word, according to the above, *ectasis* ought to be substituted—this expression being made use of already in Buchner's toxicology.*

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ON SANTONIN.†

BY PROFESSOR PERETTI,
OF THE ARCHIGYMNASIUM, OF ROME.

I give the name of santonin, and not santonine, to a crystallised substance, discovered, almost at the same time, by MM. Köhler

* *Medical Gazette.*

† Buchner's *Repertorium*, xxxvii., 3.

* This fresh confirmation of the peculiar and extraordinary action of the protoxide of nitrogen, in a therapeutical point of view, deserves all attention, as it may be hoped that this body, by being administered as a medicine in cases of debility of the arterial activity, the predominant venosity of the blood, want of organic development of heat, and perspiration of the skin, &c., may be of great service—a fact which really took place with regard to the epidemic cholera. Care is, however, to be taken that the patient may inhale the gas in small quantities, in a state of great dilution with atmospheric air—viz., it is from time to time in adequate proportions to be admixed with the air of the room of the patient.—B.

† *Journal de Pharmacie.*

and Almi, in the Arthemisia contra santonica judaica, vulgarly called holy seed (*semence sainte*). This substance presents, according to the above authors, the following character: it is neutral, crystallised in flattened prisms, with six faces, obliquely truncated at the summit, without perceptible color or taste, with the exception that, after long mastication, a slight bitterness is remarked. It refracts light, which in a few moments communicates to it a yellow color. It does not become colored in the dark; it is sparingly soluble in cold water and alcohol; soluble in hot alcohol, in ether, and the fatty and volatile oils; these solutions have a very bitter taste. It fuses at the temperature of from 275° to 280°F. into a colorless liquid, which, by cooling, becomes a crystalline mass. At a higher temperature it is converted into dense white vapors, from which, every precaution being taken, they are condensed into needles without the substance having suffered any alteration: it dissolves in concentrated sulphuric acid, and, when water is added to this solution, the santonin is precipitated. Concentrated acetic acid perfectly dissolves it, and, by evaporation, the santonin is precipitated without alteration of form: with most of the metallic oxides combinations are formed which are, to a certain extent, soluble in water. The saturated solutions do not bear boiling. If the oxides are insoluble in water, they are precipitated, and the santonin crystallises by cooling.

After having studied the characters which belong to santonin and the processes for obtaining it, I occupied myself with investigating its electro-chemical properties, and with ascertaining whether it were capable of saturating the metallic oxides, or whether it saturated the acids. With this object, I took the crystallised santonin of commerce, and I boiled it in a porcelain capsule, with water and potassa, and I observed that the liquor did not exert any action on turmeric paper; I filtered, and, after cooling, I observed that some small crystals were separated. The liquor retained its state of neutrality, and I introduced it into an U tube, into which I dipped platinum wires, and, on passing into it an electric current, it became turbid. On testing the liquors with test-papers, I observed that turmeric paper, reddened at the negative pole, and litmus paper at the positive pole; and I thus ascertained that santonin possesses an electro-negative property, from which I conclude that this body has acid properties, as Thompson and Liebig, who regard it as greatly resembling the fatty acids, have suspected.

Not finding the processes hitherto published respecting santonin satisfactory, nor the characters ascribed to this body sufficiently clear, I at once investigated the state in which it is found in the seed, whether combined with an oxide or dissolved in an acid.

Köhler announces that he has found in the ethereal extract of the holy seed an acid which forms a crystallisable salt, but I do not know whether he means that this acid forms a crystallisable salt with santonin or with some metallic oxide.

The decoction of holy seed is acid; by gradually adding potassa in order to saturate the free acid, I obtained an abundant yellow precipitate, which was separated by filtration: the liquor evaporated to a syrupy consistence, gave, on cooling, a viscid residue, in which some irregular crystals were found; this residue was treated with alcohol, and the whole thrown on a filter; the crystals were dissolved in boiling water, and the liquor, again filtered, very soon deposited crystals in quadrilateral diaphanons, and very regular prisms; if nitrate of silver or chloride of calcium be poured into the mother-liquor, a white precipitate is obtained. This precipitate, dried and introduced into a glass tube, closed at one extremity and heated over a spirit-lamp, does not turn black, and no action is produced on test-paper; heated in a platinum capsule on the open fire it leaves an alkaline residue without charcoal. If oxalic acid be added to a solution of this salt, small crystals are immediately produced, which precipitate at the bottom of the liquid. This character, and various others formerly noticed, led me to presume that the free acid found in the seed was oxalic acid; but, considering that the neutral oxalate of potassa crystallises with difficulty, I abandoned this view. I intend to return to this subject, and to endeavour to procure a greater quantity of the same salt; for the present I will content myself with saying that, by treating with alcohol the yellow precipitate obtained, by adding potassa to the decoction of the seed, boiling and decoloring by animal charcoal, I obtained santonin, which I likewise extracted in decoloring the alcohol used for dissolving the extract from which the salt of which we have just spoken was separated. Having recognised the electro-chemical characters of santonin and its property of saturating bases, I examined the crystals deposited by the cooling of the product of the boiling of the seed with potassa, which I consider as a composition of santonin acid and potassa, and which I will designate under the name of *santoniate*. These crystals, not having regular forms, were a mixture of prisms, lamellæ, &c., with a scarcely

perceptible taste, sparingly soluble in cold alcohol, soluble in boiling alcohol, which deposited, on cooling, small prismatic needles united in fasciculi.

Submitted to the action of heat, these crystals fuse into a colorless fluid which, on cooling, becomes a crystalline mass.

By increase of temperature, the fluid takes a dull, ruby color, gives off empyreumatic vapors, and does not leave any carbonaceous residue, but a white substance, soluble in water, which reddens turmeric, restores the color of reddened litmus, and effervesces with the acids, with characters of carbonate of potassa. The salt of which we speak is, therefore neutral santoniate of potassa, and that which is met with in commerce is the bisantoniate.

A decoction of holy seed, besides being acid, yields a black precipitate with the salts of iron: when well saturated, it has a deep yellow color and preserves its transparency to ebullition; but, by cooling, it becomes turbid, and, left to itself, it deposits a granular substance of a yellowish gray, of a bitter and astringent taste which, moistened with a solution of a salt of iron, becomes black. This matter appears to be a combination of tannic acid and bisantoniate of potassa. Notwithstanding that the liquid deposited the substances above indicated, it continued turbid, and, left to settle, it deposited a new precipitate. This second precipitate is almost white.

These two precipitates being treated by boiling alcohol, we obtain, by cooling, crystallised bisantoniate of potassa. When poured into water the liquid is rendered milky, and, after being left to settle for several days, a new quantity of bisantoniate is obtained.

The two alcoholic solutions being decolored by an animal charcoal, and being converted into aqueous solution, bisantoniate of potassa is obtained well crystallised and white.

In admitting that the crystallised solution, hitherto designated by the name of santonine, was a bisantoniate, I wished to ascertain whether it contained potassa: for that purpose I burned some santonine of commerce in a platinum capsule, and I obtained carbonate of potassa as a residue.

A decoction of holy seed was entirely decolored by animal charcoal; the liquor was no longer bitter; the charcoal was well washed and dried and treated by boiling alcohol, and the alcoholic liquor was very bitter. After having diluted it with water, the alcohol was distilled off; it became turbid and white, and, on cooling, it deposited bisantoniate of potassa.

Into a saturated alcoholic solution of holy seed, and poured an alcoholic solution of potassa, which formed a viscid precipitate composed of yellow coloring matter and potassa: the alcoholic liquor filtered and diluted with water, became turbid as before, and deposited bisantoniate of potassa, somewhat colored.

An infusion of holy seed in olive oil deposits, in time, almost white but uncrystallised bisantoniate of potassa, which is obtained in crystals from a boiling solution in alcohol.

It seems, therefore, that the vermifuge property of the holy seed is not entirely due to the bisantoniate of potassa, the proportion of this body which is obtained by any process whatever being very small.

NOTE ON SANTONIN OR SANTONIC ACID.*

BY H. GUALTIER DE CLAUBRY.

Being at Venice a short time ago, Professor Zantedeschi communicated to me the results of a curious experiment which he made on santonine, and which was opposed to those which different chemists have obtained, with severel organic coloring matters, and in particular those of Professor Bizio on the purple of the ancients.

It is known that indigo and orcine, for example, are colorless, and take the brilliant tint which characterises them only by the absorption of oxygen; other analogous facts appear to lead to the general result that many coloring matters present the same character.

It is known that santonin, exposed to the action of the solar light, speedily acquires a yellow color; but is oxygen in this case the principle which actuates this change? According to the experiments of Professor Zantedeschi, this does not appear to be the case. If santonin be exposed to isolation in the barometric vacuum, it acquires a yellow tint almost as bright, and nearly in the same time, although rather less rapidly, as another portion of the same substance, placed under the same luminous influence in the air or in oxygen. Professor Zantedeschi concludes from this that the coloration is due to a peculiar action of light.

Without adopting this conclusion, which could only be the result of numerous experiments, it may be well to call to mind a very old experiment of Dr. Vögel, sen., in which phosphorus, exposed to complete isolation in hydrogen gas or *in vacuó*, was converted into a red body analogous to the oxide of this body.

* *Journal de Pharmacie.*

ON THE ACTION OF THE BERRIES OF THE ATROPA BELLADONNA (DEADLY NIGHTSHADE) ON THE HUMAN ORGANISM.*

BY FR. APOIGER.

Basing myself on the statement given by Gottfr. Hagens, in his *Lehrb. der Apothekerkunst*, where he says, that 10 to 12 berries of the ripe plant may kill a man, I ventured to eat four of the largest berries of this fruit. After a quarter of an hour, already I felt a lassitude in my limbs, which soon became augmented to a degree that the joints of my knees were continually tottering, and it was completely beyond my power to keep myself upright. In addition to this I had a copious and continued secretion of saliva, which is not in accordance with what is stated in the toxicological manuals, viz., that the palate and throat are dry. The pharynx was contracted indeed, which greatly molested me. To remedy this, I took some tepid milk, with butter melted in it. I avoided the use of an emetic, by which I might have been deprived of the real image of the poisoning by belladonna. Dilatation and immobility of the pupilla, I perceived by the aid of a looking-glass; vertigo and complete stupor, without vomiting; then painful retentio-urinæ, were the last symptoms of the poisoning perceived by myself. In this state I threw myself on my bed, and after a most restless sleep of fourteen hours, I awoke greatly suffering from head-ache, without experiencing any other inconvenience besides lassitude in the arms and feet.

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EFFECTS OF OPIUM ON THE HUMAN ORGANISATION.†

BY FR. APOIGER.

To obtain an idea of the Turkish opium intoxication, so much talked of by Chardin, in his work entitled *Voyage en Perse*, vol. i. p. 12, I took at first 2 scruples, and after six hours one drachm, of comp. opium tincture (Laud. liquid. Sydh.) Though the same was prepared wholly according to the Bavarian Pharmacopœia, and the tincture exhibited the stated specific gravity, viz., 1·050, I will consider one drachm of this vinous opium tincture as containing merely 8 grains of pure opium, as the admixed ingredients (cinnamon, cloves, and saffron) may prevent the principal constituent from being entirely dissolved. I, therefore, consumed, according to my calculation, 13½ grammes of opium. By this quantity of opium, which is certainly considerable, I thought to call forth a state of intoxication bordering on ferocity and frenzy, or that thousands of the loveliest images would occupy my imagination; but I was deceived in my expectations. After half an hour, on having taken the second dose, I fell into a kind of intoxication which above all augmented my muscular powers, and felt inclined to quarrel and fight; and experienced at the same time erectio carnis. After eight hours the effects of the opium entirely disappeared.

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* Buchner's *Repertorium*, vol. xxxvii. p. 3.

† Buchner's *Repertorium*, vol. xxxvii. p. 3.

ON THE EFFECTS OF THE BERRIES OF THE PARIS QUADRIFOLIA (THE HERB PARIS, TRUE LOVE) ON THE HUMAN ORGANISM.*

BY FR. APOIGER.

Four berries of the Paris quadrifolia, when in a ripe state, swallowed by myself, produced no effect. Gessner maintains to have taken one drachm of the berries with vinegar, with the effect of much sweating and dryness in the pharynx.† The four berries swallowed by myself weighed 1½ drachms, and were also taken with vinegar, by which latter fact I believed to have heightened their action.‡ In experiments of this kind we must avoid all anxiety and fear, or else the experimenter will mistake the effects of an excited mind, for that of the poison.

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* Buchner's *Repertorium*, vol. xxxvii. p. 3.

† Gessner's *Epist. Med.*, and Buchner's *Toxicologie*, p. 278.

‡ That the einbeere, solanum quadrifolium of the ancients, is but of a feeble action, has also been observed by Schubarth; he gave to dogs from 15 to 20 of these berries without any result (Horn's *Archiv.*, vol. i. p. 67). Bergius maintains that the powder of the herba paris quadrifolia promotes in some measure the intestinal excretions; Coste and Willemet found that the root may cause vomiting (Wibmer, *die Wirk. der Arzneim, und Gifte*, vol. iv. p. 166). Desbois de Rochefort also prescribed it as an emetic. Wibmer, however, took half a drachm of the root reduced to powder, without any striking result; a decoction of 2½ drachms of the root taken gradually, caused only a slight grating in the pharynx, and a sensation of weariness in the head. (*Repertorium für die Pharm.*, vol. xxxi. p. 217, and vol. xxxix. p. 27).—B.

CAUSES OF THE FEVER OF THE NIGER.*

Some years ago, the late Professor Daniell inferred that the pestilential miasmata of the coast of Africa were owing to the evolution of a large quantity of sulphuretted hydrogen gas, from the decomposition of the sulphates contained in the waters in contact with organic matter aided by a high temperature. He found in examining the copper sheathing of the ships which had been placed on that station, that the metal was partially changed to sulphuret. He also detected sulphuretted hydrogen in the water procured from the African coast, which had been sent to him by government for analysis. The medical officers attached to the late Niger expedition, have, however, proved that the symptoms attending the fever are entirely different from those which accompany poisoning by sulphuretted hydrogen gas; and further, they have not discovered sulphuretted hydrogen in the waters of any of the lagoons on the coast. In the water examined by Professor Daniell, it had, therefore, probably resulted from a decomposition which had taken place in it subsequently to its removal from Africa. We quote the following remarks from the last number of the *Edinburgh Medical and Surgical Journal*.

"The banks of the mouth of the Niger are from one foot and a half to two feet above the level of the river, and even this moderate elevation extends very little from the river, for all behind is flat and swampy. A short distance up the river, the banks are broken into lagoons and creeks, the edges of which are profusely covered with mangroves. Small bays even are formed by the sea running into them during the flowing tide, and these abound with crocodiles and multiplied races of reptiles, while the mangrove-roots are covered, as in all other places, with numerous shell-fish and various forms of mollusca. During low water a disagreeable odor was perceptible in some of the lagoons entered by Drs. McWilliam and Stanger, but the most delicate tests gave no evidence of the presence of sulphuretted hydrogen in any of the waters."

[Some of the most offensive effluvia resulting from the decomposition of animal and vegetable matter do not contain a trace of sulphuretted hydrogen gas.—Ed. Gaz.]

MEDICO-LEGAL RESEARCHES ON ARSENIC.

BY M. BLONDLOT.

The author states that two modifications ought to be made in Marsh's apparatus: 1. In destroying the animal matter by sulphuric acid, as recommended by MM. Flandin and Danger, the operation should not be continued until a light friable charcoal is obtained, but should cease as soon as the mixture presents the consistency of paste; water must then be added, and a current of chlorine gas made to pass through the solution, so as to remove all the organic matters; finally it is this liquid which must be made to act upon zinc. 2. Marsh's apparatus should terminate in Woulf's tubes, in one of which the zinc, in a spiral form, should be placed so that merely by elevating or depressing it the current of hydrogen gas may be augmented or diminished, as by this means the liquid will act on more or less of the zinc plate.

IV. REVIEWS AND NOTICES OF BOOKS, &c.

OUTLINES OF CHEMISTRY, FOR THE USE OF STUDENTS. By William Gregory, M.D., Professor of Chemistry in the University of Edinburgh. With Engravings on Wood. Part II. Organic Chemistry.

The organic department of chemistry, zealously studied by numerous chemists in England and abroad, has recently occupied the attention of the public to a greater extent than any other science, chiefly on account of its relation to physiology and pathology, concerning which, however, much matter for ingenious controversy has sprung up. Unfortunately, many of the chemists who have labored in this ample field, have built up theories on imperfect or erroneous data, on the results of inconclusive experiments; and, in many instances, have supported these theories by illogical reasonings. They have also worked out the theories of chemists who have carried the world along with them in their views, without sufficiently assuring themselves of the correctness of the bases on which those theories are founded, blinded to inaccuracy by authority ren-

* *Medical Gazette.*

dered illustrious by popular acclamation, and which it would be loss of reputation to oppose. Although we admit that much good has been effected, still, we insist that so much has been taken for granted on the *ipse dixit* of those who have started at least as great a number of theories remarkable for boldness rather than correctness of deduction, as of theories capable of bearing even biassed scrutiny. In fact, chemists have brought organic chemistry into a state which requires immediate rectification. The experiments on which the theories are constructed require repetition and verification: this would most certainly lead to the rejection of many, to the modification of the remainder.

The work before us is an excellent epitome of the science as it at present stands, affording the student a knowledge of all that has been done, and of the directions in which investigations have been made; and as such, we strongly recommend it to all who desire to commence the study of chemistry, but especially to those whose limited means may render the larger works inaccessible to them. It is, indeed, *multum in parvo*.

We conclude our notice and recommendation of this work with the following extract, selected almost at random:—

"THE NUTRITION OF PLANTS AND ANIMALS.

"The animal and vegetable kingdoms of nature are connected together in a beautiful system of mutual dependence, exhibiting a perpetual circulation of certain elements through both, the mineral kingdom being the point of departure and that also where the circulation terminates, to recommence unceasingly.

"Plants derive their nourishment exclusively from the mineral world. It is clear that the first plants must have done so, and although the decaying remains of former plants now contribute to vegetation, we shall see that they do so under mineral forms, and not essentially: they promote vegetation, but are not indispensable to it.

"The mineral food of plants, then, consists of carbonic acid, water, and ammonia, all of which are obtained from the atmosphere, and of sulphur (sulphuric acid), phosporus (phosphoric acid), alkalies, earths, salts, and metals, all derived from the soil. Without the aid of the matters derived from the soil, the most abundant supply of carbonic acid, water, and ammonia, is of no use. But if a soil contain these necessary substances, plants will thrive in it, even if they have no carbonic acid or ammonia furnished in the shape of manure beyond the usua atmospheric supply.

"During germination, oxygen is absorbed and carbonic acid produced: starch is transformed, probably by the action of diastase, or of an acid developed during germination, into sugar or dextrine, which, being soluble, are fitted for being conveyed to all parts of the plant. Meanwhile the azoto-sulphurised ingredients of the seed also become soluble, and with the sugar, &c. contribute to the formation of new parts, destined to collect food from the air or the soil.

"The leaves and roots, as soon as formed, absorb carbonic acid from the air and from the soil. Alkalies are at the same time taken up by the roots, and with their aid the carbonic acid, under the influence of light, is decomposed, its carbon being retained, while its oxygen is given off. At the same time, water, ammonia, sulphuric acid (or a sulphate), and phosphoric acid or phosphates are taken up, and their elements, along with the carbon, give rise to fibrine, albumen, caseine, &c.

"It is probable that the fixation of carbon is a gradual process, having successive stages; that the carbonic acid is first reduced or deoxidised so far as to yield oxalic acid; thus $C^2\ O^4 - O = C^2\ O^3$; that oxalic acid, with the acid of water, is further reduced, so as to yield malic or citric or tartaric acid: thus, $C^4\ O^6 + 2\ H\ O = C^4\ H^2\ O^8$; and $C^4\ H^2\ O^8 - O^4 = C^4\ H^2\ O^4$; this last formula doubled is that of anhydrous malic acid, $C^8\ H^4\ O^8$. From this and similar compounds, sugar, $C^{12}\ H^{12}\ O^{12}$, starch, $C^{12}\ H^{10}\ O^{10}$, gum, $C^{12}\ H^{10}\ O^{10}$, and woody fibre, $C^{12}\ H^9\ O^9$ or $C^{36}\ H^{22}\ O^{22}$ are easily deduced, by the addition of the elements of water and the elimination of oxygen. Thus, 3 eq. hydrated malic acid, $C^{24}\ H^{18}\ O^{30}$, plus 6 HO and minus O^{12}, is equal to 2 eq. dry sugar of grapes $C^{24}\ H^{24}\ O^{24}$. There is good reason, to think that the chief function of the alkalies in plants is to promote these metamorphoses.

"Water not only acts by its elements, but also as the indispensable solvent through which the whole food of plants, especially that derived from the soil, can alone enter them.

"From the above considerations we may draw several useful inferences.

"1. The presence of decaying vegetable matter in the soil promotes vegetation by furnishing a steady supply of carbonic acid gas. The proportion of decaying matter or humus must not exceed a certain limit; otherwise there is too much carbonic acid in the air of the soil, and the plant dies in such circumstances.

"2. The presence of decaying azoto-sulphurised matter in the soil is very advantageous, furnishing a supply of ammonia,

which is essential to vegetation, and is scantily supplied by the atmosphere.

"3. The supply of carbonic acid and of ammonia can only favor the development of vegetation in so far as alkalies and phosphates are supplied by the soil.

"4. Since all the azoto-sulphurised principles, albumen, fibrine, caseine, &c., contain sulphur and phosphorus, or rather phosphates, it is evident that seeds and such other parts of plants as contain these principles can only be developed in so far as the soil contains alkaline or earthy phosphates and sulphuric acid or sulphates.

"5. If the soil is rich in alkalies, sulphates, and phosphates, and if it also contain soluble silicates, essential to the stem of the grasses and cerelia it is fertile for all nutritious crops; and such crops will, in that case, derive from the atmosphere alone all the carbon and nitrogen (carbonic acid and ammonia) they require, *provided time be allowed*. The advantage of decaying organic matter, or of manures containing ammonia, in such a soil, consists in shortening the time necessary for the development of the plant; a matter of the last importance in our uncertain climate, but of far less consequence in southern regions, where summer is perhaps twice as long.

"6. The ashes of wood, straw, leaves, &c., consisting entirely of matter extracted from the soil by the plants for the purposes of vegetation, must prove a most fertilising manure: and in all cases the ashes of any crop must be the best manure for that vegetable.

"7. But as the ashes of plants are represented by the excreta of the animals (or the ashes of these excreta) which fed on them, so the excreta of animals fed on turnips, hay, straw, potatoes, &c., must be the best manure for turnips, hay, corn, and potatoes, respectively.

"8. When by the addition, to an average soil, of guano, or of bone earth, a very heavy crop is obtained, say of wheat, we are not to expect that a repetition of the same treatment will produce the same effect. We must bear in mind that the presence of the increased supply of phosphates has enabled the plant to take up a much larger quantity than otherwise it could have done, of alkalies, silicates, and the other necessary minerals. Since we have not added those substances in our guano or bone earth, it may happen that the soil is exhausted of its whole actually available supply of them by that one crop, and that years may elapse before it becomes, in the course of nature, as fertile as before.

"9. The only certain rule is this: as far as possible to restore to the soil, in the shape of manure, exactly what it has lost in the crop; if the soil were originally fertile this will maintain its fertility, which will even be gradually augmented by the action of the weather on the subsoil.

"10. With a view to this, every particle of solid or liquid manure, especially human, should be preserved with the utmost care. It is their mineral elements which are the most valuable; and since these have all come from the soil, in preserving them for manure we are only restoring what we have taken away. This has long been systematically done in China, and is also generally practised in the Netherlands; but in this country the waste of valuable manure is lamentable, and is necessarily followed by a slow but certain deterioration of our soil and crops, which we are now endeavouring to remedy by the expensive, precarious, and partial measure of importing bone earth and guano. But guano will, ere long, be exhausted; and when other countries know the real value of their bone earth, they will not willingly part with it, at all events not except at a very high price.

"11. If a soil is not fertile generally, it must be deficient in most of the substances above alluded to; but if it yields good crops of one vegetable and not of others, it must be wanting in the characteristic mineral elements of the latter, which must then be supplied.

"12. The ashes of the plants being known, the fact that a certain vegetable, cultivated or wild, thrives in any given spot, furnishes us with an analysis of the available or soluble elements of the soil, and enables us to direct our measures of improvement according to the crop we wish to raise.

"Although certain bases characterise the ashes of certain plants, as potash does those of turnips and potatoes, and lime those of peas, beans, &c.; yet in many cases, one base may be substituted for another, as soda for potash, or magnesia for lime.

"It is maintained by some that carbon is introduced into plants, in part, as humus, humic acid, or humate of ammonia, dissolved in the juice, and derived from the mould in the soil. But there is no evidence that fertile soils contain humus in a form soluble in water, and the sap, when first entering the plant, is colorless, while all solutions of humus, &c., are brown. Besides, in forest land, which is not manured, the proportion of humus or of carbon in the soil, instead of diminishing, rather increases, while enormous quantities of carbon are removed annually in the shape of wood. Here, as in the case of the first vegetables, it is plain that all the increase of carbon must be derived from carbonic acid: since, if the plant does absorb humus or humic acid, or humate of ammo-

nia, which is not proved, it must give to the soil as much or more humus, &c., as an excretion from its roots.

"It has also been argued by some that plants may obtain their nitrogen, either directly by absorption of the nitrogen of the atmosphere, or by causing that gas to form ammonia, combining with hydrogen derived probably from water, &c.; or, lastly, from the decomposition of nitric acid, which acid is supposed to be formed in the atmosphere by direct combination of its elements. But no evidence has ever been given, either that plants can absorb nitrogen directly, or that they can cause the nitrogen of the atmosphere to combine with hydrogen. As for nitric acid, although traces of it have been observed in thunder-storms, it does not appear to be formed in sufficiently large quantity; and if it were, no proof has yet been offered that plants can derive their nitrogen from it. The action of nitrate of potash, or of soda, as manure, proves nothing, because it may be due to the alkalies alone, and probably is so, since they do not seem to act better than other salts of the same bases. Moreover, many plants, such as tobacco, and sunflower, contain much nitrate in their juices, and therefore appear rather to form nitric acid than to destroy it. On the whole, it appears nearly certain that ammonia is the only source of nitrogen in plants. It is self-evident that the atmosphere must contain ammonia, derived from the putrefaction of animal and vegetable matter, and also that, however small the *proportion*, the absolute quantity in the air at any one time must be sufficient for the supply of the vegetable world, and through it of the animal world, since all animals and vegetables ultimately putrefy, giving off their nitrogen in the form of ammonia. It is quite easy to detect ammonia in rain-water, by which means it is conveyed to the roots or leaves of plants; and it has also been proved that the juices of plants contain abundance of ammonia.

"The nutritious principles, albumen, fibrine, and caseine, are formed by plants alone from ammonia, sugar (or gum, starch, &c), sulphates, and phosphates. They pass into the body of animals, and are there converted into blood. As they cannot be formed nor exist without the phosphates, so by their means the animal body is supplied with bone earth, and with the soluble phosphates necessary for the other tissues.

"In the animal body, the leading and characteristic action is the absorption of oxygen and the oxidation of the tissues; that is, of carbon and hydrogen, so that animals take up oxygen, and give out carbonic acid. This is the reverse of what occurs in plants, which absorb carbonic acid, and give out oxygen.

"In the lungs, oxygen enters the blood, and is carried, apparently, by the agency of a compound of iron, to every part of the body. The oxidation of the effete, or worn-out tissues, which have meanwhile been replaced by the blood, takes places in the capillaries, and is, in all probability, the source of the animal heat. The final result of this oxidation is the production of a large quantity of carbonic acid and water, which are given off by the lungs and skin.

"Health, in the animal body, consists in the due balance or equilibrium between the oxidising, or destructive agency of the atmosphere, and the process of nutrition by which the other is compensated.

"Since the nutritious, or blood-forming elements of food, have the same composition as the albumen, fibrine, &c., of the tissues—indeed, in the case of animal food, are identical with them—we may consider the process of oxidation and destruction, either as affecting the food directly, or, what is more probable, those portions of the tissues which, having performed their functions, are to be thrown off. But we must not forget that in the herbivora, a great part of the combustion which yields the animal heat is carried on at the expense of those parts of the food which cannot form blood—namely, sugar, starch or gum, fat, &c.

"The fat of the animal body is, at all events in great part, derived from the non-azotised elements of the food when these are in excess, and oxygen is deficient. In these circumstances, the deficiency of oxygen is supplied at the expense of sugar, starch, or gum, which, by losing oxygen, gives rise to fat; for the proportions of carbon and hydrogen in sugar, &c., and in fat, are exactly the same, that of oxygen alone being different. Hence the conditions favorable to the formation of fat are, abundant farinaceous food, and rest, that is, defective aëration, as is seen in stall-fed animals, and in the fattening of geese fed on maize and deprived of the power of locomotion. The formation of wax, a species of fat, from sugar by the bee, is another example.

"It is very interesting to remark that the composition of the chief elements of the bile, and the urine, bears a close and simple relation to that of the blood or tissues. If the formula of urate of ammonia be added to half that of choleïc acid, the sum represents the composition of blood or flesh, or differs from it only by a very little water and oxygen, substances which take part in almost every chemical change in the body, water being always present in the body, and oxy-

gen being introduced through the lungs. Now, urate of ammonia is the urine of birds and reptiles, and represents that of the mammalia; for uric acid, when further oxidised, yields urea and carbonic acid, while urea, with the elements of water, passes, as in putrefying urine, into carbonate of ammonia. As for the bile, the other product of the destruction of the tissues (at least, this is its origin in the carnivora, and partly in the herbivora), it undergoes resorption in the intestines, and is finally oxidised into carbonic acid and water, thus contributing to keep up the animal heat. The final result is, that the tissues, which may be supposed, with the addition of a little oxygen, to be resolved into choleic acid (bile) and urate of ammonia (urine), are entirely converted, or oxidised, into carbonic acid, water, and ammonia.

"The food of animals may thus be said to be literally burned in their bodies, and this, as in the case of other combustibles, for the purpose of producing heat. The gaseous products of the combustion are sent off through the skin and lungs, while the smoke, soot, and ashes are represented by the excrements and urine.

"The food required by animals must bear a certain relation to the waste of matter, and to the heat required. Thus, a hard-working man, in whom the change of matter is rapid, requires much more food (blood, or proteine compounds) than a sedentary person; and in cold climates a much larger quantity of food rich in carbon, especially fat, blubber, and similar matters, is necessary than in warm climates, where, indeed, such food excites invincible repugnance. Any misproportion in the amount or nature of the food has a tendency to induce disease. Thus, Europeans, who often eat and drink, as at home, when they go to tropical countries, pay the penalty of their ignorance in the very frequent liver complaints observed among them. For the same reason, hepatic disease is more frequent during summer than during winter.

"After death, the animal body is slowly, but surely, resolved into the ultimate products of putrefaction—namely, carbonic acid, water, and ammonia, which rise into the atmosphere, having completed the circuit through which we have traced them, in order to recommence it by once more contributing to the growth of plants on which animals will again feed.

"The bones of dead animals are also by degrees restored to the soil from which they were taken; and nothing is finally lost. Even the rich manure which we recklessly cast into the sea serves as manure for sea plants, on which are fed fish, which, in their turn, become food for sea fowl, the excrements of these last, in the shape of guano, returning to fertilise the fields from which their mineral elements were perhaps originally taken.

"It is evident that there must be a balance or equilibrium kept up between the animal and vegetable worlds, for the atmosphere in which both live does not, at least perceptibly, change in its composition. There is no appearance either of increase or diminution in the proportion of oxygen, or in that of carbonic acid. Yet we know that animal life tends directly and powerfully to increase the proportion of carbonic acid, and to diminish that of oxygen, while the tendency of vegetable life is exactly the reverse. Hence if, in any quarter, population or animal life extends, a corresponding augmentation of vegetable life must somewhere be the result; or if, by cultivation, the amount of vegetation in any quarter is increased, the inevitable consequence must be an extension of animal life, otherwise the air would become richer in oxygen. As both carbonic acid and ammonia are partly supplied to the atmosphere from the earth itself, not merely from the decay of organic matter at the surface, but from great depths, as, for example, in hot springs, so we may conceive the absolute amount of carbonic acid and ammonia on the surface of the earth and in the atmosphere to be somewhat greater now than some thousand years ago. If not, then the increase of animal and vegetable life in one part of the globe must necessarily cause a diminution of one or both in some other part, since, at all events, the atmosphere becomes neither richer nor poorer in carbonic acid and ammonia. Should an addition of these substances be made, they would instantly be appropriated by the vegetable kingdom, and converted into food for animals, of which an additional number would soon be produced by the increased supply of food; and thus the aggregate amount of animal and vegetable life might be increased, while the composition of the atmosphere remained unchanged, the vegetable world exactly balancing the animal, because each produces the food of the other, as the necessary result of its own existence.

"Such is a very brief and general sketch of the chemistry of animal and vegetable life, as far as we are at all acquainted with it. It is only within a very short time that this department of science has been properly cultivated, but the results already obtained are most important and highly encouraging. When we reflect that the processes by which carbonic acid, water, ammonia, and the salts of the soil, are made to assume the forms,

first of vegetable and then of animal tissues, as well as those by which these tissues are again resolved into the elements of which they were formed, are, and must be, purely chemical processes of combination and decomposition, governed by the laws of chemistry as ascertained by observation and experiment, but modified by the vital force, it is easy to see that from the assiduous study of the chemical changes going on in plants and animals, in health and in disease, we may confidently expect the most beneficial results. All that has hitherto been done has only pointed out the path to be followed in order to obtain valuable and permanent results. But enough has been done to satisfy all who are acquainted with the actual state of physiology that, henceforth, it is chiefly to chemistry that we must look for the extension and improvement of physiological science. Already a knowledge of chemistry is admitted to be indispensable to the physiologist, and before long this opinion will be so far acted on that no one who is not well versed in chemistry will venture to write on physiological subjects."

CARRARA WATER.

As journalists devoted to the cause of chemistry, we feel at all times great pleasure in observing a successful application of our favorite science to the every-day purposes of life—much as chemistry has benefited mankind we know that this is as nothing to her unlimited resources; guided by such feelings it is with no ordinary satisfaction that we hail the introduction of carrara water to the notice of our numerous readers. Founded upon a careful consideration of principles purely scientific, and based on the strongest inductive reasoning, the invention of this invigorating and refreshing beverage is a real triumph to chemistry, with which chance or accident could have no connection, since the first part of the process employed in the formation of this water actually produces an effect the reverse of that which is ultimately obtained by merely continuing the same operation, as must be at once obvious by considering the mode in which the carrara water is manufactured; thus lime water, made with lime resulting from the decomposition of the purest carrara marble, is subjected to the action of pure carbonic acid gas under great pressure; the whole of the lime is precipitated as a carbonate by the first portions of gas which pass into the water, but this carbonate is instantaneously converted into bicarbonate of lime, and redissolved by the remaining par of the carbonic acid, and a bright, transparent, colorless fluid is formed, on which Mr. Maugham, its inventor, has bestowed the appellation of carrara water, from the name of the marble with which it is made. As thus prepared, carrara water is by far the most delicious effervescing drink with which we are acquainted, greatly surpassing soda water and other beverages of that kind in its power of retaining carbonic acid after it is drawn. We have drank it largely ourselves, and can strongly and unhesitatingly recommend it to our readers and the public at large; its medicinal virtues have been amply testified by Dr. Barham and other eminent physicians and surgeons; and "there needs no ghost to come from the grave to tell us" that ere long it will be in common use from the Land's End to John O'Groat's. In the mean time we heartily congratulate the talented inventor of the carrara water, and wish him all that success which his genius and perseverance so richly merit.

SOCIETY OF ARTS.

Amongst the subjects for which rewards were adjudged by the Society, and presented by H.R.H. Prince Albert at the late distribution, was a very ingenious though simple machine, called Tomes' Patent Dentifactor, or Dental Carving Machine, the invention of Mr. Tomes, of Mortimer-street. The patentee explained to His Royal Highness that the invention embraced two subjects in the manufacture of artificial teeth. First, a peculiar moulding composition in which, by the aid of a cast of the mouth, he makes a perfect model of the required teeth, and that should this composite model on trial perfectly fit the mouth, he fixes it with the material chosen for constructing the teeth in the *dentifactor*, which forms the second subject of the invention. It was sufficiently apparent in viewing the machine in the model-room, that by its action a perfect copy of the composite model could be obtained far exceeding in accuracy any work done by hand.

Mr. Tomes received from His Royal Highness, amid considerable applause from the large company assembled, the well-merited gold medal.

Notice.—*All Communications and Books for Review must be addressed* "To the Publisher of The Chemist, 310, Strand, London." *Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

MEMOIR ON THE PROTOSALTS OF MERCURY, AND ON THE AMMONIACAL PRODUCTS WHICH RESULT FROM THEM.

BY M. JULES LEFORT.

This work comprises the study of several protosalts of mercury. I apply myself more especially to the examination of compounds which have a pharmaceutical application; but as the comparative study of different salts of the same metallic series frequently elucidates the constitution of each of them, I have included in this memoir some salts which have not as yet received any application. Thus, by the side of the nitrates formed by the protoxide of mercury and of the products which result from the action of ammonia on the same salts, I have placed a new examination of the *carbonate, nitrate, oxalate, iodate,* and *acetate of protoxide of mercury.*

I have estimated, as far as possible, the greater number of elements contained in these different salts. I adopted, for the determination of the mercury, the analytical process published by M. Millon, introducing, according to each saline species, some slight modifications.

The formulæ which represent these different salts have been calculated according to the new equivalent of mercury, 1250·9, recently declared by MM. Erdmann and Marchand.

The determination of the nitrogen was particularly necessary for fixing the formulæ which express the composition of the nitrates. I found the apparatus employed by MM. Reiset and Millon well adapted for this purpose.

As the analysis of the water, at the same time as that of the nitrogen and the mercury, was the only means of removing some doubts which existed in my mind respecting the formulæ of the nitrates of mercury, I applied myself to making a correct estimation of it.

I was not able to separate the water from the mercury, but I succeeded very well in simultaneously condensing the mercury and the water. As the proportion of the mercury was very accurately known to me, I obtain the weight of the water by subtracting that of the mercury.

For condensing the water and the mercury, the apparatus applied to the estimation of the mercury receives a current of pure and dry carbonic acid gas, and a tube containing weighed chloride of calcium is adapted to the extremity of this apparatus. The mercury and water arising from the decomposition of the nitrate passes either into the vessel provided for condensing the mercury, or into the tube containing chloride of calcium.

In several cases, I have had occasion to doubt whether the compounds of protoxide of mercury are true combinations or simple mixtures. The question is one of extreme delicacy. I hardly dare to affirm that I have entirely solved it. Nevertheless I have recognised that, in rubbing for a few moments, the products obtained by protoxide of mercury on a sheet of gold, the gold turns white with some and remains intact with others. When the plate of gold turned white, I hastened to detect, by the employment of the common means, the presence of metallic mercury and of the salts of the binoxide. It appeared to me that this reagent strikes less than the others at the constitution of the mercurial compounds.

CARBONATE OF PROTOXIDE OF MERCURY.

The directions which have hitherto been given for the preparation of carbonate of protoxide of mercury are very inaccurate; they give a product of variable appearance and composition, which powerfully whitens the sheet of gold.

It would be useless here to recapitulate those directions, which are given in all chemical works.

A pure, homogeneous, and uniform product is obtained by operating in the following manner:—

A weak solution of nitrate of protoxide of

* *Journal de Pharmacie*, July, 1845.

mercury is poured, drop by drop, into a recent solution of bicarbonate of potassa made with cold water. A precipitate is formed, of a whitish yellow, which cannot well be washed except with water impregnated with carbonic acid.*

After sufficient washing, the product, which should be preserved from the light throughout these operations, is dried over sulphuric acid. This perfectly dry product constitutes an amorphous powder of a light yellow color, which does not whiten the sheet of gold, but which very rapidly turns black in the light, and acts immediately on the gold.

This salt is anhydrous; it retains only a fraction of interposed water, which it loses at about 212° F., in being decomposed. At 270° F. it disengages carbonic acid, leaving binoxide of mercury and metallic mercury. Cold water does not alter it very quickly, but boiling water instantaneously destroys it; all the acids disengage carbonic acid from it.

ESTIMATION OF THE MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	1·387	1·200	86·51
II.	1·231	1·066	86·59

Calculation.. 86·95

The salt was submitted to another analytical experiment, which consisted in measuring the gases evolved during a complete decomposition and weighing the metallic residue. The gaseous volume, expressed by a mixture of oxygen and carbonic acid, accorded with the loss in weight represented by the following numbers;—

Salt employed.	Loss of oxygen and carbonic acid.	In hundredths.
0·896	0·113	12·61

Calculation.. 13·04

Formula : CO^2 HgO.

NITRITE OF PROTOXIDE OF MERCURY.

This salt, which Dr. Kane regarded as basic nitrate of protoxide of mercury, is formed in certain circumstances when mercury is attacked by concentrated nitric acid.

Three principal preparations may be distinguished—

1st. By putting into a flask equal equivalents, of nitric acid with 4½ equivalents of water, and metallic mercury, and by submitting the mixture to the action of heat, by the evaporation of the liquor, a yellow salt of crystalline appearance, which consists of pure nitrite is obtained; it is washed with water, which does not decompose it.

2nd. An almost syrupy solution of uncrystallisable binitrate of mercury, diluted with 3 or 4 volumes of water, and put in contact with metallic mercury, deposits very abundant crystals of nitrite, sometimes mixed with nitrate of protoxide.

3rd. Finally the nitrates of protoxide of mercury, moderately heated in an alloy-bath, turn yellow and disengage water and red vapors; if the residue be redissolved in water slightly acidulated with nitric acid, so as to dissolve the subsalt of binoxide which has been formed, nitrite of protoxide of mercury is also obtained.

Nitrite of protoxide of mercury is sparingly soluble in water and diluted nitric acid; nitric acid poured on it in excess, immediately converts it into nitrate.

Exposed to the action of heat, it resists the temperature of 535°F. At 295°F. it begins to disengage red vapors.

ESTIMATION OF MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	2·007	1·636	81·15
II.	1·4725	1·2025	81·66

Calculation.. 81·24

In estimating the mercury of the nitrite and nitrates, it is indispensable to put copper turnings into the decomposition tube, in order to completely destroy the oxygenous compounds of nitrogen.

ESTIMATION OF THE NITROGEN.

	Salt employed.	Nitrogen reduced in weight after correction.	In hundredths.
I.	0·8120	0·0473	5·83
II.	0·8705	0·0482	5·93

Calculation.. 5·75

Formula: NO^2 HO.

The presence of nitric acid in the foregoing salt may very easily be detected by destroying it with an excess of hydrochloric acid, and afterwards filtering the liquors; an abundant deposit of protochloride of mercury is formed, and the filtered liquor remains saturated with nitrous acid which hydrosulphuric acid, dissolved in water, enables us to detect. With the least traces of nitrite, hydrosulphuric gives a very manifest deposit of sulphur, whilst a nitrate submitted to the same treatment leaves the hydrosulphuric acid perfectly intact. It is

* It is indispensable to pour the nitrate into the bicarbonate. By acting in the inverse manner, not a trace of carbonate would be obtained, and this salt would be replaced by a nitrate of peculiar composition, which will be described further on.

probable that this reaction misled Dr. Kane, who estimated the mercury of the nitrate in the state of sulphur; his calculation related to a mixture of sulphur and sulphuret of mercury.

OXALATE OF PROTOXIDE OF MERCURY.

Oxalate of protoxide of mercury is obtained by mixing two solutions of oxalic acid or binoxalate of potassa and nitrate of protoxide of mercury. The white precipitate which is formed is perfectly insoluble in water, which does not decompose it.

The oxalate is not hydrated as asserted by M. Burchardt; only it retains, like most of the anhydrous protosalts of mercury, a small fraction of water from which it cannot be freed by drying over sulphuric acid, but which the heat of 212°F. is sufficient to remove.

Gradually exposed to the action of heat in an alloy-bath at from 230°F. to 250°F., oxalate of protoxide of mercury is decomposed without detonation; but, when suddenly heated, it produces a very powerful explosion.

It is important to protect it from the rays of light, which decompose it with great facility.

ESTIMATION OF MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	2·366	1·923	81·29
II.	1·681	1·377	81·90

Calculation.. 81·66

Formula: $C^{12}O^8Hg^2O$.

In making this analysis, it is important to mix the oxalate with eight or ten times its weight of caustic lime; without this precaution, the oxalate would soon explode when heated.

IODATE OF PROTOXIDE OF MERCURY.

This salt is obtained in a very pure state by pouring nitrate of protoxide of mercury into iodic acid, or into iodate of soda in solution. It occurs under the form of a white powder insoluble in water.

This salt is anhydrous; it retains a trace of water which may amount to two or three thousandths, and is dispelled at 212°F. Towards 480°F., it is decomposed into biniodide of mercury, binoxide of mercury and oxygen; boiling water does not decompose it. Cold nitric acid has no action on it, but if the temperature be raised, red vapors appear, and iodate of binoxide of mercury is then formed.

Hydrochloric acid decomposes it into chloride of iodine and bichloride of mercury, and chlorine is disengaged.

ESTIMATION OF MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	0·699	0·373	53·35
II.	1·198	0·644	53·75

Calculation.. 53·42

In making this estimation of mercury before putting the salt into the decomposition tube, it is necessary to put into the tube a mixture of potassa and lime reduced to powder, so as to retain the iodine which would volatilise at the same time as the mercury.

ESTIMATION OF THE OXYGEN.

	Salt employed.	Loss of oxygen in weight.	In hundredths.
I.	0·738	0·092	12·46
II.	1·972	0·257	13·02

Calculation.. 12·82

Formula: IO^5Hg^2O.

ACETATE OF PROTOXIDE OF MERCURY.

Of all the protosalts of mercury, the acetate is one of those whose history has been best traced; therefore, I shall have but little to add to what is already known.

Prepared with acetic acid, and nitrate of protoxide of mercury, the acetate is presented under the form of beautiful white pearly scales; prepared by the double decomposition of the acetate of potassa and nitrate of protoxide of mercury, it constitutes a white precipitate. It has been recommended to prepare this salt by means of acetate of binoxide of mercury and metallic mercury; this process does not possess any real advantage, for it is necessary to maintain the ebullition for a long time, in order to bring the acetate of a binoxide to the state of acetate of protoxide.

ESTIMATION OF THE MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	2·177	1·692	77·72
II.	1·026	0·793	77·29

Calculation.. 77·22

Formula: $C^4H^3O^3$, Hg^2O.

The decomposition-tube in which the estimation of the mercury contained in the acetate is to be made, should be filled, the first half with metallic copper and the second half with caustic lime, in order to completely destroy the acetone or other products which may be formed during the decomposition.

NITRATES OF PROTOXIDE OF MERCURY.

I will confine myself, in this article, to furnishing the most precise indications concerning the principal nitrates of protoxide of mercury; it would be much too long and almost useless to discuss each of the circumstances in which I deviate from the direction hitherto admitted.

The nitrates of protoxide of mercury which I have been able to isolate, are represented by the following formulæ:—

Biatomic acid nitrate, $2NO^5,2Hg^2O,5\frac{1}{2}HO$.
The same salt dihydrated..... $2NO^5,2Hg^2O,HO$.
Intermediate nitrate. $2NO^5,2Hg^2O,2\frac{1}{2}HO$.
Biatomic neutral nitrate $NO^5,2Hg^2O,2HO$.

All these nitrates may be combined in a group at once polyatomic and hydric $(Hg^2O)2HO$.

This group would be subject at the same time, in its union with nitric acid, to the rules of polyatomic bases and of hydric bases. Consequently, the following arrangement must be given to the foregoing formulæ:—

I. Biatomic acid nitrate .. $NO^5(Hg^2O)$, $2HO + NO^5,4\frac{1}{2}HO$.

II. The same salt dihydrated .. $NO^5(Hg^2O),2 + NO^5HO$.

III. Intermediate nitrate.. $NO^5(Hg^2O)$, $2NO^5HO + 2[NO^5HO,(Hg^2O),2HO]$.

IV. Biatomic neutral nitrate .. $NO^5HO(Hg^2O)2HO$.

The intermediate represents 1 equivalent of nitrate, number II., and 2 eqs. of nitrate, number IV.

When the nitric acid is very concentrated, a mixture of bisalt and protosalt of nitrate and nitrite is always obtained, although the mercury is in excess. The binitrate may, it is true, be brought entirely to the state of protosalt by diluting the liquor and boiling it for a long time over mercury; but this conversion takes place only after very long boiling.

In operating without the aid of heat, by taking nitric acid with $4\frac{1}{2}$ equivalents of water, diluted with half its bulk of water, or else with heat, acid diluted with 3 volumes of water, protosalt of mercury is always obtained, provided that the metal be in excess; nothing but protosalt is formed from the commencement of the operation.

The study of the different nitrates of protoxide will show in what circumstances each may be obtained.

BIATOMIC ACID NITRATE OF PROTOXIDE OF MERCURY.

This salt is obtained by dissolving, in pure dilute nitric acid, all the nitrates which I describe further on. It is also obtained when an excess of mercury is treated without the aid of heat, with nitric acid with $4\frac{1}{2}$ eqs. of water, diluted with 1 or 2 volumes of water, or else by boiling, till reduced to one half, nitric acid with $4\frac{1}{2}$ eqs. of water, diluted with 10 volumes of water, or an excess of mercury.

The saline solution, which is formed in the latter case, should also be evaporated to about one-half. The salt which is deposited by the cooling of the liquor, crystallises in obtuse, colorless rhombohedrons, soluble in water, with a slight residue.

Exposed to the air, this salt is dishydrated with great facility; therefore it is proper, when it is wished to keep it, to press it rapidly between leaves of blotting-paper, and to put it into a flask which may be hermetically sealed.

Submitted to the action of heat, it fuses at 160°F.

ESTIMATION OF THE MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	2·6625	1·861	69·98
II.	1·794	1·239	69·06

Calculation.. 69.69

ESTIMATION OF THE NITROGEN.

	Salt employed.	Nitrogen reduced in weight after correction.	In hundreths.
I.	1·728	0·0847	4·90
II.	1·508	0·0730	4·83

Calculation.. 6·04

Formula: $NO^5,(Hg^2O)2HO + NO^5,4\frac{1}{2}HO$.

When the biatomic acid nitrate is exposed over sulphuric acid, until its weight is invariable, it is found that it lost $4\frac{1}{2}$ eqs. of water, or 7·13010.

ESTIMATION OF WATER.

	Salt employed.	Water removed.	In hundredths.
I.	2·620	0·190	7·25
II.	1·153	0·080	6·93

Calculation.. 7·13

ESTIMATION OF MERCURY.

	Salt employed.	Mercury obtained	In hundredths.
I.	1·797	1·345	74·86
II	19·14	1·424	74·39

Calculation.. 74·99

ESTIMATION OF NITROGEN.

Salt employed.	Nitrogen reduced in weight after correction.	In hundredths.
1·089	0·0536	4·98

Calculation.. 5·31

Formula: $NO^5(Hg^2O), + NO^5,HO$.

INTERMEDIATE NITRATE OF PROTOXIDE OF MERCURY.

When one part of nitric acid, with 4½ eqs. of water, diluted with five parts of water, is boiled for about five or six hours with an excess of mercury, care being taken to renew the water in proportion as it evaporates, the salt which is obtained by the cooling of the liquor is presented in rhomboïdal prismatic crystals, transparent and unalterable in contact with the air.

If the ebullition has not been kept up long enough, crystals of biatomic acid nitrate are deposited at the same time as the intermediate nitrate.

ESTIMATION OF THE MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	2·692	2·139	79·75
II.	1·862	1·487	79·83
III.	1·555	1·240	79·42

Calculation.. 79·25

ESTIMATION OF NITROGEN.

	Salt employed.	Nitrogen reduced in weight after correction.	In hundredths.
I.	1·381	0·0572	4·14
II.	1·372	0·0544	3·96
III.	1·371	0·0556	4·06

Calculation.. 3·73

ESTIMATION OF WATER.

	Salt employed.	Water and Mercury.	Mercury pr. ct.	Water to be deducted.	Water pr. ct.
I.	1·655	1·366	82·53	79·92	2·55
II.	2·056	1·700	82·66	79·98	2·88

Calculation.. 3·24

Formula: $NO^5(Hg^2O)^2,NO^5,HO + 2[NO^5 HO(Hg O)^2,HO]$.

NEUTRAL BIATOMIC NITRATE OF PROTOXIDE OF MERCURY.

This salt is formed when weak nitric acid and an excess of mercury are exposed for some time to a sand-bath, heated to from 164°F to 176°F.

But it is also formed in two remarkable circumstances:—

1. If a solution of bicarbonate of potassa be poured into proto-nitrate of mercury, until a precipitate is formed, the liquor separated from this precipitate deposits very bulky crystals, which consist solely of biatomic nitrate.

2. When the product which results from the action of concentrated nitric acid on the excess of mercury is evaporated to dryness, and the residue redissolved in boiling water, the latter deposits on cooling crystals of neutral biatomic nitrate.

The biatomic nitrate is in oblique prismatic crystals sometimes bulky, sparingly alterable in contact with the air.

ESTIMATION OF THE MERCURY.

	Salt employed.	Mercury obtained.	In hundredths.
I.	1·225	1·0005	81·67
II.	2·437	1·999	82·02
III.	2·815	2·299	82·03

Calculation.. 81·93

ESTIMATION OF THE NITROGEN.

	Salt employed.	Nitrogen reduced in weight after correction.	In hundredths.
I.	1·489	0·0523	3·51
II.	1·157	0·0375	3·10

Calculation.. 2·90

ESTIMATION OF THE WATER.

	Salt employed.	Water and mercury.	Mercury to be deducted.	Water pr. ct.
I.	2·685	2·2975	81·93	3·63
II.	2·310	1·972	81·93	3·44

Calculation.. 3·68

Formula $NO^5 HO (Hg^2 O)^2, HO$.

The intermediate and neutral biatomic nitrates are decomposed by cold water as well as by hot water. The acid biatomic nitrate is also decomposed by water, but in smaller proportion. These phenomena of decomposition have misled a great number of chemists, who have described as distinct salts the different degrees of this decomposition. As soon as water has acted on the first two basic salts, the residue powerfully whitens a sheet of gold. Rose (Berzelius, *Annual Report*, 1843) has perfectly demonstrated that the salts of protoxide of mercury, under the influence of boiling water, give metallic mercury and binoxide of mercury.

ACTION OF ALKALIS AND CAUSTIC AMMONIA ON THE PROTO-SALTS OF MERCURY.

M. Guibourt was the first who announced that protonitrate and protochloride of mercury, in presence of caustic potassa or soda, gave a mixture of metallic mercury and binoxide of mercury, instead of protoxide of that metal, as was previously supposed.

We have several times recurred to this reaction of the alkalis, and with different conclusions. I placed myself in the most proper conditions for furnishing protoxide of mercury, and I have successively acted on a very great variety of salts, *carbonate*, *oxalate*, *iodate*, *acetate*, &c. The product which I obtained always whitened the sheet of gold, and always acted like a mixture of binoxide and metallic mercury.

Ammonia appeared, hitherto, to exert an action very distinct from that of the alkalis, and very recent works have assigned particular formulæ to the compounds which result from the action of caustic ammonia on the protosulphate, protochloride, and protonitrate of mercury.

The experiments and analyses which I have made lead me to believe that ammonia differs from potassa and soda only in the peculiar action which it exerts on the bisalts of mercury. When caustic ammonia, diluted or concentrated, acts on a salt of protoxide of mercury, the black or grayish product which is formed possesses the property of whitening a sheet of gold; there are also observed in it all the characters which belong to the ammoniacal bisalts of mercury, which may be formed by virtue of the peculiar reaction of ammonia on the bisalts of mercury.

Protochloride of mercury gives only with caustic ammonia a mixture always of the same constitution, but this uniformity of composition is perfectly explained by the complete insolubility of the white precipitate, which may be said to equal that of metallic mercury.

With all the other salts, the proportion of mercury increases in the ratio of the solubility of the ammoniacal bisalt, either in water or in caustic ammonia.

Thus, by making ammonia in excess and in a highly concentrated state act on the precipitate formed by protosulphate of mercury, liquid mercury is obtained as a residue.

With protonitrate of mercury ammonia furnishes a mixture whose proportions vary with extreme facility. As the products which result from this reaction have frequently been employed in medicine, under the name of Hahnemann's soluble mercury, I have established by analysis the variations which the composition of this product may present, however little the circumstances of preparation may be modified.

It is sufficient, as we shall see, to operate at from 32°F. to 77°F., and to wash more or less, to obtain in the mixture very different proportions of mercury, even in the following, with the exceptions of washing and temperature, the conditions described in pharmaceutical works.

ANALYSIS OF HAHNEMANN'S SOLUBLE MERCURY.

Prepared at 32°F. and washed 8 times.	Prepared at 32°F. and washed 16 times.
Mercury per cent. 83·42	Mercury per cent. 89·47
Prepared at 77°F. and washed 8 times.	Prepared at 77°F. and washed 16 times.
Mercury per cent. 84·94	Mercury per cent. 91·11

OUTLINES OF A NATURAL SYSTEM OF ORGANIC CHEMISTRY.*

BY DR. KEMP.

The principles on which Dr. K. proposes to establish his system are founded on the assumption that all organic bodies are derived from some compounds which have either formed portions of living organs, or been produced by their agency. Thus, assuming that all organic bodies of which nitrogen forms no part have been originally derived from starch, and that those, on the other hand, in which nitrogen is an essential element, proceed from protein, he would have two general classes, amylogenic and proteïnogenic. These classes are divided into orders, accordingly as the products are the result of natural causes or of artificial agents; genus being determined by the addition or subtraction of the element of water, and species by the condition of the compound as it regards oxygen and carbon.

ON ANIMAL HEAT.†

BY JUSTUS LIEBIG.

If we determine the quantity of oxygen which an animal consumes in a given time in the act of respiration, and if we calculate the amount of heat which this oxygen should have developed if it had been directly combined, and in the proportions found with

* British Association.

† *Annalen der Chemie und Pharmacie.*

carbon to form carbonic acid, and with hydrogen to form water, and if this quantity of heat be compared with that which the animal yields in the same time to surrounding bodies, we find a great difference. If we establish the quantity of heat abandoned by the animal = 100, the quantity of heat which belongs to the oxygen (converted into carbonic acid) is, according to the investigations of Dulong and Despretz in the following ratio with this number :—

	Minimum.	Maximum.
Dulong	69	80
Despretz....	74	90

Thus, according to these numbers, the animal body develops from $\frac{1}{10}$ to $\frac{2}{10}$ more heat than it should yield according to the hypothesis which would admit the combination of oxygen with certain principles of the animal body, and its conversion into carbonic acid and water, as the sole source of the heat disengaged. It is owing to these investigations that physiologists and many chemists have admitted, in the organism of the animal another source of heat, serving, with the oxygen absorbed, to maintain the temperature of the animal.

I shall show further that the supposition of the existence of a cause of the formation of heat in the animal body, inexplicable by known physical laws, is in contradiction to the most positive facts; and I shall show that the loss of heat which an animal undergoes is in a direct ratio with the consumption of oxygen in the same time, and that the quantity of heat yielded by the animal is exactly similar to that developed by the oxygen consumed by the animal, if it be directly converted into carbonic acid and water.

The knowledge of the number of degrees of heat which a known weight of water receives owing to the combustion of a given weight of carbon and hydrogen, or of the conversion of a given quantity of oxygen into carbonic acid and water, is the basis of the calculation of the quantity of heat which should, in the act of respiration, be attributed to the conversion of the oxygen absorbed into carbonic acid and water.

The body, the heat of whose combustion is to be determined, is burned in a metallic vessel, surrounded on all sides with a known weight of water. The weight of the body and the temperature, as well as the water, before and after the experiment, are known, and it is easy to calculate, according to the increase of temperature, how many degrees of heat the water and the vessel have increased at the end of the experiment. The quantity of heat yielded by an animal to the surrounding bodies, in a given time, is determined by a perfectly similar process. The animal is placed in a metallic vessel, surrounded with water of a known temperature and weight. The number of degrees of heat which the water has received is determined with accuracy, and by the determination of the quantity of carbonic acid produced in the same time, as well as by that of the loss of oxygen which the air has undergone, or of the oxygen gas which the animal has consumed, without formation of carbonic acid, we have all the data ready for expressing in numbers the quantity of degrees of heat produced in the animal body by the transformation of oxygen into carbonic acid and water, and which it has yielded to the surrounding bodies; for it is positively known, by the direct determination of the heat of the combustion of carbon and hydrogen, the number of degrees of heat which correspond to a known weight or volume of carbonic acid produced, and which are developed by a known weight or volume of oxygen in its transformation into water.

If we combine carbon and hydrogen in an appropriate apparatus, the surrounding bodies receive a certain number of degrees of heat; it now becomes a question whether a given quantity (by weight) of carbon and hydrogen furnishes more or less heat by its *direct* than by *its indirect* combustion, when an equal weight of these two elements is converted, in the body for example, into carbonic acid and water. The experiments of Dulong and Despretz have affirmatively solved the last of these two questions: according to them a given quantity of carbon and hydrogen develops from $\frac{1}{10}$ to $\frac{2}{10}$ more heat if its transformation into oxygenous combinations takes place in the animal body than in its direct combustion.

The apparatus employed for the determination of animal heat and the heat of combustion have the same principle of construction. The same sources of error may, therefore, be admitted in them, and it is not surprising in these circumstances that so high a value attaches to these experiments.

I am far from depreciating the value which they should have in our eyes as positive observations; but it would, in my opinion, be acting with great precipitation to consider oneself authorised by these results in concluding upon a new source of heat which has nothing in common with the physical and chemical phenomena of the formation and development of heat; indeed, before arriving at this conclusion, there remains also a great many questions to be solved, and it would be without foundation before their solution.

It is clear that if the number of degrees of heat developed by a given volume of oxygen,

when it is directly converted into carbonic acid and water, is not the same, but is smaller than that developed by this same quantity of oxygen, when it is indirectly combined in the body of an animal to form carbonic acid, or with hydrogen to form water, the conclusion to be drawn from it at once was, evidently, that the quantity of heat developed by a combination which contains hydrogen and carbon (a principle of the animal body) in its conversion into carbonic acid and water is greater than the quantity of heat liberated in the combination of hydrogen and carbon isolated in the state of elements. If a proof had been furnished that the heat of the combustion of an animal combination is the same, or is not greater than that of its combustible elements, a second conclusion might have been deduced from the results of Dulong and Despretz, namely, that the heat of the combustion of carbon and hydrogen on which these two philosophers have based their calculations, contains an error.

These two conclusions find their application in the critical examination of the experiments of Dulong and Despretz.

Indeed, Dulong, in his calculation of the quantity of heat produced in the act of respiration at the expense of the oxygen which is converted into carbonic acid and water, took for his basis the determination of Lavoisier and Laplace, according to which 1 part by weight of hydrogen and 1 part by weight of carbon convert in their change into water and into carbonic acid by combustion, the first 28·5 parts and the second 96·5 parts of ice of 32°F. into as much liquid water of 32°F.

Now, as it was admitted at that period that in the fusion of 1 part of ice by weight, there is combined as much as is contained in one part of water at 75°C (=167°F.), it is clear that 285 parts of ice require as much heat for their fusion as is contained in 285 parts of water at 75°C., in all 285 × 75 = 21375°C. (=38507°F.) We calculate in the same way, according to Lavoisier and Laplace, the heat of the combustion of 1 part of carbon, according to the quantity of ice whose fusion it effects, and we arrive at 7237°C. (13058° 6′F.) Now, as 1 litre (or 1000 cubic centimetres) of carbonic acid contains 1 grain or 0·5404 grammes of carbon, and as this weight of carbon develops in its combustion 3911° 2′C. (7052°F.), sufficient, consequently, to raise the temperature of 3911·2 grammes of water one degree, it was easy in Dulong's experiments to deduce by calculation from the number of cubic centimetres of carbonic acid gas found in the air expired by the animal, that of the degrees of heat which the surrounding water has received by the formation of carbonic acid.

In its conversion into water 1 quart of oxygen gas (=1·4410 grammes) combines with 0·1802 grammes (=2 litres) of hydrogen gas, and, at the same time, according to Lavoisier and Laplace, 3851°C. (=6963° 8′F.) of heat set at liberty. It is clear that 1000 cubic centimetres of oxygen absorbed in the act of respiration correspond, in the supposition that it has formed water, to 3851°C. (=6963° 8′F.) of heat.

Such are the bases of the calculation concerning the heat of combustion in Dulong's experiments. As to his opinion, that the oxygen which has been absorbed in respiration without forming carbonic acid is combined with hydrogen to produce water, no doubt can be raised against it, since the organism contains, with the exception of carbon and a very small quantity of sulphur, no other element capable of combining with the oxygen.

Now, we know, at the present time, with the greatest certainty, that the data furnished by Lavoisier and Laplace concerning the heat of the combustion of carbon and hydrogen, and calculated according to the latent heat of water, which was admitted then, are incorrect; for the latter is not 75°C. (=167°F.), but, according to the last determinations of M. Regnault, confirmed by M. Provostaye, 79°C. (=174° 2′F.), consequently, 4 degrees more for each pound of ice melted. This makes, for 1 part by weight of hydrogen, 1140°C. (=2084° 8′F.); and for 1 part of carbon, 386°C. (=726° 8′F.) more of heat.

It is clear that the numbers indicated by Dulong in his experiments for the quantity of heat which is produced in the act of respiration at the expense of the formation of carbonic acid and water, rectified according to the last determinations of latent heat should be raised one-nineteenth.

Despretz took as the basis of his calculations concerning animal heat his own determinations on the heat of combustion. He found that 1 part, by weight, of carbon, develops 7815° C. (=14099° F.), and 1 part of hydrogen 23640° C. (=42584°F.) of heat. According to subsequent investigations, he indicates for the combustion of carbon the number 7912° C. (=14273° 6′ F.), and for that of hydrogen 20624° C. (=37159° 2′ F.). The latter is too much at variance with the former, as Lavoisier's experiments have shown, for any reliance to be placed in its accuracy. As to the heat of the combustion of carbon, the number 7912° C. approaches as near to the rectified combustion which Lavoisier found [7623°C. (=13753° 4′F.)] as can be expected in investigations of this nature.

In the interval, we have heard by the post-

humous writings of Dulong the results which he obtained in the direct determinations of the heat of combustion of carbon and hydrogen; they appear to resemble as much as possible his solution of the question concerning the origin of animal heat.

Dulong found that 1 litre of hydrogen at 32° F., and 0·76 in. pressure, gives by combustion the following units of heat:—3120° C.; 3118° C.; 3111° 3′ C.; 3075° C.

According to these numbers, 1 litre of oxygen develops by its conversion into water (in combining with 2 litres of hydrogen)—6240° C.; 6236° C.; 6217° C.; 6122° 6′ C.; 6150° 6′ C. units of heat; and from this is deduced by calculation for the quantity of heat developed by 1 part, by weight, of hydrogen, the number 34743° C.; the difference of the latter from that of Lavoisier and Laplace (21375° C.), which M. Dulong took as the basis of his calculations concerning animal heat, is extreme; if it be admitted that it is accurate, he must evidently raise by 2362° C. for each litre of oxygen disappearing, the number of the degrees of heat liberated in consequence of the formation of water.

But it is not permitted to doubt the accuracy of this number. These data have, indeed, been verified by Hess, and his experiments have demonstrated that 1 litre of hydrogen gas at 32° F., and 0·76 in. pressure, furnishes, as a mean of 5 experiments, 3114 units of heat, a number perfectly in accordance with the average of Dulong's determinations. We may remark, in passing, that it is somewhat below that found by Crawford.

The accordance of these experiments enable us, therefore, to regard it as certain that 1 part, by weight, of hydrogen develops, in combining with oxygen, from 34743° to 34792° C. of heat.

As to the heat of the combustion of carbon we are very far from being as certain as with regard to hydrogen.

Pure carbon (the diamond) has never been employed for this purpose. Dulong used wood charcoal, and Despretz the charcoal of sugar heated to whiteness. It is perfectly impossible to consider them as pure carbon. Wood charcoal contains ash and variable quantities of oxygen and hydrogen; sugar charcoal, which is much more pure, contains, after having been heated to whiteness (according to the determinations of Marchand and Erdmann), 0·6 per cent. of hydrogen, and 3·1 per cent. of oxygen, and after having been again exposed to the fire, 0·2 per cent. of hydrogen and 0·5 of oxygen. The determination of the heat of combustion of carbon presents a great obstacle: it is the difficulty of making the kinds of charcoal, however impure, enter into ignition. The charcoal cannot be heated to ignition in the apparatus for collecting the heat developed; and if it be heated to redness outside, a portion is burned before its arrival in the apparatus. All these determinations are, therefore, only approximations, and the numbers are in all cases lower than these which should have been obtained.

According to Despretz, the heat of the combustion of carbon is about 7912° C.

According to Dulong's first experiment, in which he used a less pure charcoal (a cylinder of wood charcoal), it was lower; according to his last, the heat of the combustion of carbon is calculated on a litre of the vapor of carbon.

8009; 7540; 8040; 7843 units of heat.

Mean..... 7858

Chemists are not, as is known, agreed as to the weight of a litre of vapor of carbon; the weight of the carbon contained in a litre of carbonic acid gas is 0·5404 grammes. Now, if we admit, with Berzelius and most German chemists, that this quantity of carbon expresses the weight of half a litre of vapor of carbon, 1 litre of vapor of carbon should weigh 1·0808 grammes, and these 1·0808 of carbon thus corresponds, according to the determination of Dulong, with 7858 units of heat.

Most of the French chemists admit that the quantity of carbon in a litre of carbonic acid expresses the weight of a litre of vapor of carbon; and it is scarcely possible now, since the death of Dulong, to decide whether the number of units of heat, which corresponds according to him to a litre of carbon in combustion, amounts to 0·5404 or to 1·0808 grammes of carbon. If the latter number be regarded as the weight of a litre, we deduce by calculation as the heat of combustion of 1 part, by weight, of carbon the number 7270; according to the other, it would be 14540. In one of the cases, the determinations of Dulong approaches those of Lavoisier, Laplace, and Despretz; according to the other determinations, the heat of the combustion of carbon is much higher. It appears, however, to result, from the determinations of Dulong concerning the heat of the combustion of several combinations which contain carbon and hydrogen, such as those of olefiant gas, ether, and alcohol, that the heat of combustion of carbon is in every case much higher than the number 7270, which he obtained by the direct combustion of the cylinders of charcoal.

We have every reason to believe that the heat of combustion of a combination which is composed of several combustible elements, cannot be higher than the quantity of heat

which the equal weight of these same combustible elements develops by itself, since it may be supposed that heat has already been developed in consequence of the union of these elements to form the combustible combination. But we see, on the contrary, that (with the single exception of marsh gas, to which the less importance is attached, as we know of no process for obtaining it pure) all the combinations of carbon hitherto burned give a higher heat of combustion for carbon. I do not wish to attach any importance to the heat of combustion which cyanogen gas may lead us to conclude upon (although this cannot be the most accurate), since, in Dulong's experiments, there is formed, at the same time, a quantity of nitrous acid which has not been determined; but the determinations of the heat of combustion of olefiant gas, alcohol, and ether present so astonishing an accordance, that they may, in my opinion, serve with much more certainty than any other process for establishing the heat of the combustion of carbon.

Indeed, there scarcely exist three bodies better adapted for this determination than olefiant gas, alcohol, and ether. 1 litre of vapor of alcohol contains the elements of 1 litre of olefiant gas and of half a litre of vapor of water; half a litre of the vapor of ether likewise contains 1 litre of olefiant gas and the elements of a quarter of a litre of vapor of water. It is needless to say that the elements of the latter can take no part in the combustion.

Now, it is clear that, if we know the heat of combustion of 1 litre of olefiant gas, vapor of alcohol and vapor of ether, we have the sum of the heat of combustion of 2 litres of hydrogen gas, and 1 litre of vapor of carbon, admitting, indeed, that 1·0808 grammes of carbon which are combined with 2 litres of hydrogen gas in olefiant gas, and in the vapors of ether and alcohol, express the weight of a litre of vapor of carbon. If, then, the heat of combustion which belongs to the hydrogen gas, be deducted from the sum of the heat of combustion of the elements, that of the vapor of carbon remains.

Olefiant gas, in five experiments, furnished Dulong by its combustion—

15264; 15228; 15576; 15051; 15501 units of heat.

Mean.....	15338
1 litre of vapor of alcohol, which contained 1 litre of olifiant gas, gave	14375
Half a litre of vapor ether, which likewise contained 1 litre of olefiant gas	16674

If we take the mean of these determinations of the quantities of heat furnished by olefiant gas supposed alone and in combinations, we obtain the number 15462
for the units of heat; if we deduct the heat of combustion of 2 litres of hydrogen 6213

there remains the heat of the combustion of 1 litre of vapor of carbon, or 1·0808 grammes of carbon 9249

In setting out from this number, the heat of combustion of 1 part, by weight, of carbon was established at 8558.

This number is higher than that given by all the direct determinations for the heat of the combustion of carbon; but certainly it approaches nearer to the truth, precisely because the errors of observation would be doubled supposing that we had separated in the olefiant gas the carbon and hydrogen, and burned each element separately.

I have already said that the heat of combustion of carbon determined with cyanogen gives a still higher number, and the heat of combustion of vapor of oil of turpentine furnishes, in Dulong's experiments, a new proof that the number which we have deduced from olefiant gas for the heat of combustion of carbon is the true one.

Indeed, 1 litre of vapor of oil of turpentine furnished, in Dulong's experiments, 70607 units of heat. Now, as 1 litre of oil of turpentine contains 8 litres of hydrogen gas and 5 litres of vapor of carbon (the specific gravity of the vapor is, according to M. Dumas, 4·764), we obtain the heat of combustion of 5 litres of vapor of carbon (or 5 times 1·0808 grammes of carbon), by deducting from the sum total of all the units of heat obtained, the heat of the combustion of 8 litres of hydrogen gas, that is to say 24848 units, there remains for the heat of the combustion of 5 litres of vapor of carbon 45758 units of heat, or, for 1 litre, 9151; this number approaches, as nearly as can be expected in experiments of this nature, the heat of combustion of carbon furnished by olefiant gas. Finally, it results from Dulong's experiments that alcohol, ether, and oil of turpentine were burned in the liquid state, so that nothing has to be deducted from these numbers for the latent heat of vapors.

Now, if we calculate by aid of these much more certain bases the quantities of heat which, in the experiments of Dulong and Despretz on animal heat, were produced at the expense of the conversion of oxygen into carbonic acid and water, we obtain the following results:—

DESPRETZ.

	Animal heat.	Calculation of Despretz.	Corrected. C=8558 H=34792.	Corrected. C=7912 H=34792.
1 Rabbit	100	90·4	107·48	102·04
2 Ditto.....	100	85·8	101·74	96·76
3 Six young rabbits..	100	82·1	98·71	94·09
4 Rabbit	100	86·7	101·78	96·38
5 Indian pig.....	100	88·8	104·45	98·97
6 Ditto.....	100	88·9	104·53	99·00
7 Dog	100	80·8	98·16	93·80
8 Ditto.....	100	74·1	90·31	86·30
9 Young dog	100	74·5	91·30	87·40
10 Cat	100	80·6	96·81	92·25
11 Pigeons.....	100	78·8	93·10	88·26
12 Dog	100	79·2	94·52	89·80
13 Cock.....	100	79·7	94·43	89·50
14 Ditto.....	100	77·0	95·41	91·50
15	100	74·6	88·50	84·00
16	100	75·4	89·20	84·00

The foregoing table requires scarcely any explanation; it is admitted in the first column that the animal yields 100 degrees to the surrounding water; the second indicates the number which M. Despretz calculated according to the carbonic acid produced and the oxygen absorbed by the animal, taking as a basis 7815 units of heat to 1 part, by weight, of carbon in combustion, and 23640 for 1 part, by weight, of hydrogen; the third column the rectification of these numbers according to the last determination of the heat of combustion of hydrogen and the indirect determination of the heat of the combustion of carbon; the fourth contains the rectification of the numbers of the second column, according to the direct determination of the heat of combustion of carbon by Despretz.

The following are Dulong's experiments:—

DULONG.

	Heat yielded by the Animal.	Calculation of Dulong.	Corrected. Carbon=8558 Hydrogen=34792.
1 Cat.....	100	72·9	94·9
2 Ditto	100	68·8	90·1
3 Ditto	100	71·5	91·7
4 Ditto	100	75·8	68·6
5 Ditto	100	73·6	95·7
6 Dog	100	72·8	94·6
7 Ditto	100	80·2	104·7
8 Ditto	100	79·2	103·0
9 Hawk.....	100	71·5	97·2
10 Ditto	100	70·9	104·1
11 Indian pig	100	69·4	83·6
12 Ditto young	100	74·9	90·2
13 Ditto ditto	100	80·0	95·5
14 Rabbit	100	75·5	91·1
15 Ditto young	100	83·3	101·8
16 Pigeon.....	100	74·5	95·4
17 Ditto	100	78·7	95·9

These ciphers speak for themselves: they prove beyond doubt that the number of degrees of heat which an animal yields to the surrounding bodies is exactly similar to that which the same apparatus would receive if a quantity of oxygen corresponding to the carbonic acid found and to the water formed were converted in it into an equal quantity of carbonic acid and water, and the solution of the question relative to the source of the heat of the animal follows as a matter of course.

With respect to the nitrogen exhaled during the respiration of animals in the experiments of Dulong and Despretz, a simple calculation shows that the observations themselves cannot be accurate; that the augmentations found in the volume of air in which

the animals respired, should be imputed to error in measuring the carbonic acid gas formed and the oxygen gas absorbed.

In Dulong's experiments, a dog, weighing 1150 grammes, expired in 80 minutes 0·375 of a litre of nitrogen gas, which gave in 24 hours 6·75 litres or 7·46 grammes of nitrogen. Admitting that this nitrogen arose from the body of the dog, we know that 100 parts, by weight, of fresh flesh contain (75 per cent. of water) 4 per cent. of nitrogen, and if we admit that the whole dog was formed of flesh, and contained 4 per cent. of nitrogen (which is certainly too much, since the blood and bones contain less, and the fat none), there would remain of the dog at the end of seven days only the earthy substance of the bones and the salts of the body, precisely the same as if it had been burned with oxide of copper; all the rest—hair, skin, flesh, and blood—would be transformed into gas.

Two dogs (of unknown weight) in Despretz's experiments, expired in 102 minutes 1·097 litres of nitrogen gas; in 24 hours 15·4 grammes of the same body; the dog would lose in 24 hours a pound of flesh only. It is naturally admitted in both cases that these animals would not pass urea in the urine, or nitrogen in the excrements. These conclusions are absurd in themselves.

EXPERIMENTS ON THE SPHEROIDAL STATE OF BODIES, AND ITS APPLICATION TO STEAM BOILERS, AND ON THE FREEZING OF WATER IN RED-HOT VESSELS.*

BY PROFESSOR BOUTIGNY.

Professor Boutigny, who made his communication in the French language, first proceeded to show that a drop of water projected upon a red-hot plate does not touch it; but that a repulsive action is exerted between the plate and the fluid, which keeps the latter in a state of rapid vibration. At a white heat, this repulsion acts with the greatest energy, whilst it ceases, and the ordinary process of evaporation takes place at a brown-red heat. The temperature of the water whilst in the spheroïdal state is found to be only 96°, and this temperature is maintained so long as the heat of the plate is kept up. To bring this water to the boiling point (to 212°), it is therefore necessary to cool the plate. These phenomena are explained by M. Boutigny on the supposition that the sphere of water has a perfect reflecting surface, and consequently that the heat of the incandescant plate is reflected back upon it; and some experiments have been made which show that this is the case, the plate becoming visibly redder over those parts on which the vibrating globule played. Several experiments were made in proof of the necessary cooling to produce ebullition. The red-hot plate, with its spheroïdal drop, was removed from the spirit-lamp, and after a minute or two the water began to boil, and was rapidly dissipated in steam. Ammonia and ether were shown, although so exceedingly volatile, to act in the same manner; the ether, however being decomposed whilst in the vibratory condition, in the same manner as it is by the action of platina wire, forming a peculiar acid. Iodine put upon the heated plate became fluid, and revolved in the same manner as other fluids, no vapors escaping whilst the high temperature of the metal was maintained; but when allowed to cool to the point of dull redness, it was immediately dissipated in violet vapors. The nitrate of ammonia fused on the glowing hot plate, and vibrated with great energy; but on cooling the capsule, the salt entered into vivid combustion. The repulsive action was shown by plunging a lump of silver at a glowing red-heat into a glass of water. As long as its bright redness was maintained, there was no ebullition; but as it slowly cooled, boiling took place. In this experiment, it appeared as if the glowing metal formed around itself an atmosphere; and the contiguous surfaces of the water appeared like a silvered plate. The application of the principles involved in these phenomena to the tempering of metals was then explained. If a metal to be tempered is in a highly incandescant state, the necessary hardening will not take place on plunging it into water. It is, therefore, necessary that a certain temperature should be observed. Experiments were made to show that the repulsive power of the spheroïdal fluid existed, not merely between it and the hot plate, but between it and other fluids. Ether and water thus repelled each other, and water rested on and rolled over turpentine. The bursting of steam-boilers came next under consideration; and it was shown that many serious explosions may be referred to the phenomena under consideration. In a great many cases, the explosions have occurred during the cooling of the boilers after the withdrawal of the fire. An experiment was shown in proof of the opinion entertained by M. Boutigny. A sphere of copper, fitted with a safety-valve, was heated, and a little water being put into it, it was securely corked up, and withdrawn from the lamp. As long as the metal remained red, everything was quiet; but upon

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cooling, the cork was blown out with explosive violence.

The concluding experiment excited great interest. The production of ice in a vessel at a glowing red-heat was a result so anomalous, that every one was desirous of witnessing the phenomenon for himself. It was beautifully performed by M. Boutigny in the following manner:—A deep platina capsule was brought to a glowing red-heat, and at the same moment, liquid sulphureous acid, which had been preserved in the liquid state by a freezing mixture, and some water, were poured into the vessel. The rapid evaporation of the volatile sulphureous acid, which enters into ebullition at the freezing point, produced such an intense degree of cold, that a large lump of ice was immediately formed, and being thrown out of the red-hot vessel, handed round to the company in the Section.

Mr. G. Rennie referred to some experiments of his own, in confirmation of M. Boutigny's views.

ON A NEW PROPERTY OF GASES.*

BY PROFESSOR GRAHAM.

After explaining the law which regulated the diffusion of gases through porous bodies, and stating the fact, that the lighter gases diffused themselves much more speedily than the more dense ones—that their diffusion was equal to the square of their densities—he proceeded to relate his experiments on the passage of gases into a vacuum. To this passage the term effusion has been applied. The velocity of air being 1·, the velocity of oxygen was found to be ·9500 by experiment, and by calculation ·9487. Carbonic acid being much heavier than air, gave the number ·812. Carburetted hydrogen gave ·1322 as the velocity of its effusion. Hydrogen gave as the velocity of effusion 3·613 by experiment, which was very nearly the amount given by theory. The interference of friction, even of minute orifices, was then described, and shown to admit of easy correction. Some useful applications were mentioned; as in the manufacture of coal gas, where it is desirable to ascertain the quality as well as the quantity of gas manufactured. As the gas will pass the orifice on its way to a vacuum the quicker the lighter it is, and the more slowly as it increases in density, and as the superior carburetted hydrogen is heaviest, it would be easy to construct an instrument to register this velocity, and thus mark at once the required quality and quantity of gas. It was also proposed that an instrument might be used in mines to detect the presence of light carburetted hydrogen (fire damp). The passage of gases under pressure through porous bodies was termed by Professor Graham, transpiration. The mode adopted in experiment was, to take a glass receiver, open at the top, which was closed with a plate of stucco. This was placed on an air-pump, the air exhausted by the pump, and the velocity with which the air passed through the stucco being marked by the mercurial gauge of the pump. The transpiration of atmospheric air was found to be greater than that of oxygen. Carbonic acid is found to be more transpirable than oxygen, or even, under low pressure, than atmospheric air. The transpiration of hydrogen is one-third more rapid than that of oxygen. The applicability of this process of experimenting to the explanation of exosmose and endosmose action in the passage of fluids through porous bodies was pointed out.

Mr. Bain made some observations to the effect, that Professor Graham's researches went to explain many meteorological phenomena, but particularly the suspension in the atmosphere of masses of aqueous vapour, in the form of clouds.

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RECENT EXPERIMENTS ON OZONE.*

BY PROFESSOR SCHÖNBEIN

Professor Schönbein was first induced to undertake his researches from the obscurity which rested upon the phenomena of the odor produced during the galvanic decomposition of water, resembling the smell of an electrical machine, and during thunderstorms. In pursuing these researches, the author was led to the discovery that the smell was always developed at the positive pole; that it was capable of being preserved in closed bottles; that heat destroyed it, and that many of the metals had also the same power. Experiments were made with a view to discover some means of procuring ozone easily; and after the trial of a great many bodies, it was found that phosphorus was particularly suited for the purpose. If a piece of phosphorus is placed in a bottle of common air, when maintained in a moist state, it readily produces this peculiar principle, on which the electrical smell depends. Several experiments were shown, to illustrate the effects of ozone in bleaching litmus-paper and paper colored with indigo, or a solution of that substance. If powdered iron or silver are placed in vessels containing ozone, the smell is immedi-

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ately removed, and the metals exhibit a kind of oxidation. Other bodies were named, as producing a similar result; and many chemical decompositions were found to arise from exposure to the action of this peculiar principle. Solution of iodide of potassium is rapidly decomposed, iodine being set free; this was shown by a mixture of this salt with starch, which, on being exposed to the action of ozone, turned blue by the formation of the iodide of starch. Bromide of potassium was also decomposed by this principle, and bromine liberated. Salts of iron were shown to undergo the same changes. A number of organic bodies, vegetable as well as animal, destroy the ozonous smell. If ozonodised air be made to pass through a tube, and this tube gently heated, all the properties of ozone are destroyed. This was shown by experiment. By the inspiration of ozone, similar effects are produced on the lungs to those resulting from chlorine and bromine. A mouse was killed in five minutes, and the experimentalist himself was seriously affected by breathing an atmosphere charged with this odor. By electrolysis, gold or platina points are necessary for the development of ozone. The electrical brush in all cases produces the same effects as those above described; all the decompositions can be produced, and the same smell is distinctly evident. In this case, as in the other, heat and some of the metals also destroy the odor. Endeavors have been made to procure ozone in an isolated state, but they have not been successful. Ozone, although at first supposed to be an elementary body, was afterwards considered as a compound of oxygen and hydrogen. The fact of heat destroying this peculiar odor at once, shows that this principle is produced from the elimination of an oxygen compound from the decomposition of water. This is quite in accordance with the views entertained by Mariniac, who has pursued the investigation with great industry, and who has published a memoir, in which his views are luminously set forth. The author of this report is of opinion that ozone will turn out to be a compound isomeric with the binoxide of water. A theoretical view of the production of this body was then entered into. The ordinary action of phosphorus undergoing oxidation in the atmospheric air was explained, and the remarkable fact stated, that although phosphorus was luminous in moist air, it was not so in perfectly dry air. Ozone may now, therefore, although long regarded by Professor Schönbein as an elementary body, be looked upon as, in all probability, a tritoxide of hydrogen. Its bleaching properties are very remarkable, and it may possibly be of considerable practical utility.

ON THE ACTION OF CHLORINE ON CYANIDE OF MERCURY UNDER THE INFLUENCE OF THE SOLAR RAYS.*

BY M. JULES BOUIS.

M. GAY-LUSSAC, in his remarkable work on hydrocyanic acid observed, that by placing cyanide of mercury in flasks of chlorine exposed to the sun, an oily, yellow liquid is produced: but he confined himself to noticing the fact, the object of his work being different.

Serullas afterwards studied this body, and he regarded it as an association of chloride of nitrogen, of a liquid chloride of cyanogen and of perchloride of carbon held in solution.

Serullas did not analyse this body; he intended to return to a subject so worthy of attention, which he could study only at intervals on account of the danger of the fulminations and painful watering of the eyes, produced by the volatilisation of these bodies. There is no doubt that if time had permitted him to devote himself again to this question, his skill and sagacity would have solved it in a complete manner.

I now make known to the Academy the results furnished me by a new examination of this substance.

When we expose to the solar rays flasks of chlorine, with a saturated and boiling solution of cyanide of mercury, an oily yellow liquid is obtained, heavier than water, and insoluble in that liquid, soluble in alcohol and ether; this liquid, which is of an excessively strong irritating odor, affects the eyes to a considerable degree; its taste is very caustic; it burns with a red flame. Humid or dry, it deposits after a long time the crystals of sesquichloride of carbon, of Faraday, and is partially decolored.

Like nitrate of methyline, this body, although explosive, may be burned with oxide of copper at a red-heat; it is, however, very difficult to make the combustion proceed in a regular manner.

The composition of this body is represented, according to my analysis, by

$$C^8 N^4 Cl^8, C^4 Cl^6 (\dagger)$$

It is easy to explain the formation of the first term, $C^8N^4Cl^8$, since we have the presence of the elements of cyanogen with an excess of chlorine. We may even suppose the formation of a second molecule of the same nature; now, the latter, under the influence of water, is decomposed and gives carbonic acid and nitrogen, which are disengaged, sal-

* *Comptes Rendus*, July 21, 1845.

† C = 6, N = 14, Cl = 35·5.

ammoniac, which remains in solution, and finally sesquichloride of carbon, as indicated by the equation—

$$C^8 N^4 Cl^8 + H^8 O^8 = C^4 O^8 + N^2 Cl^2 H^8 + N^2 + C^4 Cl^6.$$

The sesquichloride of carbon, in the nascent state, unites with the product $C^8 N^4 Cl^8$, giving rise to a compound more stable: $C^8 N^4 Cl^8 + C^4 Cl^6$.

If the action of chlorine be continued, this product remains, and a disengagement of nitrogen is observed at the same time as hydrochloric acid is formed.

The production of these two gases is due to the decomposition of the sal-ammoniac. My experiments and my analyses, which I cannot detail here, accord with this view.

The yellow liquid, decomposed by heat, deposits sesquichloride of carbon, disengages nitrogen, and is presented under the form of a transparent, colorless liquid, of the composition

$$C^8 N^4 Cl^4 + 2 (C^4 Cl^6) + C^4 Cl,$$

and derived from the first product; indeed,

$$2 (C^8 N^4 Cl^8) = C^8 N^4 Cl^4 + 2 (C^4 Cl^6) + N^4.$$

Finally, nitric acid acts on the yellow liquid as an oxidising agent, and gives a more irritating, more caustic product than the foregoing, represented by

$$\left.\begin{matrix} C^8 N^4 Cl^8 \\ O^4 \end{matrix}\right\} + C^4 Cl^6.$$

Ammonia also decomposed the yellow liquid into different products, among which Faraday's sesquichloride of carbon is always remarked.

From the foregoing facts, the existence of three chlorides of cyanogen may be admitted —protochloride, bichloride, and terchloride, acting the part of acids and combined with the sesquichloride of carbon. In the terchloride of cyanogen, 4 equivalents of chlorine are replaced by 4 equivalents of oxygen. These products form the series—

$$C^8 N^4 Cl^4 + C^4 Cl^6 + 2C^4 Cl^6.$$
$$C^8 N^4 Cl^8 + C^4 Cl^6.$$
$$\left.\begin{matrix} C^8 N^4 Cl^8 \\ O^4 \end{matrix}\right\} + C^4 Cl^6.$$

The sesquichloride of carbon, $C^4 Cl^6$, being derived from ordinary ether, $C^4 H^5 O$, these combinations might be regarded as chloruretted ethers formed by the union of the chlorides of cyanogen with the sesquichloride of carbon.

I own that I should not have undertaken this work, on account of the difficulties which its study presents, if the kind advice of my master, M. Dumas, had not emboldened me to enter upon these researches.

ANALYSES OF THREE SPECIES OF FUCUS.*

BY E. G. SCHWEITZER.

These analyses were undertaken with a view to ascertain the causes which render one kind of fuci beneficial as a manure, whilst other kinds do not succeed. The author found that the alkaline carbonates predominated in the *Laminaria saccharina*, whilst there was an excess of sulphate in the *Fucus vesiculosus* and *Fucus serratus*; and in the laminaria potassa is found in larger quantities than soda, whilst the contrary is the case with the fuci. The quantity of iodine in laminaria was also greater than in the other two. Some experiments were tried, to ascertain whether the laminaria exhaled iodine under the influence of the solar rays, but no such exhalation could be detected. The analyses were exceedingly minute, giving twelve or thirteen constituents determined.

Prof. Johnston made some remarks on the imperfections of our knowledge of the chemical principles of rotation, the difficulty having been the length of time required for a full investigation of the subject. He objected to the opinion expressed by some chemists, that any crop may be grown on the same soil for any length of time, by the application of manures, the quality of which were to be decided by analysis. In addition to chemical considerations, the physical conditions of the soil are to be taken into account. Several points were shown to require strict attention, particularly the structure of the plant itself, and the character of its root, which he had observed to vary with the conditions of the soil.

ON THE VOLTAIC REDUCTION OF ALLOYS.*

BY C. V. WALKER.

This communication was intended to explain the methods by which the author has succeeded in throwing down metallic alloys from compound solutions by the action of galvanic electricity. The process adopted, is to prepare a strong solution of cyanide of potassium, and commence electrolising it, by means of a copper anode; as soon as copper begins to be dissolved, the copper anode is removed, and its place supplied with one of zinc; after the action has continued for some little time, brass will be liberated on the cathode. The solution is now ready for

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† *Idem.*

use, and is operated upon by two or three Daniell's cells, and with a brass anode. By similar means alloys of gold and copper, or gold and silver, may be deposited. Mr. Walker reasons, that true brass is a definite chemical compound; and states—It appears possible that the anode, which is a brass of commerce, is a true alloy, plus an excess of zinc; that the solution it produces is a mixed solution, which consists of the potassio-cyanide of brass and the potassio-cyanide of zinc. This solution is very readily decomposable; it is therefore necessary to prepare it a short time previously to its use. Many specimens were exhibited of copper and other metals coated with brass. The author makes some remarks on the theory of the action; and concludes by stating that it will be quite possible to determine, within certain limits, the character of the alloy that shall present itself, and that we may be enabled to throw down gold and silver according to standard.

MEMOIR ON THE DECOMPOSITION OF WATER BY THE METALS, IN PRESENCE OF ACIDS AND SALTS.*

BY M. E. MILLON.

The alteration of metals, either by pure water, or by water charged with acid or saline principles, is a phenomenon frequently presented in the laboratory: this phenomenon affects also our domestic utensils, and among industrial facts, it certainly occupies an important place.

The experiments which I am about to describe relate to this alteration of metals by water, in the presence of acids and salts; they comprise, in fine, only a simple indication of some circumstances under the action of which the metals in most common use—iron, zinc, tin, and copper—are attacked and sometimes preserved. But, among all these circumstances is one which I will immediately detach, because I believe its intervention to have passed hitherto almost unperceived. I refer to the influence of small quantities.

This influence, if I am not deceived by my first impression concerning it, is considerable.

If we were to say, indeed, that a small quantity of metallic solution, added in the proportion of a thousandth, a hundred-thousandth, and often in smaller proportion, is sufficient for centupling the action of an acid on a metal, or for annihilating that action, or for provoking it when it does not exist, or, indeed, for changing the nature of the products to such a point that hydrogen replaces nitrous compounds, we should appear to be as far from the facts as from chemical provisions.

Such, however, is the case. The detail of the experiments will show how easy it is to reproduce all these phenomena.

On Zinc and on the Decomposition which it exerts with the aid of small quantities of Metallic solutions, on Acid or Saline solutions and on pure Water itself.

Zinc and sulphuric acid.

Many chemists have already noticed the peculiar resistance which zinc sometimes offers to the solvent action of dilute nitric acid. We are generally disposed to attribute this resistance to the purity of the metal, which would decompose water with facility only with the aid of another metal combined with the zinc. The phenomenon has been viewed here in a different manner: the zinc has been taken as found in commerce, whilst the acid, saline water, and the pure water itself have received, before being put in contact with the zinc, a small quantity of a foreign saline solution.

This laminated zinc was put in contact with dilute sulphuric acid, to which had been added a few traces of either of bichloride of platinum, emetic, arsenious acid, sulphate of copper, or sulphate of silver, &c. The metal, weighed before the reaction, was weighed again when the reaction had been carried on for a given time. In other circumstances, the gas was collected and measured.

I will indicate the principal conditions of the experiments made with zinc and dilute sulphuric acid.

The zinc was laminated in very thin leaves which presented a surface of 100 to 102 cut square centimetres of the weight of 15 grammes.

The sulphuric acid was composed of one part by weight of monohydrated sulphuric acid SO^3,HO, to 12 parts of distilled water.

The zinc was weighed accurately, and six nearly equal quantities were introduced into six glass flasks, each of which contained 1¼ decilitre of sulphuric acid diluted as above; the flasks were distinguished by the Nos. 1, 2, 3, 4, 5, and 6.

In flask No. 1, the acid was left pure; No. 2 received four drops of a solution of bichloride of platinum made with one part of bichloride and ten parts of water; Nos 3, 4, 5, and 6 received a few drops of saturated solutions of emetic, arsenious acid, sulphate of copper, and sulphate of silver.

The duration of the experiment was ten minutes for each flask.

* *Comptes Rendus*, July 7, 1845.

No. I. Dilute sulphuric acid without addition.	Zinc, before the experiment, 10 gr. 119; loss, 0 gr. 028.
No. II. Sulphuric acid with bichloride of platinum.	Zinc, before the experiment, 100 gr. 466; loss, 4 gr. 298.
No. III. Sulphuric acid with 10 drops of emetic.	Zinc, before the experiment, 10 gr. 600; loss, 0 gr. 800.
No. IV. Sulphuric acid with 10 drops of sulphate of copper.	Zinc, before the experiment, 9 gr. 808; loss, 1 gr. 234.
No. V. Sulphuric acid with 15 drops of arsenious acid.	Zinc, before the experiment, 10 gr. 857; loss, 3 gr. 706.
No. VI. Sulphuric acid with 15 drops of sulphate of silver.	Zinc, before the experiment, 10 gr. 349; loss, 0 gr. 071.

It was impossible to employ, in each experiment, an exactly similar quantity of zinc; but by reducing, by calculation, the numbers representating the weight and loss of the metal to the same expression, and by taking the action of the pure acid on the metal as unity, we arrive at the following table:—

Action of pure sulphuric acid	1
Ditto ditto, with the addition of bichloride of platinum.....	149
Ditto ditto, with the addition of arsenious acid	123
Ditto ditto, with the addition of sulphate of copper	45
Ditto ditto, with the addition of caustic	29
Ditto ditto, with the addition of sulphate of silver.....	24

It is remarked, in the addition of the above saline solutions, that the influence of bichloride of platinum is immediate and decreases as it proceeds; the influence of the arsenious acid, on the contrary, is established rather slowly, but it is rapidly developed in such a manner as very soon to equal that of the bichloride of platinum. Sulphate of copper, emetic, and sulphate of silver also produce an increasing acceleration.

I have measured this acceleration as regards the sulphate of silver, in two comparative experiments which lasted an hour, instead of ten minutes; I obtained—

First experiment.—Zinc and sulphuric acid, diluted as above, without addition: zinc, before the experiment, 10 gr. 091; loss, 1 gr. 187.

Second experiment.—Zinc and sulphuric acid, with 15 drops of sulphate of silver: zinc, before the experiment, 10 gr. 278; loss, 5 gr. 527.

Thus the acceleration produced by sulphate of silver should be expressed by 2·4 for a duration of ten minutes, and by 4·6 for an hour.

The saline solutions of cobalt, nickel, tin, cadmium, chromium, lead, antimony, and bismuth, also accelerate the disengagement of hydrogen; but their influence has not been measured as in the case of the foregoing solutions which seem to present the most striking examples of acceleration, slow or rapid.

When a few drops of bichloride of mercury are added to dilute sulphuric acid, the zinc is covered with a layer of amalgam, and is thus preserved in a very remarkable manner. This is a well-known fact, which has been usefully applied to the construction of certain electric piles.

In an experiment by which I endeavored to estimate this preservation of zinc by mercury, 20 grammes 978 of metallic zinc were left for seventy hours in contact with a quantity of sulphuric acid dilute ten times sufficient for dissolving all the metal in about an hour and a half. The loss of laminated zinc, at the end of the seventy hours, was only 0 grammes 343.

It is evident that a small quantity of mercury acts in an opposite way to a small quantity of platinum or arsenic. These oppositions will be multiplied and will become more striking, when the same saline solution, added in equally minute proportion, is seen to act as a preservative of iron even after leaving centupled the solution of zinc.

It was interesting to ascertain whether, by varying the proportion of the small quantities, their influence would be modified. The following are the results of two comparative experiments:—

First experiment.—Sulphuric acid diluted 12 times, with the addition of 1 drop of bichloride of platinum: weight of zinc before the experiment, 10 gr. 377; loss, 1 gr. 848.

Second experiment.—Sulphuric acid diluted as above; 10 drops of bichloride of platinum added: weight of zinc before the experiment, 10 gr. 257; loss, 3 gr. 871.

The duration of each experiment was about five minutes.

The quantity of bichloride of platinum, in becoming ten times greater, produced an action about twice as energetic. The proportion of the small quantities is not, therefore,

indifferent; it would doubtless become so beyond certain limits, or else it might be subject to other influences.

With respect to the persistance of the influence exerted by the small quantities, a notion may be formed from the following experiments:—

1st. Three quantities of zinc, sensibly equal in weight, were separately put into sulphuric aid diluted 12 times; then they were removed, the first after 5, the second after 10, and the third after 15 minutes.

2nd. Three quantities of zinc, equal in weight to the foregoing, were put into sulphuric acid diluted as above, and containing four drops of the bichloride of platinum; they were removed from the acid at the same intervals as those of the first experiment.

FIRST SERIES.

I. Zinc, before the experiment, 15 gr. 104; loss in 5 minutes, 0 gr. 025.
II. Zinc, before the experiment, 15 gr. 041; loss in 10 minutes, 0 gr. 058.
III. Zinc, before the experiment, 15 gr. 215; loss in 15 minutes, 0 gr. 120.

SECOND SERIES.

I. Zinc, before the experiment, 15 gr. 352; loss in 5 minutes, 3 gr. 553.
II. Zinc, before the experiment, 15 gr. 326; loss in 10 minutes, 6 gr. 325.
III. Zinc, before the experiment, 15 gr. 237; loss in 15 minutes, 7 gr. 206.

The action of dilute sulphuric acid on zinc increases very rapidly; it is not the same when the sulphuric acid contains a few traces of bichloride of platinum. Thus we are far from finding, after an addition of bichloride of platinum, very considerable differences when the commencing action is no longer under examination. If we compare, for example, the time necessary for a complete solution, it will be found that bichloride of platinum only renders the solution five or six times more rapid.

The concentration of the acid is also a point to be considered. Sulphuric acid diluted 10 times presented less characteristic effects; a greater concentration effaced them more and more.

I do not enter here into the causes which determine these variations.

ZINC IN CONTACT WITH SOME ACIDS.

The mineral and organic acids are subject, like sulphuric acid, to the influence of small quantities of metallic solutions; but this influence varies with each acid. These differences may be appreciated by the numbers which I will furnish on the subject of hydrochloric acid. I will confine myself, with respect to the other acids, to general indications.

Hydrochloric acid should be very dilute in order that the addition of the small quantities may have a powerful influence on the results of its action.

To 1 volume of hydrochloric acid not less than 40 volumes of water are required.

The experiment was arranged as with the dilute sulphuric acid; the contact of the acid and zinc lasted an hour.

First Experiment.—Dilute hydrochloric acid, without addition: zinc, before the experiment, 5 gr. 640; loss, 0 gr. 59.

Second Experiment.—Dilute hydrochloric acid with 4 drops of bichloride of platinum: zinc, before the experiment, 5 gr. 691; loss, 2 gr. 586.

Third Experiment.—Dilute hydrochloric acid, with 20 drops of arsenious acid: zinc, before the experiment, 5 gr. 664; loss, 2 gr. 258.

Fourth Experiment.—Dilute hydrochloric acid, with 15 drops of solution of emetic: zinc, before the experiment, 5 gr. 656; loss, 2 gr. 054.

Expressing the action of the pure acid by.. 1
That of the same acid containing bichloride of platinum should be expressed by 43
With the addition of arsenious acid, by.. 38
And with the addition of emetic, by 35

Thus, bichloride of platinum is far from producing so great an acceleration as with sulphuric acid and emetic, which, with the latter acid, acted with only one-fifth of the power of bichloride of platinum, possesses an almost equal influence in presence of hydrochloric acid.

A weak or concentrated solution of pure oxalic acid does not convert zinc into oxalate, even by the aid of boiling. With the intervention of the slightest trace of bichloride of platinum, all the metal is converted, even without heat, although the oxalate of zinc is insoluble. Bichloride of platinum alone acts in an evident manner in this case; sulphate of copper, arsenious acid, &c., appear to have no influence.

Radical acetic acid, diluted with one volume of water, feebly attacks metallic zinc; but a single drop of bichloride of platinum communicates to the reaction a most remarkable intensity. In comparing the quantity of zinc dissolved, I found two hundred times more on the part of the bichloride of platinum. Bichloride of mercury does not exert, on the contrary, any preservative influence with this weak acid; arsenious acid develops its action only after a very long time. Sulphate of copper and emetic accelerate, but much less than the salt of platinum.

Dilute butyric acid acts exactly like acetic acid.

Citric acid furnishes a very good example of comparative acceleration. It must be dissolved in five or six times its weight of water; its action is then very feeble; but bichloride of platinum, sulphate of copper, and arsenious acid determine a very abundant disengagement of hydrogen; the sulphate of silver possesses no applicable influence; bichloride of mercury preserves the metal very well.

Tartaric acid, dissolved in seven or eight times its weight of water, presents phenomena analogous to those of citric acid. The action of sulphate of silver is an exception, since it considerably accelerates the conversion of zinc into tartrate.

Racemic acid also belongs to the category of the two foregoing acids, and closely resembles tartaric acid. Sulphate of silver also accelerates the formation of the racemate; but arsenious acid acts but very slowly.

ZINC IN CONTACT WITH DISTILLED WATER AND SALINE SOLUTIONS.

The acceleration of the action of certain acids on zinc, by means of a few drops of a metallic solution, also takes place when those acids are replaced by various saline solutions.

The chlorides of potassium and sodium, the sulphates of soda, potassa, and magnesia, in solution in water, and the alkaline leys of soda, potassa, and ammonia, are among these. It is sufficient to add to these solutions a few drops of bichloride of platinum for the granulated zinc to determine, even without the aid of heat, a sensible disengagement of hydrogen. At the temperature of boiling water this gas is produced in abundance.

Sea water, river water, and distilled water itself, are decomposed by zinc, under the influence of small quantities of metallic solutions. Distilled water furnishes less hydrogen than that which contains saline principles. This disengagement is instantaneous, but it continues for a long time. A flask, of the capacity of eight ounces, containing granulated zinc, was filled with distilled water, to which six drops of bichloride of platinum had been added: hydrogen was immediately produced; 300 cubic centimetres were produced in 24 hours. On the second and third days, the disengagement was nearly the same; at the end of eight days, only, about 200 cubic centimetres were formed in the same space of time; at the end of twenty days, the production of gas still continued. It must be added, that bichloride of platinum and sulphate of copper are the only salts which, added to distilled water, provoke its decomposition by zinc. Arsenious acid, emetic, and sulphate of silver, do not produce a single bubble of hydrogen. Bichloride of platinum acts more energetically than sulphate of copper; and, of all saline solutions, that which contains sulphate of soda gives the most abundant disengagement of hydrogen.

A saturated solution of common salt, to which bichloride of platinum is added, furnished 127 cubic centimetres of hydrogen, whilst a saturated solution of sulphate of soda gave, in analogous conditions, 224 cubic centimetres.*

When the zinc in the acid or saline solutions, or even in pure water, meets with the small quantity of metallic salt added to it, it may easily be perceived that the metal of this small quantity is precipitated on the surface of the zinc. Thus, the latter metal is covered with a black layer, in the case of bichloride of platinum, and sulphate of copper deposits on its surface a layer of red copper. Perhaps it is not always a simple metallic solution which is produced; but this is the most ordinary case. As there is then formed a small quantity of chloride or some other salt of zinc, corresponding to the small quantity of bichloride of platinum or other salt employed, it may be asked if the salt thus formed participated in the foregoing reactions. If this influence be felt, it is at any rate a very feeble one. Indeed, after the precipitation of the platinum or other metal, the pieces of zinc may be very carefully washed, and the primitive saline

* Light certainly aids in the progress of the phenomena which I have just pointed out. It is especially on slow actions, with saline solutions, that this influence is evinced. By avoiding the light, the disengagement of hydrogen is abated in a general manner. But, in certain cases, this abatement of action presents a very peculiar character. Thus, let two flasks of the same size be taken, one protected from the light by envelopes of paper and tin-foil, while the other receives the direct light of the sun; introduce into each of them an equal quantity of laminated zinc and saturated solution of common salt, with the addition of bichloride of platinum; a most rapid disengagement will at first be observed in the protected flask; but, suddenly the hydrogen ceases to be disengaged in this flask, and, after a few hours, the envelopes are removed without avail; hydrogen is no longer formed. In the other flask, the disengagement, which is much slower at the commencement, continues without interruption, and may last several months. It is needless to add that, from the commencement of the experiment, the two flasks were steeped in the same capsule filled with water.

or acid solution, afterwards replaced by a similar solution in which the salt of zinc resulting from the precipitation can no longer be found. This disengagement of hydrogen continues as before the change of solution.

ON THE ACTION OF GASES ON THE PRISMATIC SPECTRUM.*

BY DR. MILLER.

Referring, in the first instance, to the experiments of Sir D. Brewster on the changes produced on the fixed lines of the prismatic spectrum by various absorptive media, Dr. Miller proceeded to explain his method of examining the subject. The light, being passed through a longitudinal slit in a plate of metal, was received on a prism of Munic glass; the spectrum thus formed was passed through the gaseous medium under examination, and the resulting effect observed by a telescope. It was found that the dark lines of the spectrum materially changed their positions as different colorless gases were used; and that, by subjecting the spectrum to the absorptive influences of chlorine, nitrous acid vapor, the vapors of iodine, bromine, &c., numerous dark, and some luminous bands, not previously observed, were brought into view. The spectra produced by colored flames were also examined, and many curious conditions observed. Dr. Miller had sought to ascertain if any relation could be found between the chemical characters of the bodies under examination and their properties of exhibiting Fraunhofer's lines; but as yet no such relation could be detected.

ON THE PRESENCE OF OZONE IN THE ATMOSPHERE.†

BY PROFESSOR SCHÖNBEIN.

The action of heated platina upon the vapor of ether was shown, and the production of that principle, to which the name ozone has been given, was manifested by the bleaching of litmus-paper, and the decomposition of iodide of potassium. The production of the same principle during all the manifestations of phosphorescence was brought forward as a proof of the existence of ozone in the atmosphere. Ozone was found to be of a deleterious nature, but it was removed most readily from the atmosphere by the organic matter, to the action of which it was constantly exposed. Professor Schönbein then went into extensive speculations on the phosphorescence of the sea, which he regarded as a phenomenon in all respects resembling the slow combustion of phosphorus; and this, with the luminous phenomena exhibited by insects, by rotten wood, and by putrefying matter, he attributed to the oxidation induced by the influence of ozone, which was now most distinctly proved to be a peroxide of hydrogen.

Professor Faraday begged to correct an error into which he had been led some years since, when he stated this peculiar odor to be due to the formation of nitric acid in the air. Although he had formed nitrate of potash by the action of electrolised air on caustic potash, and consequently shown that nitric acid was formed, he believed the odor to depend upon some other cause. He could not go with Professor Schönbein in all his speculations, but he thought the inquiry of importance. Professor Schönbein stated, that during a recent snow-storm at Basle, iodide of potassium was decomposed by the action of the atmosphere in about six minutes.

OBSERVATIONS ON SOME NEW PHOSPHURETTED PRODUCTS.*

BY M. PAUL THÉNARD.

The analogy which exists between phosphorus and arsenic made me think that it would probably be possible to obtain with phosphorus a compound analogous to that which has been obtained with arsenic, and which is known by the name of cacodyle.

Guided by this idea, I was led to pass hydrochlorate of methyline through a great excess of phosphuret of lime at temperatures ranging from 350° to 570° F.; by continuing the experiment for a very long time, I have observed that at least five products may be formed, three liquid and two solid.

Among the three liquids, there is one which I have examined with much more care than the others, and which presents sufficiently remarkable properties to warrant my asking the Academy to be kind enough to allow me to submit it to its judgment.

This liquid, which I regard as formed of phosphuretted hydrogen and carbon, in proportions which I shall endeavour to point out hereafter, is a true alkali which promptly restores the blue color of reddened litmus-paper, and combines with the acids forming neutral salts.

The new alkali is colorless; it has a warm and bitter taste; its odor somewhat resembles that of ammonia; its tension at the or-

* British Association.

† *Idem.*

* *Comptes Rendus*, July 14, 1845.

dinary temperature and pressure is between 430 and 440 millimetres, and its point of ebullition between 104° and 106° F.

Experiment gave 2·61 as the density of its vapor; but this number is too high: I attribute the cause to an absorption of oxygen, which all the precautions I employed would not prevent.

Exposed to the action of heat, the alkali distils without being decomposed: submitted to the action of a feeble current of atmospheric air, it absorbs its oxygen, producing a perceptible light, and gives rise to a peculiar acid. This acid unites suddenly with a part of the undecomposed alkali, forming a salt which readily crystallises in beautiful transparent needles, and in which the acid is finally in excess. When the experiment is made by introducing small quantities of liquid into a flash, which is cooled, success is still more easy.

It is necessary to take care to pour the liquid into pure oxygen gas; it would take fire suddenly, and the explosion would be terrible.

Agitated with water, it is not perceptibly dissolved, and collects on the surface like a very light kind of oil.

Its reaction on oxide of mercury presents a phenomenon worthy of attention; it takes place when cold: the oxide of mercury is reduced, the temperature considerably raised, and at the same time sublimes beautiful white needles, which seem to be the pure acid, into which the alkali is converted by its oxygenation.

I combined the alkali with hydrochloric sulphuric, nitric, phosphoric, tartaric, oxalic, and acetic acids, and I have seen that with all it forms neutral salts, and even with some acid salts.

The hydrochlorate precipitates chlorides of platinum in a yellow crystalline powder.

It gradually forms, with the solution of copper, abundant beautiful white needles, reducing the copper from the maximum of oxidation to the minimum. It reduces the salts of gold; its capacity of saturation was determined with sufficient accuracy; it is the same as that of ammonia, for one volume of alkaline vapor requires, in order to be neutralised, one volume of hydrochloric acid.

At the ordinary temperature, all the salts of the new alkali are decomposed by soda, potassa, and lime, and the acid is liberated. I tried to analyse it by burning it with oxide of copper in the ordinary manner. I found per cent., in a first experiment, 46·1 of carbon and 11·6 of hydrogen, and in a second 46·08 and below 11·7, without appearance of any other gas. The combustion was complete; the number appeared to me something like the thing and brought me very near the truth.

But in the determination of phosphorus, I have met with difficulties which I have not yet been able to overcome entirely. One of the processes which promises most success, consists in passing the matter in vapor through a long column of an intimate mixture of oxide of mercury, carbonate of soda, and fragments of porcelain at a red heat; all the phosphorus burns and forms with the soda a phosphate which is estimated by the ordinary processes.

Although this latter experiment leaves much to be desired, I have strong reasons for supposing that the new alkali does not contain oxygen, and that it is formed in equivalents of C^6H^9P, divided into C^6H^6 and PH^3.

The other two liquids whose existence I think I have recognised, also possesses very distinct properties; both, like the first, contain phosphorus, carbon, and hydrogen.

One of them is slightly yellowish and viscid, inodorous, very dense, difficultly volatile, without action in the air at the ordinary temperature, insoluble in water and most of the acids, except concentrated hydrochloric acid, which easily dissolves it.

Finally, the third liquid, colorless, like the first, endowed with a powerful refraction, has to a certain extent the odor of cacodyle, and inflames vividly on coming into contact with the air: therefore, it must be handled only with great caution. I am induced to believe that this body is analogous, in composition, to cacodyle, properly so called. At least, it appears demonstrated to me, that under the influence of dilute sulphuric acid, it is converted, on one hand, into the sulphate of the alkali whose properties I have described, and, on the other hand, into the second liquid spoken of.

Now, if it were true that the alkali was represented in its composition by $PH^3\ C^6\ H^6$, it would be possible that the other two liquids were represented, one by $C^4\ H^4\ PH^2$, the other by $C^2\ H^2\ P^2\ H$; that is to say, that the three compounds of phosphorus and hydrogen, P^2H, PH^2, PH^3, would be combined—the first with $C^2\ H^2$, the second with $C^4\ H^4$, the third with $C^6\ H^6$. Very simple formula, which, I ought to mention, were suggested to me by the formula $C^4\ H^4\ NH^2$, which M. Dumas made known with respect to the arsenical cacodyle.

But, to be admitted, these ideas, which I present only with the greatest reserve, require to be confirmed by positive analyses, which I have not yet been able to make.

With respect to the two solids which may be formed in the action which hydro-

chlorate of methyline exerts on phosphuret of lime, one is perfectly known to me; that is, the hydrochlorate of the new alkali; by treating it in solution in water over lime, it is easy to obtain the alkali pure.

I know nothing positive concerning the second solid, except that it is very easily formed under the influence of an excess of lime in phosphuret of lime, and that it crystallises in long and beautiful white needles.

Such are the principal observations that I have hitherto made, and for the imperfection of which I solicit the indulgence of the Academy. The difficulty of the subject, and the danger of the experiments, will, I hope, serve as my excuses; I intend to continue my investigation, to complete them, and to rectify the inaccuracies which may have escaped me.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ON THE ACTINOGRAPH.*

BY MR. ROBERT HUNT.

After referring to Daguerre's statement that, the solar light, two or three hours before noon, differed in chemical character from that which proceeded from the sun at equal times after it had crossed the meridian, the author went on to state, from his own researches, that he was satisfied that the amount of chemical power was not in direct ratio with the quantity of light, but that at different hours of the day, and at different seasons of the year, a remarkable variation may be found to exist. The peculiar conditions of plants in the morning and evening and the phenomena of hybernation were referred to variations in this chemical (actinic) power, and the processes of germination, of flowering, fruiting, and the autumnal decay of the leaf, shown to be, in all probability, dependent upon the same influence. This being the case, it became desirable that some *self-registering* means of marking the changes in the condition of the sun's rays should be adopted. The actinograph is intended for this purpose. It consists of a cylinder of brass, upon which is placed a sheet of photographic paper, so prepared with the bromide of silver that all the rays of the prismatic spectrum shall act upon it with equal intensity; over this is placed another cylinder, which is carried round by a clock movement once in the twenty-four hours. In this outer cylinder is a triangular opening, divided by bars into a hundred parts, the smallest part of the slit being one hundred times less than the largest. As this cylinder moves over the paper it is of course exposed for different periods of time to the solar influence over different parts of the divisions; one portion being only exposed for one minute, whereas the largest opening admits of an exposure for one hundred minutes. Thus the greatest intensity of actinic power will produce, during the time of least exposure, the same effect as is produced by the weakest radiations during the period of prolonged exposure. The papers are removed every night and divided into twenty-four parts, and we have hence an exact measurer of the amount of chemical power exerted during every hour of daylight; and as the results may be numerically registered, and the uncertain effects of fixing thus removed, we procure with attention an accurate record for any period of time. This instrument is a modification of one devised by Sir John Herschel, with some improvements suggested by Mr. Jordan. Mr. Hunt stated, that circumstances had prevented his doing more than make a few trials of the apparatus, but that he hoped after the meeting to fix it, and use the actinograph for constant registration.

* British Association. From the *Athenæum*.

STALL-FEEDING.*

BY G. DOBITO.

It is the abuse of stall-feeding that has got into disrepute with some people, and the not treading down straw enough with others. This last I hold to be an advantage instead of a disadvantage; for, depend upon it that it is not the size of the dunghill, but the quality of the manure, that causes the farmer's stack-yard to be well filled. If managed well, I contend that there is no plan so good as stall-feeding. The fatteniug-house may be of any size or shape, but it is necessary that there should be underground drains, with gratings, to carry off the urine into the liquid-manure tank; shutters behind the bullocks to regulate the heat, and a wide passage at their

* *English Agricultural Society's Journal.*

heads to feed them and clean their mangers. The advantages I conceive to be the quantity of litter required being smaller, therefore the muck being made better; the temperature being more easily regulated, and every bullock being allowed to eat his share in peace. The disadvantage of the animal not being able to rub himself so well I consider fully done away with by the rough brush, which, you will observe, I recommend using; and although theorists may fancy the health of the animal likely to suffer, I have never found it so in practice.

ON THE VENTILATION OF COAL-MINES.*

BY PROFESSOR ANSTED.

PROFESSOR ANSTED gave an outline of the methods of working coal-mines, and then proceeded to narrate the particulars of certain accidents, the circumstances of which seemed capable of suggesting improvements in future; and he referred more particularly to the fact, that, in the Haswell explosion, a considerable number of the persons employed in other panels might have been saved if the communication between them had been more effectually shut off, and if, also, there had been a separate passage or air-drift to the upcast shaft. He also described the explosion in the Killingworth colliery, where, if the Davy lamp had been in use, the accident would, in all probability, not have happened; and Professor Ansted urged the necessity of certain conditions being observed in all collieries. 1. That there should never be less than two shafts, because an explosion can scarcely happen without destroying the partition at the bottom of the shaft, and thus checking or entirely stopping the ventilation. 2. That the *panels* should always be of moderate size, and the air-courses never exceed a certain length. 3. That separate air-drifts should always be made from distant workings direct to the pit bottom. 4. The exclusive use of the safety-lamp; in those mines, at least, where the escape of gas from fresh workings, faults, &c., rendered the working at all dangerous.

Professor Faraday said, the subject of mining accidents had long occupied his attention. The more he pursued the inquiry, the more he was disheartened at the apparent hopelessness of finding out any good general remedy. The explosions were not simply the effects arising from the mixture of gases, but from the combustion of the coal-dust and coal-gas which the first explosion made. In the fatal case at Haswell, the place where the accident originated had been ascertained; and the progress of the fire could be traced on the scorched beams and props of the galleries, and the deposits of coke made from the coal-dust which the explosion raised. To this circumstance the great force of the explosion was due, and not to the first escape of gas. A similar explosion had been known to take place in a cotton-wadding manufactory, the whole atmosphere of the place being fired by means of the particles of cotton in it. Of all the workmen killed in the Haswell accident, perhaps not one was really burned to death, but suffocated by the *choke-damp*. In one part of the workings the explosions had produced sharp vibrations, like the firing of gunpowder, and in another the burning went on slowly, like a common fire. But although two panels were blown into one, and solid stoppings of brickwork thrown down, there was no indication of the accident in the shaft. If the stoppings had not been blown down, and the supply of air had continued, the mine would have taken fire, and the men been burnt instead of choked. Since the late investigation, Mr. Faraday and Mr. Lyell have had many hundred plans submitted to them, urging ill-considered and contradictory measures. They had examined every part of the Haswell colliery, accompanied by the mine-viewer, and received recommendations from the best informed men upon the spot; and they were convinced that the conditions under which the accident happened were so variable, that no general practical rule could be obtained. Far more information, however, was required. The plan of splitting the air-courses was good, as far as the power of the upcast shaft admitted; but, if carried too far, it would produce stagnant points, which could not be prevented by any arrangement consistently with the ever-moving condition of the works. The abolition of the use of gunpowder and lighted candles would, in some cases, double the price of coals. But the great source of danger was the mental condition of the miners. With regard to the present race this was so hopeless that nothing could be done for them; although smoking was strictly forbidden, they had been known to contrive to light their pipes in dangerous workings even from the Davy lamp; and Mr. Faraday had himself on one occasion sat down with an open candle to watch the preparations for blasting, and when he inquired for the gunpowder was told he was sitting on it. Mr. Faraday took an opportunity, also, of expressing his

* British Association. From the *Athenæum*, in which a full report of the meeting will be found.

firm conviction of the safety of the Davy lamp when properly used, and of its being a complete and practical contrivance, to which he would willingly trust his own life, as he had already done on many occasions.

AN IMPROVEMENT IN THE METHOD OF TAKING POSITIVE TALBOTYPES (CALOTYPES).*

BY SIR D. BREWSTER.

In the method now in use, the face of the negative talbotype is placed directly upon the side of the paper which has been brushed over with a solution of nitrate, or ammonia-nitrate, of silver, and which is to receive the positive picture. In strong sunlight the picture is thus taken very quickly; but there is a roughness in the shades, owing to the formation of black specks, which destroys the softness of the picture, and in portraits gives a disagreeable harshness to the human face. In order to remove this defect, the author first interposed thin plates of glass, with their surfaces sometimes ground and sometimes polished; but, though the divergency or diffusion of the light, passing through the *negative* picture, produced great softness in the *positive*, yet the outlines were too indistinct, though the talbotypes looked very well, when placed at a distance. He then tried the effect of interposing a sheet of writing paper, without the water-mark and of uniform texture. The result of this experiment fully answered his expectations. The diffusion of the light thus occasioned shaded off, as it were, all the sharp lines and points, and gave a high degree of softness to the picture. The effect was even improved by interposing *two* sheets of clean paper; and, with a very bright meridian sun, he found that *three* sheets may be used with advantage. A similar effect may be obtained, in a smaller degree, by placing the *back* of the negative upon the positive paper, so as to cause the light to traverse the thickness of the negative, and this may be combined with one or more sheets of clean paper. This, of course, will be appropriate only with portraits; and it has the advantage (sometimes required) of making the figure look another way. To those who see the experiments above described for the first time, the effect is almost magical; and when the negative is removed, we see only a blank sheet of white paper; and our surprise is very great when, upon lifting this sheet, we discover beneath it a perfect picture, which seems as it were to have passed through the opaque and impervious screen. Sir D. Brewster exhibited specimens of portraits produced in this manner, and also specimens produced by the transmission of light through two perfectly coincident negatives of different degrees of strength; together with specimens of positives, produced by placing the positive paper between two perfectly coincident negatives, and acted upon by light incident on both sides of the picture. Sir D. Brewster mentioned some unexpected theoretical results, which these experiments indicated, but which required further investigation.

* British Association.

ON MINERAL MANURE.

BY PROFESSOR LIEBIG.

After alluding to the varying conditions of soils under the circumstances of geological and geographical position, Dr. Dieffenbach proceeded to point out the advantage of using such a compound, that it should meet these circumstances. For instance, that for a moist climate it would be necessary so to prepare the saline manure that, although slowly decomposing to meet the requirements of vegetation, it should not be so soluble as to be washed out of the soil by the rains. This is proposed to be effected by Professor Liebig; and, having ascertained the mineral constituents removed from the soil, it will be the object of the manufacturer to supply their place in the mineral manure. It was then pointed out that the manures required for wheat, oats, leguminous plants, and the potato, must necessarily be different; and the object in view in the manufacture of this saline manure is to prepare it with the essential constituents for particular crops.

After the reading of these agricultural papers, a conversation ensued. Dr. Daubeny stated, that the fact of the proportion of inorganic ingredients varying in different parts of the plant was new to him; and he would be glad to know if the same difference was found to exist with other principles besides silex, as he regarded the silex merely as an excretion. He remarked, that plants grown in different places do not differ from each other so much as they had been stated by Liebig to do. He had analysed plants taken from the sea-coast, and did not find that they contained more soda than those which he had procured from the centre of England; indeed, in one case the excess was found in plants from one of the midland counties.—Professor Solly confirmed Professor Johnston's statement as to the different inorganic constitution of different parts of the plant, and stated that he had found great differences to exist at different periods of the growth of the plant.

LIME AND COMMON SALT.*

At the July meeting, of the Darlington club, Mr. Thomas Dixon, the honorary secretary, said: "Some of the leading points to be considered are—The proper quantity of those substances for one application; the best season of the year for applying them; the description of crop most suitable for applying them to; the proper interval of time between the applications; and whether it is more judicious to apply those substances to the land singly, or in combination with each other, or with other substances; and, in order to arrive at proper conclusions, it is very important that we know something about the probable quantity of these substances that each description of crop extracts from the land on which it grows. Limestone, of the best kind for agricultural purposes, is chiefly composed of carbonate of lime, from which, during the process of burning, the carbonic acid is driven off, and then it becomes what we call quicklime. This quicklime, by long exposure to the air, absorbs moisture, and falls into powder, and is then slaked lime, or, what chemists term hydrate of lime. Now if the slaked lime continue exposed to the air for a considerable time longer, it will absorb a considerable quantity of carbonic acid, and by that means it begins again gradually to assume its original quality before burning; with this advantage, however, that it is now reduced to the finest powder—much finer than it could be ground or made by any other means—and therefore it mixes intimately with the soil, and thus furnishes nourishment to the roots of growing crops. Now, the carbonic acid forms, I believe, a very considerable element in the constituent parts of all plants and crops. I am, therefore, of opinion that lime, and more particularly magnesian lime, is seldom allowed to lie a sufficient time after burning, before being applied to the land. Now, in order that lime may yield carbon to the soil, to the greatest possible extent, I am rather an advocate for composting or mixing quicklime with earth, such as hedge-back weeds, quickened heaps, or ditch clearings; as by that means, when covered over and mixed with vegetable earth, carbonic acid is formed, during the fermentation, to a much greater extent than is generally the case when the lime is simply exposed to the atmosphere. In speaking of composting, I may state my belief that it is of greater importance in the case of magnesian lime, than in that of the more pure descriptions; for composting has the effect (to some extent at least) of subduing the caustic qualities and neutralising the poisonous effects that are known to result from an overdose of magnesian lime; for it is a fact that has long been known that, when lime contains a great quantity of magnesia, it has, in many instances, when applied to the land in very considerable quantity, a poisonous effect upon vegetation. Then, with regard to the interval of time which ought to elapse between one application and another of lime to the same land, a good deal must depend upon the description of crops produced from the land during the interval, as one description of crop is known to extract a much greater quantity of lime from the soil than another. According to Professor Johnston, lime is extracted from the land in about the following proportions, according to the crop produced:—

25	bushels of	Wheat,	about 9	lbs. of lime.
38	,,	Barley,	,, 15	,,
50	,,	Oats,	9	,,
25	tons of	Turnips,	,, 140	,,
9	,,	Potatoes	,, 270	,,
2	,,	Red clover	,, 126	,,
2	,,	Rye grass	,, 33	,,

"Now, it is quite evident, from this comparison, that, in regulating the application of lime, much depends on the crops produced on the land. The other subject for this day's discussion, 'Salt,' I have had much less experience in the use of, and there ore I am much less able to judge either of its merits or its application, than of lime; but yet I have observed sufficient to convince me that it has, in agriculture, some important uses. As a manure, however, I apprehend that it will be found more generally useful in combination with other substances, than when applied to the land by itself; unless it be applied as a medicine or a remedy against some disease, or irregularity of the soil or its crops; as it is, I believe, an enemy to canker and insects. In applying salt to the land I have invariably found that it gives the land more consistency and firmness, and therefore I have no doubt that, in combination with soot, or other proper substances, it will be an excellent top-dressing for wheat and other crops in the spring, and more particularly on land where there is danger of the crop losing its roothold at that season. I also think that it might be useful to add a little salt to the dunghills. In the discussion which ensued on the lime question, several of the members were of opinion that, upon the poorer descriptions of clay land, which had been long under the plough, the good resulting from a dressing of lime was scarcely, if at all, perceptible, and therefore they considered that upon very poor worn-out land

* *Agricultural Gazette*, Aug. 2, 1843.

it would not pay the expense; but upon land of a better description an occasional liming was evidently useful."

The chairman detailed the results of some of his applications of lime. Some years ago, on a poorish field that had been long under the plough, he applied four double-horse cart-loads per acre; and in order to see the benefit more clearly, he left every fourth ridge without applying any lime, and in the following and succeeding crops he could not see that the limed land produced any better crop than those ridges which were left unlimed. He, therefore, thought that it did very little, if any, good. On another field, which had only been ploughed six or seven years, he applied five double-horse cart-loads per acre, and in this field also he omitted every fourth ridge, and in the following wheat crop the produce was nearly double on the limed, to what it was on the unlimed ridges. On another poor field, in which the crops were very subject to go off, and which previously had been long ploughed, but, at that time, had been two years in seeds, he limed with five double-horse cart-loads per acre; and, in sowing with wheat, he used the land pressers. In that case, the crop succeeded far beyond his expectations; in fact, it was a better crop than had ever been known to grow upon that field before. It also stood well upon the land, no part of it going off as had usually been the case there. From those results he felt certain that it was of no use applying lime to poor worn-out clay lands, unless they were first laid down to seeds for two or three years. The secretary and some other members recommended composting the lime for the former descriptions of land, by which means clover could be grown occasionally, and the course of cropping could thereby be more varied, they having experienced the advantages of this mode of applying lime to poor land. With regard to the quantity for one application, the interval between each application, and the particular species of crop to which it was best to apply lime, no resolution could be come to, so much was considered to depend upon the quality of the lime applied, the description of soil, and the course of cropping pursued; but it was considered that where pure lime was used it was better to apply a pretty full dressing perhaps once in nine years.

OBSERVATIONS ON THE ASHES OF PLANTS.

BY PROF. JOHNSTON,

Prof. Johnston drew attention to the differences in quantity of ash left, not only by different parts of the plant, but by the same part of different plants and by different portions of the same part. Thus, he stated that the ash of wheat-straw varied from less than 3 to upwards of 18 per cent.; that of oats from 3 to 10 per cent. in different samples. But a more interesting result was, that the quantity of ash left by different parts of straw varied very much. In wheat-straw it generally increases upwards. These facts are of importance, both in a physiological and in a practical point of view. In regard both to what may be considered as essential to the several parts of the plant, and to the functions of the inorganic constituents which are found in them.

ON RECENT EXPERIMENTS ON THE GAS VOLTAIC BATTERY.*

BY PROFESSOR GROVE.

No previous description of the gas battery having been given before the Association, Professor Grove entered into an explanation of the action of hydrogen gas upon spongy platina, and gave a description of the first gas battery constructed with platina wire sealed into glass tubes in pairs—hydrogen being put into one tube and oxygen into the other. An arrangement of this kind being connected with a voltameter, it was found that exactly the same quantity of gases was eliminated in the tubes of that instrument as combined in the tubes of the battery. Experiments have been made with a view of ascertaining if other gases might be used in the battery, and it was discovered that a great variety of gases might be so used. Professor Grove then pointed out how perfectly any eudiometric analyses might be carried on with the gas battery, provided some attention were paid to a few sources of error. A form of the instrument as hitherto constructed by Professor Grove was described, for the purpose of avoiding the absorption of atmospheric air by the fluid in the cells of the battery. A more recent construction of the battery was next described, in which many of the imperfections of the former instruments were removed, and a combination of several pairs of gas tubes are connected in one compact body. Another advantage arising from this battery is the really constant condition of it; once charged, it appears that the action will go on for years, requiring nothing more than occasionally, at long intervals, adding a little zinc to the acidulated solution in the cells, for the purpose of supplying the loss of hydrogen in

* British Association.

the tubes. The results of long experiment have shown, that the most invariable action may be calculated on for years; and that, by this instrument, experiments requiring for a long period the constant flow of a galvanic current may be most effectually carried out. Some experiments on the combination of phosphorus and sulphur with oxygen in the battery were then named; and it was found, that any body capable of volatilisation gave a galvanic action with oxygen in the other tube. Camphor, alcohol, ether, and other bodies proved the constancy of this effect. It was then stated, that in all cases it had been found that chemical action and voltaic action were convertible into each other.

ON THE MANUFACTURE OF A COLORED GLASS.*

BY M. SPLITGERBER.

SPECIMENS of glass were exhibited, into the composition of which gold entered as a chloride. These specimens were white, but upon gently heating them in the flame of a spirit lamp they became a deep red, transmitting the red rays of light only. If again the same reddened glass is exposed to the heat of an oxygen blow-pipe it loses nearly all its color, a slight pinkness only remaining. M. Splitgerber considers these results to arise from the oxidation of the chloride of gold in the siliceous compound.

REMARKS ON THE QUALITIES OF WHEAT NECESSARY FOR THE MANUFACTURE OF BREAD BY THE ORDINARY AND PATENT PROCESSES.†

BY SIR G. S. MACKENZIE, BART., F.R.S., V.P.R.S.E., ETC.

LAST year I published a pamphlet on the choice of wheat for seed, in which an attempt was made to set forth the importance of examining the chemical constituents of wheat, in reference to the manufacture of bread. I failed to impress this on the Highland and Agricultural Society of Scotland; and, though the subject has not been taken up by the Agricultural Chemistry Association, nor by farmers themselves, I can only regret these facts. They have not deterred me, however, from following up a pursuit manifestly fraught with importance, and the results of my inquiries have fully justified my perseverance. Such inquiries are unquestionably too expensive to be made by individuals, and ought to be taken up by public bodies. But, unfortunately, the latter have their attention too much attracted by useless cattle-shows, the effects of which seem to be not to benefit the public by furnishing wholesome, cheap, and abundant food, but to put money into the pockets of butchers, the providers of dinners, the builders of pavilions, and newspaper printers, besides gratifying self-love in after-dinner speeches, meaning little, and effecting nothing.

Circumstances, as I will presently show, have occurred to render the inquiries referred to greatly more interesting and valuable than they were when they were commenced.

At the time when I offered my pamphlet to public consideration, I was impressed by the belief that the nitrogenous ingredients of wheat were those which were chiefly nutritive; and that the varieties which contained the largest proportion of these, were the most valuable. This is unquestionably true; but the result of chemical investigation has proved that the wheats in greatest estimation for making bread contained a much smaller proportion of these matters than others which bakers rejected as unfit for their process. This fact is important in itself; and another has been elicited, also of importance, that the success of the manufacture of bread by the ordinary process, in other words, the baking quality of wheat depends on the proportion of sugar.

Now, every one knows that sugar, gluten, and albumen, constitute the chief nutritive ingredients of wheat. But every one may not be aware of what chemistry further tells us, that, in the ordinary process of baking, the sugar is totally destroyed, and part of the gluten. Let us see, then, how matters at present stand in reference to market prices, and the real value of particular varieties of wheat as an article of food. At the time I published my pamphlet, a Baltic wheat, known by the name of wolgast, was rated at 58s. per qr.; another wheat, a new variety sent to me by Col. Le Coteur, and called by him trémois (which was erroneously written by me tremaine), was examined by six different persons—three corn-dealers and three bakers, and the average of the values stated by them was 47s., 11s. under the value of the wolgast. Having obtained more minute analyses of these wheats, I find that the amount of the nutritive ingredients in wolgast wheat is 14·6 per cent., and that in the trémois 17·8 per cent., showing a difference in favor of the latter (which grew in Ross-shire) of 3 2. Compared with a Ross-shire wheat, the difference against the wolgast is

* British Association.

† *Agricultural Gazette*, Aug. 9, 1845.

4·7. As the sugar is lost in baking we may leave that out, when in comparison with trémois wheat the wolgast loses 4·1. Thus, then, it appears that the rejected wheat had the greatest proportion of nutritive matter, and was, in fact, the most valuable. This is a fact of too great consequence to be overlooked. Of course it is most important to prevent, if possible, the loss of the sugar and any portion of the gluten in the process of baking.

This leads me to notice some matters well worthy of attention, connected with the patent recently obtained for raising bread by the process of uniting hydrochloric acid with carbonate of soda, and thus, at the same time, introducing salt, which is necessary to render the bread palatable. This process leaves the nutritious ingredients of wheat untouched; and hence the patent bread ought to be, and, if made of nutritious wheat, unquestionably is, the most nourishing. It is obvious, however, that this patent process does not, like the old one, require wheat of a particular quality. Flour, containing scarcely any nitrogenous ingredients, may be made by it to rise into light bread. The raising is a mechanical operation effected in the one case by chemical action producing vinous fermentation, and the decomposition of the sugar, which causes carbonic acid gas and alcohol to escape; and in the other case the addition of hydrochloric acid to the carbonate of soda, sets the carbonic acid free, while common salt is formed. By this latter process bread, to all appearance good, and at the same time palatable, but not very nutritious, may be imposed on the public by dishonest persons, or even in ignorance. It has become a matter of indifference to the baker what flour he uses. In the old process of baking, the use of alum, for whitening the bread, was limited, because its use beyond a certain amount stopped the fermentation. In the new process adulteration is facilitated. The public, therefore, is very deeply interested in the matter. We now see that the ordinary process of baking actually destroys, to a considerable extent, the nutritive portions of wheat, and therefore the patent process should be encouraged. But, at the same time, it will be necessary that, to secure its advantages, no other than wheat of the most nutritive quality be used. The chemist, therefore, must be applied to, in order to check any tendency to fraud. As farmers and their families eat bread it becomes their interest to look for and cultivate wheats that yield the largest proportions of nutritive matter.

It is the interest also of the patentee of the new process to promote the search for wheats that, while suitable to the climate of Britain, shall be productive, and contain what is required in large proportion. Thus a complete revolution is likely to take place in the kinds of wheat cultivated at home, and the choice of wheats selected for importation; and I hope I may not only have laid the foundation for it, but that I may live to see its accomplishment. I am already in possession of minute analyses of a considerable number of wheats, but until I obtain some more analyses of wheats I have now growing, I will defer submitting any till all are made.

Should this meet the eyes of the leaders of the great Agricultural Association of England, I shall be happy to correspond with them on the means which they may adopt for extending our knowledge of this interesting and most important subject. Should the patentee of the new baking process see these remarks, I shall be ready to communicate with him in regard to the deep interest he may feel in reference to what those bakers to whom he has sold licenses may do.

ON THE ASHES OF NARCOTIC PLANTS.

BY F. C. WRIGHTSON.

The great problem of chemistry of the present day in its application to agriculture is, to determine the conditions necessary for a soil to produce the largest amount of any given crop of animal food—of food for man or beast. The solution to this question will in some measure be found when we have ascertained the proportions of mineral constituents required by plants of culture for attaining full perfection. The analysis of these plants alone, however, is not sufficient; we must have analyses of the weeds growing upon soils, of plants unfit for the sustenance of animals; for it is shown, that they rob the farmer, not only of a large amount of mineral manure, but also of considerable quantities of that valuable ingredient, ammonia; and we shall not have obtained the data necessary for an improved and rational system of culture, until we include in our ash analyses a considerable number of weeds and useless plants, especially those of the most destructive kind. Some of them are well known to the farmer to be of serious injury to his crops mechanically; with respect to their abstracting important constituents of the soil, their injurous effects cannot be questioned. These plants contain more than 8 per cent. chlorine, and from 30 to 50 per cent. alkalies in their ashes. The dried leaves contain between 6 and 8 per cent. of nitrogen.

ON SILICATE OF SODA AS A MANURE.*

BY PROFESSOR JOHNSTON.

I HAVE adverted to the propriety of mixing silicate of potash or soda, with guano or dissolved bones, before applying them to the land. As the use of these silicates is at present much recommended, it may be proper to consider how far they are really necessary to the soil.

1st. If a portion of soil, burned, in order to destroy the organic matter it contains, be then boiled in distilled water for a length of time, and the filtered solution be afterwards evaporated to dryness, and the residue heated to redness, to burn off the organic matter, a minute but variable quantity of saline matter will remain, in which a trace of silica will usually be found. Most soils, therefore, contain some silica in a state in which it may be taken up by water.

2nd. If, after boiling with water, the soil be digested for two or three hours with concentrated muriatic acid till it is perfectly colorless, and if the filtered solution be treated with ammonia and oxalate of ammonia, to separate the oxide of iron, alumina, and lime, and be then evaporated to dryness, an appreciable quantity of silica will be found in the saline matter which remains. Or, if the acid solution be at once evaporated to dryness, and then treated with water, some silica will remain insoluble. Soils, therefore, generally contain a further appreciable quantity of silica, in a state in which it is soluble in acid solutions. In the following table, the last line but one shows the proportions of soluble silica thus obtained from four different soils analysed in my laboratory:

	Auchengate Farm.	Thandersley Hall Farm, Essex.	Stemster by Thurso.	White Hill, Lasswade.	Kirkcudbright.	
					Top.	Bottom.
Organic matter	14·15	4·27	8·43	6·64	26·86	18·57
Alkaline salts, soluble in water and acids	0·06	0·94	0·49	0·20	1·41	0·46
Gypsum	0·02	0·02	0·03	0·09	0·11	0·07
Oxide of iron	2·09	7·00	5·61	4·46	4·37	5·93
Alumina, soluble in acids	0·60	5·14	3·08	1·08	2·44	2·90
Carbonate of lime	0·12	1·05	0·25	2·32	2·26	1·47
Carbonate of magnesia	0·26	0·78	0·45	1·02	0·70	1·86
Soluble silica	0·19	0·16	0·20	0·17	0·84	0·42
Insoluble siliceous mattter	82·95	80·29	80·39	83·31	60·47	67·24
	100·44	99·65	98·93	99·29	99·46	98·92

I could have given other such results, but these show both the presence of silica in this state in our cultivated soils, and how much it varies in different soils. In the fifth column it is four times as great as in the first and second. The quantity present in the first two columns—1-5th of a per cent., or one pound in 500 lbs. of soil—appears very small when compared with the whole soil, and yet it proves to be large when we come to calculate how much an acre of soil will contain, and how much a crop of corn will carry off from that acre. A square yard of soil, 12 inches deep, will weigh about 700 lbs., so that this extent of soil will contain 1¾ lbs. of silica soluble in acids, which amounts to 6,700 lbs. in an acre. The straw of our grain crops leaves, on an average, less than 5 per cent. of ash, of which less than one-half usually consists of silica, and a crop of straw rarely exceeds 3,000 lbs. per imperial acre. If we take these numbers as tolerable approximations, the quantity of silica carried off by a crop of straw amounts to 75 lbs. per acre; while the silica soluble in acids, and which it is believed plants can take up, amounts as above to 6,700 lbs. Or this silica will alone supply the wants of 900 crops of corn. The soil in the fifth column contains four times as much, or it would provide silica for 3,600 crops.

3rd. If the insoluble portion of the soil be washed with water, to free it from siliceous sand, and the finer or clayey part be collected and fused with carbonate of soda, and then treated with muriatic acid, a large portion of silica, in light white flocks, will remain behind; while the solution will contain alumina, lime, magnesia, and perhaps potash and soda, with which the silica had been combined in the soil. This portion of the silica of the soil is very large, and sometimes forms nearly one-half of its whole weight. In the form of silicates insoluble in muriatic acid, there-

* *Agricultural Gazette*, August 9, 1845.

fore, the soil contains an enormous quantity of silica. There can be no douht that the roots of our corn-crops are capable of taking up and appropriating that portion of the silica of the soil which is soluble in water, but it may be doubted by some if that which is taken up by acids, and especially if that which acids leave undissolved, is capable of supplying the entire wants of our growing crops.

I will not offer any of those theoretical considerations which induce chemists and physiologists to believe that both the silicates which are soluble and those which are insoluble in acids are really a source of mineral nourishment to the plant. An appeal to experiment will be much more satisfactory to the practical man. Wiegman and Polstorf* took fine white quartz sand, burned it to destroy any organic matter it might contain, digested it for 16 hours in strong nitro-muriatic acid, and then washed it with distilled water. Everything soluble being thus removed, seeds of different kinds—barley, oats, buckwheat, vetch, clover, and tobacco—were sown in it, and watered with distilled water. They all grew and came to a greater or less height. The ash left by separate portions of the seeds and plants, when full-grown, was determined and analysed. In the following table I have put together their comparative results, in so far as the silica is concerned, as well as the height to which the plants had arrived when they were gathered :—

	Height.	Silica in the Ash		Silica had increased.
		Of the Seed.	Of the Plant.	
	In.			Times.
Barley	15	0·034	0·355	10
Oats.....	18	0·064	0·354	5½
Buckwheat	18	0·004	0·075	18
Vetch	10	0·013	0·135	10
Clover....	3½	0·009	0·091	10
Tobacco ..	5	0·001	0·549	500

Thus it appears that, from quartz sand, after it has been digested in strong acids, plants are able to extract silica as well as other kinds of inorganic nourishment, to which, in the above table, I have not alluded. The sand in which the plants had grown was found, upon analysis, to contain 97·9 per cent. of pure silica, the remainder consisting of potash, lime, magnesia, alumina, and oxide of iron.

* *Ueber die Unorganischen Bestundtherle der Pflanzen-Braunschweig.* 1842.

From this experiment, therefore, we are justified in concluding that plants are capable of decomposing and extracting silica even from those silicates in the soil which are not attacked by acids. How much more easily may they decompose and appropriate the constituents of those silicates which are capable of being decomposed and dissolved by acids. Nor is it by virtue of any mysterious power that this decomposition is effected. The prolonged action of water containing carbonic acid, slowly decomposes even the most refractory silicates. The water in the soil is more or less charged with this carbonic acid—the decaying vegetable matter of the soil produces it, and the roots of living plants give it off—so that a simple chemical action is sufficient to explain how the plant can obtain food from such unpromising materials.

It is an important conclusion, therefore, for practical agriculture, that the silica, which is so necessary to the growth of our corn and hay crops, abounds so much in nearly all our soils, and in a state in which the roots of plants can more or less readily take it up. It satisfies us in regard to three points :—*a*. As to where and how the plants obtain their silica. *b*. That, in general, it cannot be necessary to add silica in any form as a manure to the soil, and therefore that—*c*. The broad assertions of those who say that guano or bones should always be mixed with silicate of potass or soda, and who ascribe certain alleged failures of these manures to the absence of such silicates—that these assertions are hastily made and are not to be depended upon. I believe there are very few soils indeed, which, if properly treated otherwise, will refuse to yield good crops merely because a soluble silicate has not been added to them.

Is the use of silicate of potash or soda, then, not to be recommended? Are there no circumstances under which it may prove profitable to the practical man? These are questions which, notwithstanding what has been said in the preceding remarks, it would be premature to answer in the negative. Soils which contain thousands of pounds of potash and soda in a state of chemical combination, from which plants can obtain it by degrees, are yet often rendered more productive by the addition of a few cwts. of wood-ashes or common salt, from which the plants can obtain alkaline matter without delay. It may be so, therefore, with the silicates.

There is only one legitimate purpose, however, for which a silicate should be added to the soil—it is to supply silica to the plant. We ought not to add silicates for the pur-

pose of supplying alkaline or earthy matter, unless some question of economy interferes. We use wood-ashes, which contain silicates, because it is cheaper to add potash in this than in any other form. There is only one evidence, therefore, that the addition of a silicate has done good to our corn crop; it must have given more silica, and consequently, as it is believed, more strength to the straw. That the crop has been generally improved, that it is more luxuriant and thriving, or even that it yields a larger return of corn, will not be a proof of the virtue of the silicate. The effect may result from the action of the potash or the soda it contains, and the use of these alone might have produced the same effect.

Oats grown upon a mossy soil are soft and dull in the straw, and I have found such straw to be deficient in silica. Wheat growing rankly upon a moist soil, and in a moist season, or a heavy thickly growing crop of barley, is apt to be laid by the wind, or to fall down under the weight of the ear. The straw is too feeble in all these cases, and it has been said that the use of silicates would abate the evil. Lampadius found the growth of wheat and rye to be greatly promoted by the use of a soluble silicate of potash, but the alkali might be the cause of the effect observed, and no other experiments of an accurate kind have yet been made. The late Mr. Oliver, of Lochend, applied silicate of soda, at my suggestion, to his corn crop last year, but his unfortunate death prevented the results from being observed.

It is still an open question, then, whether or not we have it in our power, at our pleasure, to strengthen the straw of our corn plants—whether it would be useful to apply the soluble silicates to all soils—whether there are any to which they may be applied with advantage and profit as an ordinary manure.

Irrespective of theory, it is important to determine this point. There are certain practical evils which it is desirable to overcome; will the use of silicates overcome them? Theory does her part best by practice, when, instead of boldly pronouncing upon doubtful points, she suggests experiments by which they are likely to be cleared up. It is a fair subject for trial, therefore, which I hope some of our skilful experimental farmers will take up. Silicate of soda may now be had of the Messrs. Turnbulls, of Glasgow, at 12s. per cwt. Mr. Campbell, of Craigie, has some patches on trial. I hope others will follow his example. To eliminate the effect of the soda from the experiment, three experimental patches ought to be chosen in duplicate or triplicate. Thus:—

Nothing.	1 cwt. of Silicate of Soda.	1 cwt. of Carbonate of Soda
1 cwt. of Carbonate of Soda	Nothing.	1 cwt. of Silicate.
1 cwt. of Silicate.	1 cwt. of Carbonate.	Nothing.

Not only should observations be made as to the rapidity and luxuriance of growth and to the strength of the straw, but an average sample of each of the straws should be analysed, for the purpose of ascertaining whether any more silica has really been taken up by the one part of the crop than by the other. Such experiments as these will, in a year or two, settle the point, and will free the practical man from the necessity of relying upon, or being guided by, the mere opinions and hypotheses of theoretical chemists.

EFFECT OF OILCAKE ON THE MANURE OF THE ANIMALS FED ON IT.*

BY G. DOBITO.

A FRIEND of mine has lately adopted a plan which, under the same circumstances, I should strongly recommend; it is that of giving a small quantity of oilcake to animals grazing, for the sake of improving an ordinary pasture, and its effects are astonishing. The pastures I allude to are small, and one or two bullocks more than they are calculated to carry are put into each; the lot are then allowed 4lbs. of cake per day per head; this, at a cost of about 2s. per head per week—which, I believe the stock well paid for—has entirely altered the face of the pastures from what they were three years ago, when the plan was first adopted by him; and, I believe, without any loss to himself.

ON MASSES OF SALT DISCOVERED IN THE LOWEST PORTIONS OF GUANO ON THE ISLAND OF ICHABOE.†

BY MR. PEARSALL.

THE salt was transparent and colorless, very soluble in water, and contained phosphoric acid, soda, and ammonia, and a trace of some organic matter, which became carbonaceous at a high temperature. The salt, therefore, appears to be a microcosmic salt, phosphate of soda, and ammonia.

* *English Agricultural Society's Journal.*
† British Association.

INSURANCE OF LIVE STOCK.

It is a subject of surprise that, amongst a class of individuals representing so vast an amount of interest as the agriculturists of the United Kingdom, no effort has hitherto been made for *indemnification*, by *insurance upon live stock*, against the numerous losses to which they are exposed, by disease or accident. This desideratum has, at length, been effected by the "Agriculturist Cattle Insurance Company," whose advertisement appears with the present number of The Chemist, to which, for further particulars, we beg to refer our readers.

CHICORY, AS FOOD FOR LIVE STOCK.*

Although the cultivation of common chicory has been a good deal recommended as food for live stock, particularly cattle and swine, yet it seems not to have hitherto been followed to that extent to which its superior merits entitle it. It thrives best on rich soils, and such as are rather light and well drained. It may be sown alone, either in drills or broadcast, and mixed in small quantities, amongst grass, clover, and other seeds for pasture lands. When the drill system is adopted, which in most cases is to be preferred, the rows may be at from fifteen to twenty inches distant, to admit of hand and horse-hoeing, and the plants should stand at least six inches distant in the rows; the sowing may be deferred till the month of May, as, if this be done at an early period of the season, the plants are apt to commence running to seed in the autumn months, whereby they will be materially weakened, and the first crop of the succeeding season considerably lessened. The operation of thinning should be attended to when the plants are very young; and they should be pulled with the hand in soft weather, in preference to cutting them with a hoe, as the roots are very tenacious of life, and will push out new buds although cut a good deal below the surface. One considerable crop of root-leaves may be obtained in the end of autumn the first season, and three, four, or even more, may be obtained under very favorable circumstances during each of the four or five seasons following, particularly if the first cutting be completed about the time of the plants first showing their flowers, when the average height will be from three to five feet; and in any case, the cutting should not be deferred until the plants arrive at a more advanced state, as the stalks then become hard and the future vigor of the plants impaired. The broadcast system should only be attempted where the soil is previously in a high state of fertility and completely free from root and annual weeds. The period of duration from the time of sowing will be from four to six years, if cut regularly before arriving at full flower; after which they may be ploughed up and the ground fallowed to get clear of the roots; if, however, the seed be allowed to ripen, the plants will lose much of their vigor even after the first crop. When drilled, 4 lbs. or 5 lbs. will be a sufficient quantity of seed per acre, but 10 lbs. or 12 lbs. will be necessary for sowing broadcast. There are numerous varieties of chicory, none of which seem possessed of much permanency in their characters when raised from seed, at least so far as accords with present experience; the most distinct, however, is one with long thick roots, distinguished in France by the name of *Chiccorée à café*, which is there grown for its roots as a substitute for coffee.

* Lawson's *Agriculturists' Manual.*

ON A MOLECULAR MODIFICATION OF OIL OF TURPENTINE, RENDERING IT CAPABLE OF MORE READILY DISSOLVING CAOUTCHOUC.*

BY M. BOUCHARDAT.

Ten years ago, I was consulted by a manufacturer of waterproof-cloth, to discover the best solvent for caoutchouc. At that time, the practice in England was to employ coal-tar naptha, or an oil obtained by the distillation of caoutchouc over a naked fire.

I commenced by carefully examining this pyrogenous oil. I separated from it several well-defined hydro-carbons, remarkable for their low boiling point, but I soon found that the price of this oil would long prevent its use in manufactures. The essential oil from coal has so disgusting and permanent a smell, that I resolved to seek for another solvent.

From the first, I thought that turpentine, which, as is well known, dissolves caoutchouc, might be so modified by heat as to supply the solvent we required, and experience proved the truth of this supposition. By distilling oil of turpentine once or twice over a naked fire, we obtain a very satisfactory solvent. In making this distillation on brick, the essence being submitted to a higher temperature, it becomes very little inferior to caoutchouc. This is now the solvent for caoutchouc, employed both in France and

* *Comptes Rendus*, and *Lancet.*

England, for the manufacture of impermeable textures. What change has this process effected in the turpentine? Does it form new volatile products, or is it simply a modification of the molecular constitution of the oil? We can now answer these questions.

The physical properties of the oil distilled on brick are as follow:—The color is slightly yellow, its odor that of a mixture of thyme, naptha, and turpentine. It is more limpid than the essence from which it is formed, the density of the former being 0·8736, and after distillation, 0·8420. It be ginsto boil at 85° C., but the temperature immediately rises to 154°, and remains at this point nearly stationary. I have tried to isolate the more volatile part of the distilate, but without success. The unchanged oil boils at 151° to 158°, whilst the distilled boils at 150°. I analysed the modified oil, and found its composition precisely the same as the original. The nature of the modification in question being still obscure, during the last winter I employed the apparatus for polarization belonging to the Hôtel Dieu, to pursue the inquiry, and I found that by its means I was able to perceive the molecular change which the turpentine had undergone in becoming able to dissolve the caoutchouc.

Commercial oil of turpentine, acting imperfectly upon caoutchouc has a molecular rotation of —28°,83. After distillation over a naked fire, this became —33°,23; its power of dissolving caoutchouc increasing with its power of molecular rotation. If the same essence, however, is modified by a temperature still more elevated, as by distilling it on fragments of brick, its solvent power increases still further, but the change produced is accompanied by a considerable diminution in its rotatory power, which is then only —8°,68.

By varying the conditions under which the oil is distilled, various modifications are obtained, which prove that a substance with a definite composition may exist in several isomeric states.

ON MALACCA GUANO.*

BY DR. CANTER.

This communication described a peculiar substance, of which recently samples have been sent to England. It was stated to be inferior to both the African and the Peruvian, and curiously enough to consist almost entirely of the legs and other indigestible parts of beetles.

* British Association.

HUMAN EXCREMENT AS MANURE.*

BY M. BOUSSINGAULT.

Human excrement may be regarded as a most valuable manure. In countries where much attention is paid to agriculture, it is eagerly sought after for this purpose. In Flanders it is even an article of commerce, and is there much employed as a fertilising agent in the vicinity of all the populous cities. The Chinese are in the habit of collecting human fœces in cisterns, arranged at regular distances along the most frequented high roads. Old men, women, and children are employed in working up the manure and laying it around plants. The fresh excrement is sometimes mixed with clay and made into bricks, which are dried and powdered, and then spread over the soil. Owing to this practice, the fields in China are but little infested with weeds; while the plants which are cultivated thrive. The quality of this excrement as a manure depends much on the nature and abundance of the food consumed by the individuals by whom it is voided. In relation to this subject, M. D'Arcet has reported the following singular fact:—An agriculturist bought up, for the purpose of manure, the soil of the privies belonging to the establishment of one of the most fashionable *restaurateurs* in Paris! Encouraged by the success which attended this speculation, and wishing to extend his experiments, he entered into a contract for the whole of the night-soil derived from several Parisian barracks. This, however, turned out a loss, for the military excrement was found to be very inefficient as manure.

LIMING LAND.†

We do not think that lime can be applied with advantage to the potato crop, during any period of its growth; the proper way is, to work it well into the soil in spring, when preparing the ground for a crop. Poor clay land, well drained, will be much benefited by lime; and if it be previously compounded with a large quantity of moss, or vegetable matter of any kind, its good effects will be more apparent, as clay soils are generally deficient in vegetable or organic matter. It is considered a good plan to take two or three quarts of blood from a horse, before being turned out to grass; more especially if he has been previously overworked and his winter coat of hair has not come off well.

* *Economie Rurale.*

† *Irish Farmers' Gazette.*

LECTURE ON MANURES.*

BY MR. GYDE.

MR. GYDE commenced his lecture by treating of the constituents of plants—organic and inorganic—with the means of separating those parts, and also showed the great importance of the inorganic or earthy remains as a means at the disposal of the agriculturist; he then referred to the following table:—

Inorganic matter yielded by 100lbs. of—

Meadow hay	60 to 100lbs.
Clover hay	90
Potatoes	8 ,, 10
Turnips	8 ,, 10

	Grain.	Straw.
Wheat	22lbs.	50lbs.
Barley	20	50
Oats	35	60
Peas	28	50

Elm	19lbs.	Birch	3½lbs.
Poplar	20	Pine	3
Beech	4	Oak	2

Thus, so much inorganic matter, being annually carried off the land, was necessary to be replaced for the growth of future crops, and water was the medium to dissolve and carry inorganic matter to the roots of plants. The lecturer then referred to the following table of the composition of soils:—

Composition of Soils.	Fertile. Without manure.	With manure.	Barren.
Organic matter	97	50	40
Silica	648	833	778
Alumina	57	51	91
Lime	59	18	4
Magnesia	8½	8	1
Oxide of iron	61	13	81
Oxide of manganese	1	3	½
Potash and soda	6	trace	—
Chlorine	2	—	—
Sulphuric acid	2	¾	—
Phosphoric acid	4½	1¾	—
Carbonic acid	40	4½	—
Loss	14	—	4½
	1000	1000	1000

Next he gave a calculation of crops.

Inorganic matter carried off in a four years' rotation of—

	Turnips (roots).	Barley (grain).	Red Clover and Rye Grass.	Wheat. Grain and Straw.	Total. lbs.
Potash	145·5	10·1	73·5	3·9	233·0
Soda	64·3	6·9	21·0	4·4	96·6
Lime	45·8	15·0	79·5	8·7	149·0
Magnesia	15·5	5·4	9·5	2·5	32·9
Alumina	2·2	3·9	1·1	3·1	10·3
Silica	23·6	113·6	70·0	92·0	299·2
Sulphuric acid	49·0	4·0	18·0	1·8	72·8
Phosphoric acid	22·4	7·9	15·6	5·6	51·5
Chlorine	14·5	1·9	8·1	1·1	25·6
					970·9

The turnip crop, it would thus be seen, removed more inorganic matter than any other.

Vegetable manures made the soil porous, and yielded earthy and saline properties peculiarly adapted to its requirements, and he gave the proportions. Green vegetables more speedily decayed, and rendered up its constituents; but dry vegetable manures, such as chaff, saw-dust, &c.; required a long time for decomposition. Among other green vegetables he drew attention to the use of sea-weed, which on the coast of Kent had been found to double the value and products of the land; this being in consequence of the large quantity of earthy matter supplied by sea-weed. Farm-yard dung, as a good fertiliser, he called the sheet-anchor of agriculture, but little care or husbandry had hitherto been bestowed on it: most frequently it was allowed to accumulate in heaps, ferment, occasionally it was mouldered away or trodden down, exposed to the winter rains, and its vitalising principle washed away into ditches; whereas it should be carried to the fields covered over with a layer of earth, and, when uncovered, applied without delay. Where a tank was kept, it was also advisable to collect saw-dust, weeds, &c., near to the spot, saturate the mixture till in a state of decomposition, and then it would be found nearly as good as dung. The application of dung must depend on the object required; if its effects were required to be slow and lasting, long dung must be put on, and *vice versa*. Bran, dust, &c., did not last long on

* From the *Agricultural Magazine*, Aug., 1845.

the soil. Soot, as a top-dressing, was beneficial. The nitrates produced good foliage and increased the straw, but did not make an increase of crop certain; the nitrate of soda, however, produced more gluten, or nutrition, as had been produced by Mr. Hyett's experiments, repeated by Liebig and Dr. Daubeny. Gypsum, as a top-dressing, was much valued, especially for clover; and sulphate of ammonia and gas-liquor were also good as a top dressing; but quick lime was never to be used with them or with farm-yard dung, because it caused the ammonia, or fertilising principle, to escape. Salt would be beneficial with farm-yard manure, but of course salt failed within a short distance of the sea, owing to the soil being already saturated with it.

Of the animal substances used as manures, flesh and blood yielded a large supply of ammonia; bones also were most efficient, being composed as follows:—

COMPOSITION OF BONE.

Phosphate of lime.....	555
Ditto magnesia.....	30
Carbonate of lime	48
Salts of soda.....	35
Gelatine	332
	1000

Phosphate of lime being the largest ingredient in the composition of bones, some speculators a few years ago fetched from a part of Spain a specimen, which, as it was chiefly composed of the phosphate, they expected would supersede the necessity of procuring bones. Dr. Daubeny subsequently sailed to Estremadura, and brought home about 8 cwt. (of which the lecturer produced a specimen). Spain however was much unsettled; and the article, which was a natural production, existing in a stratified form in the mountains, was difficult to be got at. Whether or not the article might be produced, and at a sufficiently cheap rate, when Spain should be quiet, was another question. The article was found in Cornwall, also, and experiments were already being made. With regard to the preparation of bones, burning would destroy all the gelatine; boiling would extract the grease, but not too much of the gelatine; and Liebig had recommended to dissolve the bones with acid. The latter had answered well: the acid dissolved and brought out the earthy or more valuable qualities; while the gelatine might be boiled into a jelly for making glue, for which it was cheaper than skins. He then proceeded to show the cheapness, efficacy, and fructification of bone manure when dissolved in sulphuric acid, and said that the result of experiments had been that the application of two bushels per acre in this way was better than sixteen in another. The proportion of the acid to the bones should be as one-half; he then showed the chemical affinities in this mixture, as separating and combining to the great advantage of the plants. The usual way of mixing was to put water with the acid to the extent of 100 to 1, but 50 to 1 had been found quite sufficient for the purpose, and more than that, the English farmer felt objections to, and difficulties in the way of, carting so much liquid manure. After giving some instructions for the mixing of compost, Mr. Gyde proceeded to speak of guano, and pointed out the following table:—

COMPOSITION OF GUANO.

	African.	Peruvian.
Water	24 ..	10
Organic matter, with salts of ammonia	38 ..	66
Bone earth	34 ..	22
Soluble salts.....	4 ..	2
	100	100

This calculation was of course made by taking the average, as no two samples were exactly alike; he spoke of the combinations it formed with different soils, but observed that the merits of the article were so well known as to require no comment from him. He then went on to speak of urinal manures, and produced a table from which it appeared that the urine of the cow contained a very large quantity of solid matter (900 lbs. in 1,300 lbs.), and was therefore valuable for the soil, as he proved by illustrations. From a calculation that had been made it appeared that a stock of 50 cows would produce in a year twenty tons of dry urinal deposit, which would be worth 200*l.*; but so badly was this generally managed that three-fourths of that amount was generally lost; and when they considered the whole kingdom in this calculation, the loss was no less than three millions of tons, worth 30,000,000*l.* per annum. The lecturer then touched on the matter of sewerage, regretting that the refuse of towns and cities was allowed to run away uselessly in our rivers and drains; he showed what was done in Paris by a method of saving it; and then went on to say that as the principal objection to the use of this manure was its effluvium, the objection might be got rid of cheaply and in five minutes by the simple application of sulphate of iron, dissolved in water, or sprinkled as a powder over the soil: it had also the advantage of fixing the ammonia.

The lecturer having terminated his paper, some of the persons present put a series of questions to him. With regard to the application of nitrate of soda, he replied that

light soils were the most eligible, as being deficient in saline matter; marl also would be useful for that purpose. As to the dissolving of bones with sulphuric acid, he recommended that the acid and water should be mixed first, and then the bones thrown in. He thought that Peruvian guano, although superior to African, was too dear to be preferred.

THE BEST MODE OF HARVESTING GRAIN CROPS.

A DISCUSSION lately took place at the Maidstone club embracing the subject of cutting, carrying, barning, and stacking grain crops, as well as some important points in the construction of the bagging-hook, and the comparative value of the different tools, implements, and buildings used in harvesting, and the following resolution was unanimously adopted:—"That it has been found desirable to cut wheat before it becomes sickle-eared, and other grain rather green. That if men can be got to bag wheat well, it is best to bag; the next best plan is to mow; but both bagging and mowing must be done well, or dirt will get into the sample, from the stalks which are torn up by the roots. That corn should always be made up in small sheaves. That when winter threshing is required, wheat threshed after having been kept in the barn is generally better than that kept in stack; but that for summer threshing, the sample is rather improved by being stacked."

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

INFLUENCE OF SULPHURETTED HYDROGEN GAS ON FISH.*

PREVIOUSLY to 1830, the waters of the harbor of Marseilles were tolerably pure, and contained a considerable quantity of fish. Some years ago, however, all the fish suddenly died; they were seen to float asphyxiated upon the surface of the water; and at the same time the odor of sulphuretted hydrogen became perceptible, and has since continued obvious to the sense of smell.

For a number of years a considerable quantity of soap has been made in the neighborhood of the town; and in its manufacture, the ley, which is rich in the sulphates, being allowed to escape, percolates through and between the various strata there composing the surface of the earth, and eventually reaches the waters of the harbor, when the sulphates, being in contact with decomposing organic matter, give rise to the production of sulphuretted hydrogen, which is at first dissolved in the water, and afterwards exhaled from its surface.

This observation may be applied to the explanation of certain geological phenomena. All the fossilised fish discovered in Provence by M. Blanchet were found lying on their sides, none being in a vertical posture; hence M. Blanchet concludes that they had perished at the moment of the irruption of the agent which had prevented their putrefaction. The same arrangement has been noticed in the remains found at Oningen and Solenhofen.

With respect to the probable cause of the death of these animals, M. Blanchet remarks that the calcareous strata at Aix, in which the fishes are found, rests immediately upon a bed of sulphate of lime of upwards of a yard in thickness. At Bouieux, the calcareous marl which encloses the organic remains is strongly charged with sulphuretted hydrogen. The same is the case with the fossilised fish of Oningen.

Hence, without positively asserting sulphuretted hydrogen to have been the poisonous agent, M. Blanchet concludes that there is a high degree of probability in favor of that supposition.

The surface of the globe is covered with a multitude of living beings, and it has been so arranged by the Creator that the elements of one generation should contribute to the formation of a new one. The earthy parts of animals, such as shells, bones, teeth, long resist this process, but the whole of the soft parts are repeatedly decomposed and disappear. Hence, if we find, as at Aix, a collection of carnivorous and other fish, coleopterous and dipterous insects, and that these remains are so preserved as to prove that their fleshy parts have not been disintegrated, we must conclude that the general equilibrium has been for once disturbed; and that the same cause which occasioned the destruction of one set of beings has also involved those of which they would otherwise have become the prey. Thus as an analogy to the occurrence at Marseilles—the presence of sulphuretted hydrogen in the rocks, and the

* *Comptes Rendus.*

position of the fishes—all tend to strengthen the supposition just indicated.

Professor Agassiz has pointed out another cause of the sudden death of fishes, viz. a rapid and considerable diminution in the temperature of the water. This fact has been observed in the Glatt, a small river near Zurich. It was probably an analogous phenomenon which occasioned the sudden death of the animals which have been found buried in ice in high northern latitudes.

ON THE USE OF ERGOT OF RYE IN PURPURA HÆMORRHAGICA AND SOME OTHER DISEASES.*

BY A. ROSS, M.D.

I HAVE read with much interest, in the last number of the *Lancet* (July 19), the case of purpura so carefully reported by Mr. Hogg. I am not, however, quite clear as to the share of merit due to the lead in effecting the cure; nay, it appears to me, that though one of the symptoms—viz., the petechiæ, began to disappear on the day on which the lead was discontinued, after three days' use, many of the most serious symptoms—the hæmorrhage from different parts of the mucous membrane, and the great constitutional disturbance, assumed a more aggravated degree after this period of the disease, and only yielded by slow steps to the various means so judiciously employed.

The report of the case extends from the 21st of April to the 25th of May; the lead was used from the 24th till the 27th of April; and on the 4th of May the prognosis was, as appears to me, still very unfavorable. I think, therefore, there is room to doubt the correctness of setting down the case as one successfully treated by lead.

Nothing can be further from my intention than to imply any doubt as to the propriety of the treatment, which must be allowed to have been most judicious, and to evince much readiness of resource in a case where the symptoms continued so urgent for so long a period. My only object is to avail myself of this opportunity of pointing out to the profession a remedy which I have found to be most energetic in a case similar to the one reported by Mr. Hogg, and which was doubtless a most obstinate one.

I was asked, in the spring of 1840, by Mr. Stutter, at that period the zealous house-surgeon of the Suffolk General Hospital, to see, as a matter of curiosity, a young man who had just been admitted under the care of my colleague, Dr. Jackson. I forget whether Dr. Jackson had then seen him or prescribed for him, but finding him laboring under purpura, with hæmorrhage from different organs, accompanied with such prostration as led us to fear that a fatal termination to the disease was not distant, I suggested, provided Dr. Jackson should approve of it, the trial of the ergot of rye, which it struck me might be usefully combined with compound infusion of roses. The use of this combination was immediately commenced, and continued for some days, in the dose, as far as I can remember, of five grains of the powered ergot, and an ounce of the compound infusion of roses, every four hours, with a generous diet. Under this treatment, with the addition of some aperient when necessary, the case went on with great rapidity to a favorable termination, without a single untoward symptom occurring to interrupt its progress.

The effects of the treatment were so decidedly beneficial in this case, apparently so unfavorable, that Mr. Stutter was anxious, I remember, that it should be communicated at the time to the *Lancet*. However, I considered then that it might be considered premature, and am only now induced to make the communication, from my attention having been recalled to the subject by Mr. Hogg's severe case.

Several years' experience of the effects of the ergot in certain stages of labor, leaves no doubt in my mind of the value of this medicine in obstretric practice.

I have also found it a valuable remedy, combined or not with infusion of roses, in menorrhagia, and in controlling sanguineous discharge from the uterus after abortion.

I have no doubt, if my idea of the value of the ergot as a remedy in purpura should be confirmed by further trial, that it might be found useful also in sea scurvy, and other passive hæmorrhages, depending on a peculiar and vitiated state of the blood.

* *Lancet.*

ON COPAIBA INJECTIONS IN GONORRHŒA.*

BY F. OATES, ESQ.

I AM desirous of recording the following case, as one of rare occurrence, and interesting, as affording a peculiar opportunity of studying the mode of action of the remedy employed for its cure. The patient was a man in the Venereal Hospital, at Paris, and the case fell under my observation while attending hospital practice in that city: it was

* *Med. Gaz.*, Aug. 22, 1845.

as follows:—When a lad, this patient, while playing with himself, had tied a piece of string round the middle of the penis, and so tightly, that from the sudden and great swelling induced, the urethra and corpus spongiosum were both nearly divided, and an opening into the canal was left at the point, through which the whole of the urine was voided in micturition from this time. The appearance of the penis, when not erect, was as if broken; the anterior portion hanging down almost at right angles with the posterior. The gonorrhœa was violent, with a profuse discharge from the meatus urinarius, as well as from the false opening or fistula, if one may so term it, showing that a great extent of the urethra was the seat of the inflammation. After a few days of palliative treatment, by diuretics and diluents, during which the discharge remained *in statu quo*, M. Ricord ordered for him the balsam of copaiba, under the use of which, the portion of the urethra posterior to the fistula, viz., so much of the canal as was traversed by the urine, got completely well, while from the anterior portion the discharge was as profuse as ever. The indication was here clear; the copaiba was continued, and a syringe given to the man, with which he was directed to inject his urine into the meatus urinarius after each micturition, and in a few days the cure was complete. This case shows very clearly the modus operandi of the balsam of copaiba in gonorrhœa to be, by impregnating the urine with its principles, and being so applied to the inflamed membrane, and not by being in any other way determined to the part. This is further shown in similar affections in the female, where the urethra is more implicated than the vagina; it also points out the use of injections of copaiba in gonorrhœa, and although they have been tried with partial success, it is by no means a general way of using the remedy; if, however, a formula could be found in which it could be so combined and modified as when eliminated with the urine, all the advantages of the medicine might probably be secured. There is no doubt the copaiba is the most powerful resource we possess in gonorrhœa, and where it does not succeed, it is generally from its nauseating the stomach to such an extent as to be inadmissible, or otherwise deranging the system; and could it be used in an appropriate form as an injection, the patient might be spared the disgust and unpleasantness consequent on a course of this nauseous drug.

TREATMENT OF SCALDS.*

Dr. Grosskopf speaks strongly in favor of the Ung. Plumbi Acetat. as a local application in cases of scalds, stating that if applied at once, it invariably prevents the formation of vesicles, speedily allays pain, and diminishes redness and swelling. In cases where vesicles have already formed and burst, its application quickly promotes the healing of the exposed surfaces. No evil effects on the system have followed its employment.

IMPROVEMENTS IN MARSH'S METHOD FOR THE CHEMICO-LEGAL RESEARCH FOR ARSENIC.†

BY M. BLONDLOT.

My improvements embrace two points:—1st, The disorganisation of animal matters containing the arsenic; 2nd, A modification of the apparatus, rendering it more convenient and certain in its use.

1st. I decompose the animal matter, tissues, &c., by means of concentrated sulphuric acid, as recommended by Messrs. Flandin and Danger, but instead of continuing the application of heat until a dry and friable charcoal is obtained, which may occasion the loss of some of the poison, I discontinue it when the mass has assumed a pasty consistence, and then add a quantity of water, which forms a thick black fluid, and I then pass a current of chlorine through it for a few minutes. The filtered, limpid liquid is then passed into the apparatus, and produces little or no frothing.

The advantages of this process are—1st, that not a particle of arsenic is lost; 2nd, the presence of sulphurous acid is precluded, being converted by the chlorine into sulphuric acid; and 3rd, the chlorine destroys or precipitates the small amount of organic matter which may remain in the solution.

2ndly. The design of the modification I propose in the apparatus is, to enable the operator to regulate or to suspend entirely at pleasure the disengagement of gas. For this purpose I employ a common Woolf's bottle, with three tubular openings. Into one of the lateral openings a tube is introduced, through which to pour the liquid under examination; to the other is attached the tube, the construction of which may vary according to the method adopted for the decomposition of the arsenuretted hydro-

* *Schmidt's Jahrbücher*, 1845.

† *Comptes Rendus*, July 7th, 1845, and *Lancet*.

gen. The third opening affords a passage to a glass rod, through a cork, which may be moved at pleasure, long enough to be managed easily, whilst slips of zinc are wound spirally round its lower end, and may be more or less immersed in the acid solution, so as to give the experimenter the command over the disengagement of the gas during the whole process. These, I think, are advantages not hitherto possessed by any other form of the apparatus.

POISONING BY OXALIC ACID.*

BY JAMES OGILVY, M.D.

On the 3rd of August last, I was called suddenly to visit Mrs. S——, a widow, aged 43, who was represented to be dangerously ill. I lost no time, and the place being near at hand, I was there without delay. I found the patient sitting in her bedroom, dressed, but quite dead. Mr. Bury, who was sent for at the same time, was also present. It appeared that about three hours before, she had ate a hearty dinner, and had been occupied afterwards, till a short timo before her death, in reading. Her sister was with her all the time, and also accompanied her to her bedroom, when she made no complaint. Her sister then left the bed-room for a few minutes, not exceeding four, and on her return found her faint and vomiting. She did not appear to suffer any pain, and, though at the last gasp, was quite collected.

On inquiry as to the cause of death, the friends, from the hints which she had occasionally dropped, suspected she had poisoned herself; but from there being no smell of prussic acid about her mouth, or among what she had vomited, I was more inclined, judging from previous symptoms, to suppose that a rupture of the heart, or some of the neighboring vessels, had occurred. A tumbler stood on the table, which, in the hurry, was used to hold some brandy and water, but her sister-in-law stated, that on taking it up at first it was quite clean and dry.

About a month before, I was consulted on her case by her brother, at whose house she was residing, and found her laboring under great depression of spirits, and most gloomy forebodings as to future prospects. Her husband had lately died in London, and though she was surrounded by kind friends, and in easy circumstances, still, to her imagination, want and poverty were always impending. Strange to say, however, her appetite was always good, even voraciously so. In addition to the above, she complained much of palpitation. The impulse of the heart was felt and heard over a considerable space, the pulse strong, full, and regular, leading me to suppose that hypertrophy of the left side of the heart existed. She experienced relief from the treatment adopted, but still without any abatement of her mental distress.

A coroner's inquest having been ordered, the body was examined next day, in presence of Mr. Bury, Mr. Tyerman, and myself. The body was emaciated; the lower part of the chest much contracted, evidently the effects of tight lacing. Considerable lividity was observed on the sides and back. The mouth and tongue had a bleached appearance, but not corroded. The brain was healthy, perhaps rather vascular. The heart was slightly enlarged, without perceptible thickening of the walls, but was more heavy than usual—a condition which agress with Dr. Clendinning's remark, that hypertrophy might be found to exist by having recourse to the balance, though it might be obvious to the eye. There was no rupture of the heart, or any vessel connected with it, and no disease of any of the valves. Both ventricles were full of black uncoagulated blood. On opening the abdomen, we found the liver much enlarged, but were most struck with the appearance of the stomach, which was in a state of considerable decomposition. The coats were soft and friable, indeed, so easily torn were they, that when ligatures were applied to each orifice, and the viscus removed, the weight of the contents produced laceration. The neighboring intestines, and the portion of the left lobe of the liver in contact with the stomach were similarly disorganized, as if the contents of that organ had transuded and affected them. The stomach was half full of a dark gelatinous-looking mass, the taste of which was intensely acid. On washing the stomach, the coats were found pale, the blood-vessels ramifying on the surface being dark, and filled with coagulated blood, thick, like extract. The cardiac orifice, and lower portion of the œsophagus, appeared as if boiled, the mucous coat being white, and easily detached.

On subjecting a clear solution of the contents of the stomach to analysis, we found—1. That it reddened litmus-paper. 2. A solution of nitrate of silver threw down a dense white precipitate, which was re-dissolved by the addition of nitric acid. 3. Lime-water threw down a white precipitate, which was also re-dissolved by nitric acid. 4. A solution of sulphate of copper produced a greenish-white precipitate.

Judging from the appearances of the stomach, and from the results obtained by the tests, no doubt remained on our minds

* *Lancet.*

that death was occasioned by swallowing a solution of oxalic acid, in which opinion the jury coincided. Some days after, our opinion as to the cause of death was confirmed, by the discovery, in the deceased's bed-room, of about an ounce and a half of oxalic acid loosely wrapped in a piece of newspaper, and which, from its crushed appearance, had probably been carried in her pocket for some time previously. On testing this, and comparing it with oxalic acid, they were found to agree in every respect.

The remarkable circumstance connected with the foregoing case, was the rapidly fatal nature of it. A large quantity of the poison, probably one or two ounces, must have been taken, which had speedily induced vomiting, prostration of strength, collapse, and death. There can be no doubt, from the surveillance which was kept over the patient, that the poison, even supposing it had been previously dissolved and kept ready, had been swallowed during a few minutes the sister was absent from the bedroom; consequently, it appears that death occurred within three minutes after the poison had entered the stomach. I have never before met with, or heard of, any well authenticated case of the same nature so rapidly fatal. Dr. Christison mentions one where death occurred about half an hour after two ounces of the acid had been taken, and another, which was considered remarkable, where only ten minutes elapsed.

TREATMENT OF BED-SORES IN FEVER.*

The most you can do is to apply cushions of curled hair in chamois leather under the hollows of the body, to equalise its weight on every part. Local applications to the mortified parts are worth nothing; but when they first become red, you might apply spirits, either alone or mixed with vinegar, and this will often stop the progress of the local affection. When the cuticle is abraded, lay a thin sheet of gold-beaters' leaf on the part: white of egg spread on linen will be found useful to prevent these abrasions. After the fever has subsided, and the patient is getting well, perhaps getting fat and walking about, still one of those gangrenous spots may show no disposition to heal. Should the part be the prominence of the sacrum, and the slough be attached to the tendinous aponeurosis covering it, don't be in a hurry to take your instruments to cut it off; if you do, tetanus will in all probability be the consequence.

* Dr. Colles's *Lectures on Surgery*.

ON THE USE OF A COMBINATION OF VALERIAN AND OXIDE OF ZINC IN DYSMENNORRHEA.*

BY JOHN ALDRIDGE, M. D.

Dysmennorrhea is usually divided, in systematic works, into the sanguineous and neuralgic varieties; the first occurring in robust strong women; the second in persons of delicate, nervous temperament. I cannot say that I have observed this distinction in nature; when painful menstruation occurs in young unmarried women, I have generally observed them to be strong, robust, and florid. Persons who have for many years suffered from this distressing affection to a very considerable degree, usually acquire that thin, rigid, anxious appearance, so common in persons who are in the habit of suffering pain; but it does not seem this difference is in any way connected with the causes of the disease.

But the name, dysmennorrhea, does not express any particular disease, but merely certain pains and disagreeable sensations accompanying the performance of the function of menstruation. Similar symptoms frequently accompany organic uterine diseases; and when the nature of the local lesion is recognised, the painful menstruation is properly considered merely symptomatic.

When dysmennorrhea occurs in the married, I believe it to be generally connected with uterine disease: the painful menstruation of virgins usually becomes very much alleviated after marriage—it does not follow, however, that the local cause of dysmenorrhea, when symptomatic, is of a serious nature. In the second number of the first volume of the *Dublin Hospital Gazette*, a very common cause of painful menstruation, has been shown by M. Gueneau de Mussy to be raspberry-like granulations on the neck of the uterus, a disease easily remediable, according to the same authority, by the direct application of nitrate of silver: and M. Chomel states that this cause of dysmenorrhea is occasionally to be found even amongst the unmarried. It would be desirable in every instance where these pains torment young women to ascertain, if possible, whether they depend on a local cause.

But I am inclined to think, that the majority of cases of dysmennorrhea occurring in the unmarried have no origin in any localised uterine disease; the pains in the head, back, under the left breast, along the margins of the hypochondria, along the

* From the *Dublin Hospital Gazette*, an excellent medical periodical, published on the 1st and 15th of each month.

course of the colon, down the front of the thighs, &c., which so commonly accompany a feeling of soreness and throbbing in the region of the uterus, are not confined to the period of menstruation, but are commonly more or less constant, although becoming exaggerated at the approach of the catemenial secretion; in different individuals the localities of these pains are observed to differ: thus, one will be principally subject to pain in the loins, another to pain in the hypogastrium, a third will have these pain accompanied by head-ache, a fourth, not subject to head-ache, will suffer from excruciating pain under the left breast, and so on. Moreover, I have known the remote pains peculiar to, or becoming exaggerated, at the menstrual period, unaccompanied by any local symptoms of uterine distress. The observation of Denman, that dysmennorrhea frequently can be traced to disappointed affection, I have had frequent opportunities of corroborating; and, finally, I have known men to suffer from neuralgic head-aches, pains in the region of the heart, in one or other hypochondria, &c., pains, which, in their character, and the treatment to which they are amenable, exactly simulated the remote neuralgias accompanying dysmennorrhea in the female. From these considerations I am led to infer, that in the majority of cases of painful menstruation in young unmarried women, the cause of the distressing symptoms is not located in the uterus.

There are many eminent physicians who suppose that the different pains alluded to depend on a peculiar affection of the spinal marrow, or of the origins of the posterior roots of the spinal nerves, to which they have given the name of spinal irritation: this affection can be recognized by the patient complaining of tenderness upon pressure being made over the place where the spinal nerves, distributed to the particular part, escape from the vertebral column, the pain in the part becoming at the same time exaggerated. Instead of looking on this tender condition of the origin of the spinal nerves as being the cause of these pains, I am inclined to regard it as an extension of the disease from the periphery to the centre; for, in cases of short duration, no matter how severe the pains, I have seldom been enabled to detect the spinal tenderness, although employing the usual means recommended, the application of hot towels, &c.; and, again, although cupping or leeching over the spine has frequently alleviated the local pain, the same amount of relief was experienced from the abstraction of blood from the immediate seat of the neuralgia. I may in this place be permitted to protest against the too frequent practice of cupping or leeching for these neuralgias, a practice attended by very transient relief, and leaving cicatrices, which may in after-life prove very detrimental to the individual.

I have been thus led to regard those cases of dysmennorrhea which do not arise from local uterine disease, as consisting in neuralgias of the peripheral extremities of the afferent nerves, which become exaggerated during periods of unwonted excitement, such as menstruation; and which, like other peripheral neuralgias, have a tendency to produce engorgements in their sites, and to advance towards the central nervous unions.

I believe it to be a law in pathology, that the remote disorders usually symptomatic of a local disease, are, when they occur idiopathically, capable of giving origin to the local disease: thus it has been well shown, that the constitutional irritation arising from a local cause, is apt to increase and prolong the topical disease: and in connection with uterine diseases, there is little doubt, but that persistent amennorrhea, is calculated to produce chlorosis, just as chlorosis will produce amennorrhea; that chlorosis will in other instances, give rise to uterine leucorrhea, in the same way that uterine leucorrhea will produce chlorosis. I think I have observed, in accordance with this law, that, as dysmennorrhea is frequently symptomatic of organic uterine disease, so uterine disease is most apt to occur in persons long subject to dysmennorrhea: at least, I am certain, that in most of the cases of disease of the uterus which I have examined, the patients have told me that in early life they were subject to painful menstruation.

The diagnosis of these neuralgic pains, I have not found, in general, very difficult; they are distinguishable from rheumatismal affections by the absence of tenderness, and from inflammations by their great severity, the absence of fever, and their chronicity. That pains of this nature, often lead to the suspicions of hepatitis, pleurisy, &c., I have no doubt from the multitude of leech and lancet cicatrices to be sometimes found on the breasts and sides of women; and indeed I have known old maids who have spent their lives taking blue pills, for imaginary liver complaints, but which were no other than a form of the affection under consideration. However, this disease frequently stimulates phthisis, as well as chronic cerebral disease, in a manner exceedingly difficult to distinguish. In the following case this disease resembled incipient phthisis.

Case I.—Catherine Fitzpatrick, aged 17, fat and ruddy looking; menses regular, as to time and quantity, but preceeded and

accompanied by severe pain in loins, and throbbing sensation in the hypogastric region: is subject to a pain in the left breast, and between shoulders; and there is some tenderness, on pressure, either side of dorsal spine. The pains become much worse at menstrual periods. Has cough, but no physical signs. Pulse 60, and strong. Has thirst, anorexia, and epigastric tenderness.

The total absence of anything like phthisical predisposition would at once point out the true nature of the foregoing case. In the following case, the diagnosis from cerebral disease, becomes very difficult, although I am inclined to consider it an extreme form of the neuralgic diathesis.

Case II.—Bridget Dolland, aged 30, housekeeper, teetotaller. Menses usually last a week and are rather profuse; regular as to time: always preceded and accompanied by much pelvic and lumbar pain: no leucorrhea. Had a fit apparently epileptic when a child; since then has always been subject to pain in head, noise in ears, and palpitations, is very nervous and easily frightened: apt to get cramps in the legs. Short tickling cough without expectoration, no physical signs. Appetite middling, some thirst, tongue rough, slight epigastric tenderness, constipation, no bruit de diable; nor is she subject to hysterical paroxysms.

The following case is remarkable for the supervention of the remote head symptoms, after the cessation of local uterine symptoms.

Case III.—Mary Ryan, aged 22, dressmaker, not a teetotaller; had dysmenorrhea about five years ago which was relieved by treatment; the menstrual discharge was always natural in quantity. During the last three years has been subject to severe pain at the anterior fontinelle, and pain in the right breast; these pains are always much worse at menstrual periods; subject to vertigo, muscæ volitantes, sometimes flashes of light before eyes; head hot to the hand, and temples beating during the cephalalgia; her mind sometimes wanders when the pain is most severe; memory not as good as formerly; sight weak, and eyes apt to water; the cephalalgia commences by pains shooting up neck. Some cough and wheezing; occasional palpitations. Anorexia, but no other digestive symptoms.

The train of symptoms which present themselves in this affection, may be complicated with chlorosis, menstrual suppression, hysteria, mimosis acutus, &c. Of these hysteria appears to be most frequent, as in the following case:—

Case IV.—Anne ———, aged 21, living with her family: first became subject to dysmennorrhea after a disappointment in love; the pains are confined to the loins and pelvis: menstruation regular as to time, but always profuse. Is subject to hysterical paroxysms of crying, without any sufficient cause, globus hystericus, &c. Liable to palpitation, especially at night; pain between shoulders of an aching character; slight dyspnœa on exertion; apt to have fearful dreams at night; feet swell a little towards evening; there is a slight roughness in the first sound of heart below nipple. No cough. Appetite good; thirst; red tongue, constipation, slight tenderness in epigastrium.

In the following case the hysterical complication sometimes simulated epilepsy.

Case V.—Ellen Robinson, aged 23, dressmaker, not a teetotaller. Menses regular but profuse; preceded and accompanied by severe pain in loins, throbbing in hypogastrium. Is subject to cephalalgia at vertex and over orbits; and pain along the margins of false ribs, on right side; these pains are always much worse at menstrual periods: the headache is usually accompanied by bright sparks before the eyes; she thinks her sight is failing; memory is not so good as it used to be; is subject to noise in the ears. She has sometimes fallen to the ground in a kind of fit, preceded by a sensation of jerks in the head; in these fits she did not lose her senses, and although she threw her limbs about, did not seem to be convulsed; the fits generally terminated by marked hysterical paroxysms of crying. No cough. No digestive symptoms. Her appearance fat a ruddy.

The menstrual function does not seem to be necessarily interfered with, in this affection: the catamenia is, however, sometimes increased in quantity, as in most of the cases related: more usually, however, it is diminished, as in the following case:—

Case VI.—Mary Kennan, aged 23, servant, not a teetotaller. Catamenia regular as to time, but very scanty: accompanied by pains in the bowels, and down thighs; also under the left breast, and above orbits. Lips and cheeks red, no bruit de diable. Tongue clean, some thirst, bowels confined, nausea. Slight cough, no expectoration, no physical signs. Sleepless, subject to frightful dreams, feet sometimes swell.

The next case simulated in the violence of the head symptoms, those already described connected with mennorrhagia, proving that these symptoms did not depend on the profuse menstruation.

Case VII.—Mary Doyle, aged 20, dressmaker, of a ruddy complexion and strong appearance. Catamenia which formerly lasted eight days, has during the last year seldom exceeded two days, and is very scanty in quantity. Is subject to severe

dysmennorrhœa, pain in left breast, pain in top of head; pain in left ear; throbbing in temples and occiput; head becomes very hot to the hand.

The following is a more severe example of the same nature:—

Case VIII.—Margaret Natton, aged 23, dress-maker, was a teetotaller but not at present, unmarried. Scanty menstruation, dysmennorrhœa, cephalalgia at vertex, pain in left side; these pains become worse at menstrual periods: singing in ears; eyes weak and watery; temples throb, and head burning; sparks before eyes, vertigo, &c., accompany the cephalalgia. Is subject to palpitations at night; and to rigors commencing during sleep, but which awake her. Tongue foul, anorexia, thirst, constipation, no epigastric tenderness. No cough.

The foregoing case was evidently complicated with mimosis acutus. The following case also exhibits an example of this complication.

Case IX.—Marianne Comyn, aged 20, waistcoat-maker, not a teetotaller. Scanty menstruation, dysmennorrhœa, superorbital pain, pain and palpitation of heart, pain in loins and right side; worse at menstrual periods. Of a dusky, greasy appearance, anorexia, thirst, habitual constipation, broad flabby indented tongue. Leucorrhœa. Tenderness in dorsal vertebræ.

The existence of leucorrhœa in this case is interesting to remark: this was not present in any of the preceding cases. Now, as I believe granulations of the neck of the uterus are always accompanied by an opali leucorrhœa, the absence of this catarrh is presumptive evidence that the first eight cases were not connected with that disease. I was precluded in every case from making a vaginal examination by the patients being all unmarried.

The foregoing cases are sufficient to show the nature and importance of this disease. It certainly may continue for many years, without seriously affecting the functions; and I have before stated, it usually becomes very much alleviated, if not removed after marriage; but it is not the less capable of unfitting the individual from fulfilling the necessary avocations of life in a proper manner, of preventing her from enjoying any amusement, and frequently of rendering existence almost insupportable; it thus becomes an object of great interest to the physician, the discovery of some remedy, calculated to alleviate or remove its very distressing symptoms.

The great number of cases of this disease constantly presenting themselves before me, early directed my attention to its treatment. As the majority of persons afflicted with it were otherwise strong and robust, and as I originally supposed it to be symptomatic of uterine disease, I thought it probably depended on a condition of local plethora, it might be of inflammatory origin; but although cupping the loins and sacrum might for a short time diminish the local phenomena, they had no effect on the remote neuralgias; and even the pains in the loins and hypogastrium returned with equal violence on the approach of the next menstrual period. Each of the neuralgias was similarly influenced; cupping the nape, or leeches up the nostrils, might for a time alleviate the cephalalgia, but the relief was very transient. The different contra-irritants were found to be equally inefficacious; mercury in alternative, or in large doses, and pushed as far as salivation; iodide of potassium, colchicum, turpentine, were all tried, and without benefit. Tonics in various forms were exhibited without effect; quina in large doses commenced a few days before the expected menstrual period; the different preparations of the iron, &c., all seemed rather to exaggerate the dysmennorrhœa. Drastic purgatives did not appear to have any influence in removing these pains, although persisted in as long as they safely could. Antispasmodics of various kinds, assafœtida, castor, and valerian, were given in large doses, but with the most unsatisfactory results. I did not neglect testing the efficacy of narcotics, but opium I have generally found to make the patient much worse; belladonna, whose virtues have been very much vaunted in this disease, appeared very whimsical in its action, whether exhibited internally or used as a liniment to the spine; sometimes seeming to relieve the local pains like magic, at others to be without influence on them, and sometimes to exaggerate them; and all these effects have occurred at different times in the same individual; and although the lumbar and pelvic pains may have been relieved, the remote cephalic and pectoral neuralgias remained unabated. Aconite, which from its physiological action, one would be disposed to imagine peculiarly fitted for relieving pain, seemed on the contrary to aggravate the distress. Veratria, in one instance, I believe to have completely cured a patient, applied in the form of ointment to the seat of the neuralgia (under the left breast), but I never again could obtain from its employment the same results. Electricity, whether in a continuous current, or in a succession of rapid shocks, by means of a Smee's battery, was without effect. In fine, until lately, I knew of nothing constantly beneficial, excepting the employment of the warm-bath, at the time the pains commenced to become exaggerated.

DEATH FROM ACONITE.*

A MELANCHOLY event, which has recently occurred to a respected member of our profession, forcibly sets forth the danger, not only of those who incautiously undertake the management of severe disease existing in their own persons, but also of the venturing, without sufficient care, on the administration of powerful medicinal agents, with the operation of which we are but imperfectly acquainted. Dr. Male, of Birmingham, recently fell a sacrifice to these practices. He had been reading a work in which was recommended a new and powerful agent (aconite), for the removal of deep-seated neuralgic pains, and having been suffering of late from an affection of that kind, which had resisted the ordinary means for its removal, he was induced to try upon his own person the powers of the remedial agent recommended. Not sufficiently mindful of his age, Dr. Male took the tincture of aconite in doses, the accumulation of which produced an alarming depression of the nervous system, from which he was ultimately unable to rally, and thus fell a victim to that want of a due appreciation of the circumstances of his own case, so common, we may add, amongst medical men when treating themselves, combined with the incautious use of a powerful drug, with the operation of which he was but imperfectly acquainted.

This unfortunate case should prove a warning to every medical practitioner, as well in the pursuit of his professional avocations, as in inducing him, when himself suffering under serious illness, to have recourse to the advice of some brother practitioner.

THE MODUS OPERANDI OF NITRE IN THORACIC INFLAMMATION.†

IF the congestion of the lungs is not recent, if effusion have taken place with morbid changes in the blood, in the capillaries, and in the organs themselves, bleeding is of secondary value, and other remedies must be adopted. Nitre and tartar emetic are the most generally and successfully adopted in Germany. The results of experiments show that the action of nitre on blood out of the body is to prevent its coagulation, to diminish the tendency of the blood-vesicles to unite, and to contract the membrane of the latter. Its chemical relations to fibrin, as a solvent, have been already stated. When taken into the stomach, it is absorbed into the circulation, and excites both the capillaries and blood-vesicles to contract. It hinders the tendency of the fibrin to coagulate, and by rendering the effused plasma more soluble, promotes its absorption. It also renders the absorption of oxygen into the blood more active, and so facilitates the decomposition of the pseudo-plasma and its excretion by the kidneys and skin, in the form of urate of ammonia, &c.

* *Prov. Med. Jour.*, Aug. 20.

† *British and Foreign Review.*

SEPARATION OF LIME FROM MAGNESIA.*

BY M. BACK.

ONE of the methods recommended for the separation of these two substances is, to heat to redness the mixture of the chlorides of calcium and magnesium: the chloride of calcium remains unchanged, and may be separated by water from the caustic magnesia, which is set free during the process. M. Back has lately examined this method, Berzelius having already stated that it was incorrect. In one experiment, 0·221 grammes of fresh calcined magnesia were dissolved in muriatic acid, and during evaporation, 2 grammes of dry chloride of calcium were added, the surplus acid driven off, and the whole mass afterwards incinnerated in a platina crucible. When cold, the dry residue was moistened with water, and again heated. By a repetition of this operation, muriatic acid continually escaped, and, finally, the mass was digested in water, and a residue obtained which weighed 0·401 grammes (indicating about 0·18 grammes of magnesia more than could possibly be present). This method of separating the two alkalies gives, therefore, incorrect results.

[REMARKS.—In employing this process, we have found it exceedingly difficult to drive off all the chlorine from the magnesia, so that a portion has been lost by water dissolving some of the chloride of magnesium with the chloride of calcium. The latter becomes strongly alkaline during calcination, and remains so on deliquescence; it, therefore, probably loses part of its chlorine. Mitscherlich long since remarked, that chloride of magnesium, obtained by evaporating and heating a mixture of the muriates of ammonia and magnesia, would resist a melting heat; while the salt obtained by digesting pure magnesia in muriatic acid and evaporating, was decomposed by heat, muriatic acid passing off, and caustic magnesia being left as a residue.—ED. *Med. Gaz.*]

* Heller's *Archiv. für Physiol. und Pathol. Chemie*, 1845.

DISULPHATE OF QUININE.*

The following test for the adulteration of disulphate of quinine is recommended by Dr. Nevins, of Liverpool, on account of its delicacy and simplicity:—To one or two grains of the suspected salt, add three or four drops of sulphuric acid, in a white evaporating dish, and twice as many drops of water. If the salt contains either fatty matter or starch, these will remain: whilst, if they are absent the whole will be dissolved. Let heat be next applied to the solution, and as it becomes concentrated, the acid will char any sugar which may be present, which will be indicated by a black stain round the edge of the solution, and the whole will speedily assume the same color. By this means, one or two per cent. of organic matter may be recognised in as many minutes without trouble. It is said that salicine is an occasional adulteration, and, if present, it is detected by the same proceeding. If a very minute quantity is only mixed with the quinine, blood-red points, and ultimately a general reddish color, are produced as the concentration proceeds. It is a good plan to observe the effect of the sulphuric acid before the addition of water, as it acts upon the salicine cold, producing the blood-red color. Thus the single test of sulphuric acid, diluted and heated, detects nearly every impurity which is found in the disulphate; for starch, fatty matters, and insoluble earthy salts, are left undissolved before the application of heat; and salicine, gum, and sugar, are shown by the effects of heat on the solution. Pure disulphate of quinine is not affected by sulphuric acid applied as above.

ABORTION CONSEQUENT ON THE USE OF MERCURY.†

Dr. Salomon relates two cases in which premature delivery resulted apparently from the employment of mercury. The first was that of a healthy married woman, who, during her first pregnancy, had been treated with the internal and external use of mercury, on account of some syphilitic affection, and who was in the sixth month delivered of an emaciated child. The placenta was very small and shrivelled, weighing about two ounces; the cord was very narrow, but of considerable length. On account of profuse hæmorrhage, the attendance of Dr. Salomon was required. The woman remained weak and poorly for a considerable time, but she entirely recovered, and about three years subsequently, was delivered of a fine healthy child.

In the second case, a healthy farmer's wife, mother of four children, and who was again pregnant, had brought on a profuse salivation by rubbing in for a considerable time the Ung. Hyd. Nitr. Oxyd. as a cure for the itch, which she had contracted. About the end of the fifth month of pregnancy she was delivered of a fœtus in a state of incipient putrefaction. Very profuse hæmorrhage attended the confinement, and the woman was greatly exhausted. The salivation defied all treatment, and it was more than ten weeks before it disappeared.

ON THE TRUE COMPOSITION OF LABARAQUE'S SOLUTION OF CHLORIDE OF SODA.*

BY B. KAVANAGH, ESQ.

The solution of chloride of soda, or the liquor sodæ chlorinatæ, of the London Pharmacopœia, prepared by passing a stream of chlorine gas through a solution of carbonate of soda, is a preparation whose theoretical composition has hitherto been involved in doubt.

MM. Millon and Balard have conceived, and put forward different hypotheses regarding it. The latter chemist considered it to be composed of bicarbonate of soda, hypochlorite of soda, and chloride of sodium. Thus:—suppose four atoms of carbonate of soda to be acted on by two atoms of chlorine. Two atoms of the carbonate of soda lose their carbonic acid, which uniting with the other two atoms of carbonate of soda produce two atoms of bicarbonate of soda. Then one of the remaining two atoms of soda loses its oxygen, and there remains sodium, with which one atom of the chlorine unites, to form chloride of sodium, whilst the disengaged oxygen unites with the other atom of chlorine to form hypochlorous acid, and this uniting with the remaining atom of soda forms hypochlorite of soda. This view of the subject, however elegant in conception, and strengthened by his discovery of the existence of hypochlorous acid (composed of one atom of chlorine and one of oxygen,) and indeed the theory generally adopted at the present day, will be seen to be totally untenable.

M. Millon considers it to be composed of bicarbonate of soda, and oxychloride of sodium. Thus: one atom of chlorine, and two atoms of carbonate of soda, react on

* *Lancet.*

† *Casper's Wochenschrift*, June 21, 1845.

* *Dublin Hospital Gazette*, Aug. 15, 1845.

each other. One atom of carbonate of soda loses its carbonic acid, which uniting with the other atom of carbonate of soda forms one atom of bicarbonate of soda, whilst the atom of chlorine acting on the remaining atom of soda, form chloride of soda or oxy-chloride of sodium, according to Dulong's theory of the salts. Namely, that the oxygen or electro-negative element of the base, joins with the electro-negative element added, which uniting with the metal of the base, form a compound analogous to an haloïd salt, except that the electro-negative element is compound. This oxy-chloride of sodium he considers analagous to the peroxide of sodium in which one atom of oxygen is replaced by an atom of chlorine.

By means of some researches which I have recently made, and which I shall now submit, I have proved, I trust, that this latter theory is a correct one.

That the chlorinated solution contains a carbonate is evident by its giving a white turbidity on the addition of lime water. That this is the bicarbonate may be known by its giving no precipitate with solution of sulphate of magnesia. Now, if to the chlorinated solution we add a solution of alum (the double sulphate of alumina and potash) the alumina is instantly precipitated, *without effervescence*, proving that the chlorinated solution contained a salt of soda other than the bicarbonate, for it must be by it the alumina is replaced, accompanied, on the application of heat, by the evolution of a chlorinous gas. Whether this soda salt is a hypochlorite of soda, or a chloride of soda (oxy-chloride of sodium) involves the point at which MM. Millon and Balard are at issue. The question of course can be solved by ascertaining the nature of the chlorinous gas evolved, whether hypochlorous acid or pure chlorine. With this view I placed at the end of the tube from which the gas was allowed to issue a coil of iron wire in order to determine whether the gas was hypochlorous acid, as in such case an insoluble oxide of iron would be formed, but instead of this the soluble chloride was produced, known to be such by its giving with solution of nitrate of silver the usual curdy white precipitate, soluble in strong solution of ammonia but insoluble in nitric acid. I then placed a coil of silver wire in the end of the tube in a similar manner. After the action went on for some time, a white curdy mass was formed, soluble in strong solution of ammonia, but insoluble in nitric acid—chloride of silver.

The united results of both these experiments prove the gas eliminated to be pure chlorine, so that it settles the question in favor of Millon's theory,—that it is a compound of one atom of bicarbonate of soda in combination with one atom of oxy-chloride of sodium in solution.

IV. REVIEWS AND NOTICES OF BOOKS, &c.

THE PHARMACEUTICAL LATIN GRAMMAR; BEING AN EASY INTRODUCTION TO MEDICAL LATIN, THE LONDON PHARMACOPŒIA, AND THE PERUSAL OF PHYSICIANS' PRESCRIPTIONS. By ARNOLD JAMES COOLEY. London: R. Groombridge and Sons, Paternoster-row. 1845.

A WORK better adapted for its purpose, and more wanted, we have seldom, if ever, met with.

We cannot give a better notion of its plan and value than by quoting the preface:—

"This little book was written some time ago, at the earnest solicitation of a friend of the author's, whose education had been neglected in his youth, and who was extremely desirous of instructing himself in the rudiments of Medical Latin. The benefit which this individual derived from it, induced the author to show the manuscript to several persons among his connections to whom he thought it was likely to prove useful, as well as to others more qualified to judge of its value as an elementary work. In every case it has met with the approval of the parties who have seen it, and in numerous instances copies have been solicited. At length, at the particular recommendation of several friends, the author has consented to print it in a cheap form for general circulation, and he trusts that the printed copy will prove as useful as the manuscript.

"It is hoped that the present work will be found to supply a deficiency which has hitherto existed in the department to which it belongs; viz. a SHORT and SELF-EXPLANATORY introduction to Latin Grammar and construing, adapted more immediately to the wants of dispensing Chemists and Druggists, and others connected with the preparation or administration of medicine. For this purpose it has been illustrated with copious

examples and exercises taken from the London Pharmacopœia, followed by others of purer Latinity from the Roman classics. An appendix has been added, fully exemplifying the construction of Physicians' prescriptions, together with numerous examples of medical formulæ, a *very copious* list of abbreviations, and comparative tables of the weights and measures employed in commerce and medicine.

"Though more especially addressed to persons employed in the departments above mentioned, it is confidently believed that the present work will be found one of the most easy and concise, yet comprehensive introdutions to the Latin tongue yet published, suitable to all classes, and particularly adapted to adult and self-tuition."

We conclude this very brief notice with a recommendation to every chemist, who is not perfect in his Latin, to procure Mr. Cooley's book at once, and set about improving himself.

THE CHEMISTRY OF VEGETABLE AND ANIMAL PHYSIOLOGY. By Dr. G. J. Mulder, Professor of Chemistry in the University of Utrecht. Translated from the Dutch by Dr. Fromberg, First Assistant in the Laboratory of the Agricultural Chemistry Association of Scotland. With an Introduction and Notes, by James F. W. Johnston, F.R.SS., L. and E. Part II. Edinburgh and London: Blackwood and Sons.

Our notice of the first part of this work applies to the second. We, therefore, content ourselves with giving a synopsis of its contents, at the same time paying ourselves with one or two extracts.

This part forms the sixth chapter of the entire work, and is devoted to "General Organic substances," which chapter is divided into twelve sections, which treat severally of the Cellular substance of Plants; the Amylaceous substances; Dextrin and Gum; Sugar; Mucilage; Pectin and Pectic Acid; Extractive matters; Fats; Chlorophyle; Protein; Fibrin; Gelatinous substances—Gelatine and Chondrin.

We transcribe the following:—

"GENERAL ORGANIC SUBSTANCES.

"Among the numerous organic bodies, there are some of a complicated character, the composition of which cannot be reduced to the simple form, in which the inorganic binary combinations present themselves.

"These so-called complex substances form a peculiar group. They are in a high degree changeable under very different conditions, easily re-act on substances externally applied to them, readily produce chemical changes, and are, at the same time, readily altered themselves. They are mutual combinations of certain other compounds, which, though they may be combined together into a state of equilibrium, can easily be removed from that state, in consequence of their complex combination.

"It is of such complex bodies that the principal component parts of the organic kingdom consist—bodies well adapted to induce incessant changes of substance, without affecting the form of the whole, and to develop and perpetuate the chief characteristics of vital action.

"From these complex substances, others, less complicated, are readily produced, either with or without the influence of an organic whole; and, inversely, from these less complicated, more complex substances may also be produced as circumstances or conditions vary.

"If we desire to draw a line of distinction in this respect between the two organic kingdoms, we may affirm that the animal organism has a tendency to produce combinations with simple, or, at least, well known, radicals; the organism of plants, on the contrary, to produce complex substances. The former results, art may more easily hope to accomplish than the latter; thus, art is more able to imitate the products of the animal body, than those of the plant.

"We have already stated that, the peculiar properties of the two organic kingdoms ought to be ascribed, first, to the special character of the four elements, carbon, hydrogen, nitrogen, and oxygen—to which may be added, sulphur, phosphorus, iron, and iodine—that special character in virtue of which they are capable of forming an endless series of combinations with each other; and, second, to the circumstances in which these elements are placed in the organic kingdom. As we have sufficiently treated of these two subjects in the first chapter, we shall only remark here, with regard to the latter point, that carbonate of ammonia—which salt is isomorphous with carbonate of potash—contains in itself all the four organic elements—the elements of innumerable combinations, that are alternately produced and destroyed in the organic kingdom. This salt, which is called an inorganic body, though it is composed of the four organic elements, contains within itself both the materials and the forces, from which all that is peculiar in the organic kingdom takes its rise. It shows us, in the clearest manner, where we must look for their special character, and where not.

"The fundamental substances—the building materials, as it were—of the organic kingdom are few in number. Nature is simple in her means, sublime in her aim, abundant, yea, one should almost say, prodigal, in the diffusion of her riches. Out of a small number of elements, in which she has deposited, as it were, infinite modifications of the same primary force, she creates a few compound bodies, which may be considered as the foundations of the whole organic kingdom. She makes these act upon each other, and thus causes the production of a variety, which the natural philosopher admires the more, the deeper he penetrates into this inexhaustible treasury.

"These fundamental substances, therefore, ought, above all others, to be known to us. They are of four kinds, namely, those which are common both to plants and to animals—those which exist in one of them only—those which are present in neither, being, nevertheless, materials of the organic kingdom—and, finally, the so-called inorganic substances which occur in a state of combination with the organic substances.

"The third division contains carbonic acid, ammonia, water, atmospheric air, humin and humic acid, ulmin and ulmic acid, geïc acid, crenic acid, and apocrenic acid, of which, altogether, the organic part of our arable soils is constituted. The fourth division contains salts of magnesia, lime, soda, potash, manganese, and iron, with phosphoric acid, sulphuric acid, iodine, fluorine, and chlorine, and also silica and alumina—altogether produced from decayed rocks. All these substances, however, belong to the kingdom of organic nature, since, whatever is indispensable to it, ought to be considered as organic. They belong to the necessary principles of the organic kingdom, which cannot even be conceived to exist without them.

"The first division contains proteïn; the second, for plants, dextrine; for animals, gelatine. From dextrine are produced cellular fibre, starch, gum, sugar, and probably also bassorine; from the starch are produced fats and chlorophyle. All these substances are of general importance to the chemical physiology of plants. We shall treat briefly of each of them. As to the animal substances, we shall confine ourselves, in the present chapter, to a view of the proteïn compounds, the gelatines, and the coloring matter of the blood.

"Upon these few compounds are laid the foundations of the animal and vegetable kingdoms. Their number may be even further reduced, for most of them are produced from each other. Together, they are the source of all the organic substances found in plants and animals.

"Having treated, in the former chapters, of the atmosphere, of water, and of the component parts of the soil, in connexion with organic nature, we refer, for the properties of the substances contained in the last division, to elementary works upon general chemistry. Here we intend to treat, and that briefly, of the substances of the first and second division, and especially of their origin. The great variety of substances produced from them in plants and animals will find a proper place when we come to consider the functions through which they are produced."

No chemical library will be complete wtihout this valuable work.

TO CORRESPONDENTS.

"A MEMBER OF THE PHARMACEUTICAL SOCIETY" is recommended to read, for enlightenment as to our opinion of that Society, our articles respecting it and its mismanagers, written in 1843. After a careful perusal of them, we think that he will be confirmed in in the conviction he has already arrived at.

"A TYRO."—The process you have adopted is correct.

We have received a prospectus from the Association for Promoting Cleanliness among the Poor—a highly laudable object.

Answers to some correspondents who have requested private letters, are delayed for a few days, in consequence of the total occupation of our time by various important affairs.

"H. O." (Southampton).—The subject to which our correspondent alludes cannot be treated of in the pages of THE CHEMIST.

"E. O."—*The Practical Manual of Photography*, published by Mackenzie, Fleet-street, *has* been received.

"C. H."—Let us have the article in the *early* part of the month, and if approved, shall be inserted in our next.

Several correspondents are answered by post.

NOTICE.—*All Communications and Books for Review must be addressed* "To the Publisher of THE CHEMIST, 310, Strand, London." *Communications must be prepaid, and sent before the* 15*th of each month; Books for Review before the* 10*th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE COMPOSITION OF OXALATE AND NITRATE OF UREA.*

BY R. F. MARCHAND.

In a series of investigations in which I had to operate the quantitative determination of urea, I preferred weighing the compound of this body with nitric or oxalic acid to the determination of pure urea. The indication furnished by M. Regnault, that oxalate of urea is formed of $C^2N^2H^4O^2+C^2O^3+HO$ (N=175; H=12·5), and that the nitrate is $C^2N^2H^4O^2+NO^5+HO$, with 1 equivalent of water, consequently, is contrary to the data of Prout and Berzelius. I have endeavored to verify it by the immediate determination of the oxalic acid and the nitric acid.

I. OXALATE OF UREA.

Pure urea, dissolved in water, was precipitated by an excess of oxalic acid, and the salt was submitted to a new crystallisation. The combination, dried for a long time, at 230°F, was dissolved in water and decomposed by chloride of calcium. The oxalate of lime was converted into sulphate of lime, and the weight was determined.

0·735 grammes of oxalate of urea gave 0·465 of sulphate of lime, answering to 0·2583 grammes, or 35·1 per cent of oxalic acid.

If the combination contains 1 equivalent of water, as asserted by M. Regnault, it is formed of—

Oxalic acid	450	3428
Urea	750	5714
Water	112·5	8·58
	1312·5	100·00

The anhydrous combination would contain 37·5 per cent. of oxalic acid. Berzelius says, that in his analysis of oxalate of urea he found 37·4 of oxalic acid (*Traité de Chimie*, IX., p. 443). This fact induced me to ascertain whether it was possible that at a higher temperature than 230°F., this atom of water might be separated without decomposing the combination.

0·9065 grammes of crystallised salt, dried in the air, were heated to 248°F.; the crystals became opaque and turbid; at the same time they lost 0·173 grammes=14·6 per cent. Heated above this degree, the salt was decomposed before the commencement of fusion. The salt dried at 248°F. had the composition indicated above; it contained 34·5 per cent. of oxalic acid. 0·338 grammes of another portion lost, at 239°F., 0·045 grammes=13·3 per cent. The crystallised salt contains, therefore, 3 atoms of water; it abandons 2 atoms (14·6 per cent.) at 248°F; but it retains the third atom until decomposed.

II. NITRATE OF UREA.

Prout examined the composition of nitrate of urea, and found it to be anhydrous, since he found it to contain 47·37 per cent. of nitric acid (*Annals of Philosophy*. By Dr. F. Thomson. vol. xi., p. 354). By what method did Prout determine the nitric acid? This is not pointed out in the memoir quoted, because it is an extract of a more extended memoir, contained in the eighth volume of the *Medico-Chirurgical Transactions*, which I have not been able to obtain. At all events, I am surprised that Prout should have been mistaken to the extent of more than three per cent. in the determination of the nitric acid; for, according to the composition found by M. Regnault, the combination contains only 43·9 per cent. of NO^5. According to the formula indicated above, the combination would be formed of:—

Nitric acid	675	43·90
Urea	750	48·78
Water	112·5	7·32
	1537·5	100·00

I have endeavored to determine the composition by decomposition, by means of carbonate of baryta, and precipitation of the nitrate of baryta, by sulphuric acid.

I first employed nitrate of urea which had been precipitated from its solution by the addition of nitric acid. This salt was separated from the liquor, expressed with force, several

* *Journal für Praktische Chemie.*

times, between sheets of blotting-paper, and finally dried at from 230° to 248°F.

0·6035 grammes of this salt gave 0·7885 of sulphate of baryta, corresponding to 0·3611 grammes of nitric acid, or 60·66 per cent.

According to this result this compound is composed of 2 equivalents of nitric acid, 1 equivalent of urea, and 1 equivalent of water; it would be formed of:—

2 eqs. of nitric acid ..	135·0	61·09
1 ,, of urea .... } 1 ,, of water .... }	86·5	38·91
	212·5	100·00

It is impossible that M. Regnault could have analysed this combination, since he obtained much more carbon and hydrogen, and much less nitrogen than this composition requires. It would give:—

C^2 =	150·0 ..	6·77
H^5 =	62·5 ..	2·83
N^4 =	700·0 ..	31·65
O^{13} =	1300·0 ..	58·75
	2212·5	100·00

It is scarcely possible for the additional equivalent of nitric acid contained here more than in M. Regnault's salt, to be only accessory; I have nevertheless examined, in the same manner, the combination submitted to several crystallisations.

The result was the same, since, in two experiments I found 61·2 and 60·92 per cent. of nitric acid.

A certain quantity of the same substance was heated to 284°F., and 0·770 grammes were decomposed in the manner indicated; they furnished 1·090 grammes of sulphate of baryta, corresponding to 0·50613 grammes of nitric acid, or 65·72 per cent. Thus the combination had probably become anhydrous; for it would then contain 64·3 per cent. of nitric acid.

Finally, I examined the crystals of nitrate of urea deposited, by cooling, from an acid solution, and which were submitted to a new crystallisation.

0·275 grammes of the combination gave 0·366 grammes of sulphate of baryta, corresponding to 61·79 per cent. of NO^5.

Not succeeding in this manner in obtaining the neutral nitrate of urea, I dissolved the acid combination in water and added urea to it. The crystallised combination was dried at 230°F.; it then lost 0·5 per cent. The dried substance contained 55 per cent. of nitric acid; for 0·934 grammes gave 1·165 grammes of sulphate of baryta. There was, therefore, in this salt a combination of 3 atoms of nitric acid with 2 atoms of urea and 1 atom of water; this combination would furnish 55·67 per cent. of nitric acid. After having again added a small quantity of urea to the liquor which had given the crystals of the latter salt, and after having crystallised it, I obtained a combination which did not lose weight at 230°F., and which, after having been heated for a long time at that temperature, contained 44·1 per cent. of nitric acid; for 0·6685 grammes gave 0·635 of sulphate of baryta.

It is very possible that Prout examined a mixture of these combinations. It is right to remark that the combination, separated by an analysis, contained, not 48·7 per cent. of urea, but only 33·89 per cent.

ON CHEMICAL CHANGE PRODUCED BY THE SOLAR RAYS.*

BY ROBERT HUNT.

After drawing attention to the very extensive and curious discoveries, which had been made during the past years, in the chemistry of solar radiations; the author proceeded to show, that these effects were not confined to the ordinary photographic preparations, but that they extended over a large series of the chemical compounds. In the first place he drew attention to the accelerating influence of the sun's rays on precipitation. It was found that the sulphate of iron being dissolved in water containing a little carbonic acid, precipitated a considerable portion of the carbonate of iron, when it was exposed to sunshine; but that *in the same* time a similar solution in the dark gave no evidences of precipitation. An effect similar to this had been observed, by Sir John Herschel, on a solution of lime and platinum, from which an insoluble platinate of lime was formed in the sunshine, but not any in the dark. A mixture of the iodide of potassium and the ferro-prussiate of potash, being exposed to good sunshine for a few hours, a decomposition was effected, and a considerable quantity of prussian blue was precipitated; in the dark, the same mixture underwent no such changes. Several other similar cases were named, which Mr. Hunt intends still further to investigate.

The effect of light on the color of precipitates was next mentioned. It has long been observed by the French manufacturers of carmine, that that very beautiful preparation was procured of a much richer color, if was prepared in the bright sunshine, than if pre-

* Annual Report of the Cornwall Polytechnic Society.

pared in the dark, or on a gloomy night. Mr. Hunt had discovered that the chromates of silver and mercury as well as prussian blue, varied very much in color, if they had been exposed during their preparation to the influence of the sun's rays or otherwise.

A still more remarkable effect had been observed by the author, which appeared to indicate some absorption of the principle upon which these changes depend. It is well known to chemists, that a solution of sulphate of iron will precipitate gold and silver in a metallic state from their solutions. In the dark this process occupies many hours—and even in diffused daylight a considerable time. If, however, either of the solutions—either the iron, or the silver or gold—are exposed to solar influence for some time, they undergo a change by which the precipitation of the metal is immediately effected, *even in the dark*. It is not necessary that both solutions should be exposed; the iron solution alone is quite sufficient. May we not then suppose that the principle producing these curious chemical changes, is absorbed by the fluid during the period of its first exposure?

A small galvanic arrangement having been made by placing a tube, closed at one end, with a skin diaphram, in a large test-glass; a solution of the iodide of potassium was poured into the tube, and a solution of nitrate of silver was put into the outer glass —the two fluids being connected with a piece of platinum wire. An arrangement of this kind being placed in the dark, in about four hours a very beautiful crystallisation of metallic silver was found to have taken place about the platinum wire in the silver solution. Another arrangement, in every respect prepared in the same manner, was exposed to sunshine. Not only did no metallic crystallisation take place whilst exposed, but the exposure had so changed the character of the solution, that no such crystallisation would afterwards take place, even in darkness. This experiment was many times repeated, with various modifications, but always with the same result. Here we have a very striking instance of the interference of the solar radiations, with the exercise of the force of galvanic electricity. It is not improbable, but these may be made measurers of each other; knowing the force of the current generated between the two solutions, it will not be difficult to ascertain the quantity of the solar rays necessary to retard or stop the action of that force.

Another experiment was as follows:—A solution of the bichromate of potash, and a solution of the sulphate of copper being mixed together, were exposed to the sunshine, and such light as might prevail during the month of November. A similar solution which had not been so exposed, was evaporated, and crystals obtained of a rather curious character. They were sulphates of copper and potash, and salts of chrominm, which had not been previously described. On evaporating the solution, which had been exposed to the sunshine, it was found that the characters of the crystals were completely changed, as far as chemical combination was concerned. The relative proportions of the constitutents being in each case very materially changed. Similar results had been obtained with solutions of the bichromate of potash, and the chlorides of gold and of mercury. The investigation is not, however, yet complete. Mr. Hunt exhibited several specimens of the salts prepared from both the exposed and the unexposed solutions.

INVESTIGATIONS CONCERNING THE CONSTITUTION OF THE ACIDS OF PHOSPHORUS.*

BY M. A. WURTZ.

I HAVE undertaken a series of investigations concerning the constitution of the acids of phosphorus. The most general result of my work is, that these acids are all derived from the same type, represented in its most simple form by anhydrous phosphoric acid, or by perchloride of phosphorus. Phosphorous acid would be phosphoric acid, in which 1 equivalent of oxygen would be substituted by hydrogen, and in hypophosphorous acid, 2 atoms of hydrogen take the place of 2 atoms of oxygen.

This result is not a purely theoretical speculation; it necessarily flows from the experiments which I have undertaken. They have for their object the analysis of the hypophosphites and phosphites, and the study of some organic combinations into which I have been able to introduce phosphorous acid.

I will, therefore, divide this memoir into three parts. In the first, which I have now the honor of presenting to the Academy, I will communicate the result of my investigations concerning the hypophosphites. The second will be devoted to the study of phosphorous acid and the phosphites; finally, in the third, I shall describe the products of the reaction of protochloride of phosphorous acid on the alcohols; that is to say, etherophosphorous acid, amylophorous acid, and amylophorous ether.

This work has occupied me an entire year

* *Comptes Rendus*, July 14, 1845.

in M. Dumas' private laboratory. It is to the support and kind instructions of that illustrious philosopher that I owe the undertaking and completion of my researches. I gladly seize this opportunity of publicly offering him my grateful acknowledgment.

HYPOPHOSPHOROUS ACID.

In a former work I communicated the analysis of some hypophosphites. I ascertained that these salts were very intimately combined with the elements of 2 equivalents of water, and I was led to regard this water as entering into the composition of phosphorous acid itself. This theoretical view having been rejected by Berzelius and H. Rose, I resumed my experiments, and I now complete my first work by presenting the analyses of a greater number of hypophosphites.

I prepared almost all these salts by the double decomposition of the soluble sulphates and hypophosphates of baryta.

I communicate the results of my analyses in the following table :—

Names of the salts analysed.	Formula of the dried salt.	Water of crystallisation.	Remarks.
Hypophosphite of potassa.....	$PH^2 O^3$, PO	....	These two salts are anhydrous.
— of ammonia	$PH^2 O^3$, H^4 NO..	....	
— of baryta			
— crystallised in needles ..	$PH^2 O^3$, BaO....	+HO	At 212°F. this salt loses HO.
— crystallised in tables	$PH^2 O^3$, BaO		
— of strontia	$PH^2 O^3$, SrO....	....	These two salts are anhydrous.
— of lime	$PH^2 O^3$, CaO....	....	
— of magnesia			
— crystallised (H. Rose) ..	$PH^2 O^3$, MgO ..	+Ho+5 Aq.	
— dried at 212°F.....	$PH^2 O^3$, MgO ..	+HO	
— dried at 366°F.....	$PH^2 O^3$, MgO		
— of manganese.....	$PH^2 O^3$, MnO ..	+HO	
— dried at 302°F.....	$PH^2 O^3$, MnO		
— of zinc (rhombohedral)...	$PH^2 O^3$, ZnO ..	+HO	
— of zinc (octohedral)	$PH^2 O^3$, ZnO ..	+6 HO	At 212°F. these four salts lose 4 equivalents of water.
— of iron	$PH^2 O^3$, FeO....	+6 HO	
— of cobalt.....	$PH^2 O^3$, CoO....	+6 HO	
— of nickel	$PH^2 O^3$, NiO....	+6 HO	
— of chrome	2 $PH^2 O^3$, $Cr^2 O^3$	+4 HO	
— dried at 392°F.....	2 $PH^2 O^3$, $Cr^2 O^3$		
— of copper	$PH^2 O^3$, CuO ..	....	These two salts are anhydrous.
— of lead	$PH^2 O^3$, PbO ..	....	

These analyses confirm, as is evident, my hypothesis concerning the constitution of hypophosphorous acid.

I will terminate this part of my memoir with some theoretical considerations.

I have endeavoured to demonstrate that hypophosphorous acid is a compound of phosphuret of hydrogen and oxygen, and that its constitution is expressed by the formula $PH^2 O^3$. This hypothesis has been the subject of some critical observations on the part of Berzelius and H. Rose.

If now, after having made numerous experiments with the view of verifying the point of view which I at first enunciated, I make, in my turn, some observations on the objections which have been made to me, and if I arrive at conclusions opposed to those of such eminent chemists, I yield only to a profound conviction, and to a sincere desire of arriving at truth.

Berzelius regards my hypothesis, concerning the constitution of hypophosphorous acid, as incorrect, because it abolishes the analogy which exists, according to his own experiments, between the oxygenous and the sulphurous compounds of phosphorus.

The compound PO is, according to him, a natural and necessary term of the series of the oxidation of phosphorous, and finds its analogy in the hypophosphuret of sulphur.

It appears to me that this argument goes beyond the facts. No one has ever yet isolated the body PO; no one can demonstrate that it exists in the hypophosphites. It is at present a purely hypothetical combination.

I will go further. When the combination PO shall be isolated, the analogy of hypophosphuret of sulphur will be found, but it will remain to be demonstrated that this body really enters into the composition of

the hypophosphites; for it is only by absorbing water that the body PO can be converted into hypophosphorous acid, properly so called, and form with bases the hypophosphites, as they are known to us. Now, the fixation of a certain quantity of water in the elements of this anhydrous acid may profoundly modify its constitution. The time no longer exists when the study of the water in the combinations was regarded as accessory, and the researches of modern chemistry have demonstrated all the importance of the part which it might play.

Berzelius adds that to account for the energy with which two equivalents of water are retained by the hypophosphites, it is sufficient to bear in mind that the ordinary phosphate of soda loses its last equivalent of water only at a very high temperature, and that in general phosphorus and its compounds have a great tendency to combine with three equivalents of a more electro-positive body.

I may be allowed to observe that if one equivalent of water is retained with energy, in the ordinary phosphate of soda, it is because it acts in it the part of a base. Now, it is impossible to form a similar supposition with regard to the hypophosphites. I think I have demonstrated, in my first memoir, that hypophosphorous acid is now a basic acid, and that the two equivalents of water met with in the hypophosphites, not being capable of being replaced by another base, cannot be regarded as basic water.

H. Rose published, some time ago, a memoir on the hypophosphites, in which he disputed the correctness of some facts which I had advanced.

I had insisted on the facility with which the hypophosphites are converted into phosphites in various circumstances, and especially under the influence of bases. The most feeble oxides, and subacetate of lead itself, operate this transformation with a disengagement of hydrogen. Rose admitted that, in these circumstances, the hypophosphites were converted into phosphates, and he maintains his conclusions in this respect.

However, in repeating his former experiments, he discovered two distinct periods in the reaction.

In the first, phosphite of potassa was formed, and in the second, the salt itself was converted into phosphate; he adds that this conversion of phosphite into phosphate succeeds only when a very concentrated alkaline solution is employed, and by evaporating to dryness. In operating on hypophosphite of lime, Rose obtained phosphite, but he admits that it was impossible to change all the phosphite into phosphate.

It appeared to me impossible to conclude, from these facts, that the hypophosphites are converted into phosphates under the influence of bases; I think that it is more correct to say that they are converted into phosphites, and if, in certain circumstances, the phosphite formed be decomposed, it is only owing to a secondary reaction, which is no longer operated on the hypophosphite.

I stated in my first memoir that the salts of copper were reduced by hypophosphorous acid, with a disengagement of hydrogen; this fact appeared to me of a nature to confirm my hypothesis relative to the constitution of this acid, and I took great care in proving it. I was surprised at Rose absolutely denying it.

By varying a little my first experiments, I discovered hydroguret of copper, and I proved that, by mixing equal equivalents of hypophosphorous acid and sulphate of copper, and by heating the liquid beyond the point at which hydroguret of copper is decomposed, torrents of hydrogen gas were disengaged. The hypophosphorous acid arising from the decomposition of 1 gramme 551 of hypophosphite of baryta was mixed with a solution of 1 gramme 351 of crystallised sulphate of copper, and the liquid was immediately heated to 212° F. In a few moments, I collected 77 cc. 5 of hydrogen gas at the temperature of 52° F., and under the pressure of 0 m. 745. This quantity of gas does not perfectly correspond to 1 equivalent. But, as there is formed in this reaction a little protoxide of copper, which remains in solution in the liquid without reacting on the hypophosphorous acid, it results that an excess of this acid should remain in the solution. I convinced myself of this by adding a little sulphate of copper to the decolored liquor; it became turbid, and I collected 8 cubic centimetres of hydrogen gas.

It is evident that this reaction is very clear when we operate on the proportions which I have just indicated; it must necessarily be modified when we employ an excess of sulphate of copper, as H. Rose did. In these conditions, the nascent hydrogen, instead of being disengaged, acts on an excess of oxide of copper, and we obtain double as much oxide of copper as is formed in the foregoing experiment.

The hypophosphite of copper is not the only one which is reduced by the disengagement of hydrogen; this property also exists in the hypophosphite of nickel. According to the theory which persists in admitting that hypophosphorous acid is an oxide of phosphorous PO, this disengagement of hydrogen could be explained only by raising a supposition of very little probability. It would be necessary to admit that this acid could de-

compose water at the temperature of 158° F. in a very acid liquor, and in presence of an oxide which, in being again reduced, furnishes oxygen.

I stop in this discussion: it is not my object here to reproduce the arguments which I used in my first memoir. I must confine myself to presenting some obervations on the facts.

I will, however, call to mind that I admitted in hypophosphorous acid the existence of a peculiar radical PH^2, combined with 3 atoms of oxygen. This radical has since been discovered by M. Paul Thénard; it is the compound corresponding to amidogen in the series of phosphates. Like cacodyle, it possesses so powerful an affinity for oxygen, that it inflames in the air, and then gives rise to products in decomposition. It is possible that, by presenting oxygen to it in a slow and gradual manner, the reaction may be moderated, and hypophosphorous acid formed.

However, until experiment enables us to form a decision on this subject, we should admit only with reserve the existence of this radical in hypophosphorous acid. For our own part, confining ourselves strictly to facts, we will say that this acid, as it exists in its saline combinations, is a ternary compound, PH^2O^2. This mode of viewing its composition carries along with it no hypothesis; it is only the natural expression of facts. That which is hypothetical is the existence of the body PO, which not only has never been isolated, but which has not even been discovered in any of its combinations.

Dulong, struck with the remarkable properties of hypophosphorous acid, and with the very peculiar properties of its salts, had already foreseen a theory which I have just developed. I am happy to be able to rest my opinion on so competent an authority. The following is what that illustrious philosopher says:—"In all the preceding, I have reasoned according to the hypothesis that hypophosphorous acid is a binary combination; but there are, however, very strong reasons for thinking that it is a triple combination of oxygen, hydrogen, and phosphorous, forming a new kind of hydracid. If this view of its nature were to become prevalent, we should be obliged to call it by hydroxyphosphic acid."

MEMOIR ON THE DECOMPOSITION OF WATER BY METALS, IN PRESENCE OF ACIDS AND SALTS.*

BY M. E. MILLON.

(*Concluded from p.* 404.)

IRON AND TIN.

Iron and tin were submitted to tests analogous to those of zinc.

Sulphuric acid diluted twelve times, very rapidly dissolves iron turnings; a few drops of bichloride of platinum communicates to this action an extreme intensity. Arsenious acid, on the contrary, entirely arrests the action of sulphuric acid on iron; this influence is sufficient to protect the metallic iron for a month, and, doubtless, for a still longer time in sulphuric acid diluted twelve times, which would dissolve it very rapidly. When the iron is well cleaned, a few drops of an aqueous solution of arsenious acid are sufficient for preserving it from the acid; it preserves, at the same time, a metallic lustre, whose tint only becomes deepened. Emetic retards the action of sulphuric acid on iron, but it does not arrest it. Bichloride of mercury acts in the same way as emetic. Sulphate of copper powerfully accelerates the disengagement of hydrogen; sulphate of silver also accelerates it, but in a less degree.

Hydrochloric acid receives, from small quantities of metallic solutions, the same influence as diluted sulphuric acid; these reactions may also be observed with very concentrated hydrochloric acid. It is sufficient to dilute the pure and fuming acid with two or three volumes of water. In some cases the addition of a small quantity of sulphate of copper, has very evidently suspended the action of hydrochloric acid. The iron is covered with the metallic copper, and remains several hours without furnishing hydrogen; but it has not always been thus, without its being possible to recognise whether this difference was owing to the hydrochloric acid not to the iron.

Diluted radical acetic acid, or the commercial acid arising from the distillation of wood, acts very slowly on iron. Bichloride of platinum renders the disengagement more sensible; the other metallic solutions seem indifferent. Arsenious acid arrests all reaction as in the two foregoing acids.

Tartaric and racemic acids are confounded in all the influences which they undergo. A more abundant disengagement with bichloride of platinum; arrest of the reaction with arsenious acid; almost complete

* *Comptes Rendus.*

indifference on the part of other metallic solutions.

When oxalic acid has received a few drops of bichloride of platinum, the iron is covered, as in the foregoing cases, with a black layer of platinum; but, instead of dissolving more rapidly, the iron is preserved exactly as it would have been with arsenious acid, which does not cease to exert its preserving influence here.

Binoxalate and quadroxalate of potassa, containing a small quantity of bichloride of platinum, act like oxalic acid with respect to iron. This exception, presented by oxalic acid and its compounds, is the only one which is met with in the influence of bichloride of platinum on iron and on all other metals.

The saline solutions and distilled water may produce hydrogen with iron, when a small quantity of bichloride of platinum intervenes; but these actions are slow, and much more easily observed with iron than with zinc.

Nitric acid receives a very curious influence from the presence of bichloride of platinum, when it acts on metallic iron. Indeed, when nitric acid, with 4½ equivalents of water, diluted with two or three times its volume of water, is poured on iron turnings, the metal is dissolved, disengaging abundant nitrous vapors, and producing a persalt of iron. But, if we add to the same acid, similarly diluted, one drop of bichloride of platinum, the iron, in dissolving, no longer produces nitrous vapors. It is hydrogen which is disengaged. At the same time a protonitrate of iron and nitrate of ammonia are formed. Thus, the small quantities of metallic solutions possess not only the power of moderating, accelerating, or annihilating chemical actions, but also of changing the nature of the products which result from them.

Tin.—It is with hydrochloric acid that tin presents the clearest and most striking phenomenon, as this reaction gives rise to the production of protochloride of tin, of which there is a very great consumption: the influence of small quantities of metallic solutions, finds here an immediate and extremely simple application.

In the experiments which will now be detailed, the tin, which was that of commerce, was granulated or laminated.

The acid employed was sometimes that of commerce, and sometimes pure and fuming hydrochloric acid diluted with one volume of water.

Bichloride of platinum and emetic have the most marked influence on the solution of tin. Arsenious acid and the other metallic solutions do not possess any appreciable influence.

Laminated tin was put in contact with pure and fuming hydrochloric acid, diluted with its own bulk of water, at the temperature of 70°F. The tin was weighed before and after the reaction.

First Experiment.—Pure hydrochloric acid, without addition: tin, before the experiment, 19 gr. 672; loss, 0 gr. 562.

Second Experiment.—Hydrochloric acid, containing 15 drops of a saturated solution of emetic: tin, before the experiment, 19 gr. 841; loss, 6 gr. 296.

Third Experiment.—Hydrochloric acid, containing 15 drops of a solution of bichloride of platinum: tin, before the experiment, 18 gr. 974; loss, 7 gr. 495.

In expressing the action of pure hydrochloric acid on tin, by 1, the action of the same acid, containing a small quantity of emetic may be expressed by 11, and with the addition of bichloride of platinum by 13.

In operating at the temperature of boiling water, the difference between platinum and antimony was in favor of the latter; and it was found that emetic, at that temperature, rendered the action of hydrochloric acid five times as rapid, whilst bichloride of platinum only tripled it.

The influence of small quantities of metallic solutions is still more powerful when granulated tin is employed, and when the hydrochloric acid is poured on it.

100 grammes of granulated tin were dissolved in 20 minutes by 500 grammes of commercial hydrochloric acid, heated to 212° F., and containing 40 drops of a saturated solution of emetic. The same acid, in the same circumstances, but without any addition, and poured on 100 grammes of granulated tin, dissolved only 90 grammes in the space of three hours.

Thus, with the addition of a few millionths of antimony, hydrochloric acid dissolved a greater quantity of tin in one-ninth part of the time.

When the action of the boiling acid is compared with that of the cold, but without the addition of the emetic, it is found that the latter acts almost as rapidly; so that, in the manufacture of protochloride of tin, heat may doubtless be substituted by emetic or any other salt of antimony, or else by means of the same addition, the operation may be rendered eight or ten times more rapid. When we reflect that the use of glass vessels is necessary for the preparation of protochloride of tin, it will be understood how desirable it is for the manufacturer to avoid the employment of heat and to abridge the duration of the operation.

The phenomena presented by more or less dilute sulphuric acid, in its action on tin,

present no fact which appears to me worthy of interest. I have also abandoned the study of the organic acids, which, in all circumstances, act only very slowly or not at all.

LEAD, COPPER, AND ANTIMONY.

It was interesting to ascertain how far this influence of the small quantities of the metallic solutions would extend to affecting the decomposition of water by certain metals which in ordinary circumstances do not disengage hydrogen in presence of the most energetic acids. I submitted to this examination, lead, copper, antimony, bismuth, silver, and mercury.

I speedily ascertained that granulated lead disengaged hydrogen very abundantly when concentrated hydrochloric acid was poured on it. The addition of bichloride of platinum communicated great energy to the reaction, and provoked a disengagement of hydrogen even with dilute hydrochloric acid, which would not otherwise act.

Bismuth, silver, and mercury, do not decompose water in any case; but copper without heat, and antimony with heat, furnish hydrogen in contact with hydrochloric acid, containing a small quantity of bichloride of platinum. With copper this decomposition is very brisk. The hydrochloric acid may be diluted with one or two volumes of water without hydrogen ceasing to be produced. By heating a little, the gas is produced as rapidly as with zinc and dilute sulphuric acid. Granulated copper suffices, in all cases, for producing the reaction.

Antimony also gives hydrogen with hydrochloric acid, containing a minute quantity of bichloride of platinum; but only a very small quantity of gas can be obtained, and it is necessary to maintain the acid at the boiling point.

Thus, two metals, copper and platinum, act, by their concurrence, in cases in which they are, seperately, inert. Both are incapable of decomposing water under the influence of the acids; but it is sufficient to add, the most indifferent of the two, in appearance, for this decomposition to be set up.

The study of the chemical effects, due to small quantities of metallic solutions, has already enabled me several times to prove that two feeble insensible affinities determine energetic effects, when they are united.*

Nitric acid, mixed with nitrous acid, is increased in its oxidising properties; iodic acid burns alimentary substances as soon as the combustible power of iodine comes to

* The addition of a small quantity of bichloride of platinum to nitric acid arrests the oxidising action which the latter exerts on copper. Pure nitric acid, diluted with three or four volumes of water, may, owing to this addition, be kept for some time in contact with granulated copper. But the smallest quantity of nitrite is sufficient for determining a prompt reaction. I have resumed, on this subject, the examination of the reaction which takes place between copper and weak nitric acid, very free from nitrous acid. I attributed (*Annales de Chemie et de Physique*, 3rd series, tome vi., p. 73) to nitrous acid the property of inducing the conversion of copper into nitrate, and of propagating it by an incessant reproduction of nitrous acid. Gay-Lussac seemed to think that it was not thus, and that nitrous acid acted in nitric acid as it would do, for example, when added to sulphuric acid, and oxidised only in proportion to its quantity, without any propagation.

I weighed, in this re-action, the quantity of nitrite of potassa, which I added to nitric acid, and by likewise weighing the sheets of copper attacked by the nitric acid thus rendered nitrous, I found that the copper might lose in weight 10 or 15 times more than it would have done, if the initial action of the nitrous acid were not renewed at the expense of the nitric acid. I always took care to arrange a comparative experiment, in which the same nitric acid, free from nitrous acid, remained in contact with a similar quantity of laminated copper. The minute quantity of copper dissolved, in the latter case, was subtracted from that dissolved in the former, and I attributed the difference to the propagating reaction of the nitrous acid.

I think, then, that there is some probability in favor of the conclusions at which I have arrived with respect to the influence of nitrous acid in the oxidising influence of nitric acid. I will say as much of aqua regia, notwithstanding the interesting discovery of M. Baudrimont's compound. I have ascertained, by very various experiments, that this liquid, which is otherwise very curious, does not correspond to NO^5, but to NO^4. It is almost entirely converted in contact with water, and without any disengagement, into NO^4 and HCl, and consequently, into nitric, nitrous, and hydrochloric acids. So that in aqua regia, as well as in the aqueous solution of M. Baudrimont's compound, the metals are found in presence of several acids, among which is nitrous acid. To apply to this case the idea which I now develop, I will say that aqua regia presents the union of several parallel chemical forces, namely, two oxidising acids, NO^5 and NO^3, and a third acid, for dissolving the product of the oxidation.

its assistance; copper and platinum, by a conjoint action, decomposed waters. Is it not necessary to investigate in bodies, in addition to the chemical affinity, which acts by direct effects, a very small secondary affinity, which, to effect a sensible reaction, requires the assistance of another affinity, in the same direction? We can perceive all the interest which would attach to such an observation, and to the discovery of this parallelism of affinity, since it would adduce from the chemical principles put in reaction results which we are far from expecting, at least, we may say, for the present, that it would not be sufficient to arouse two antagonist terms in chemical actions. We must often reckon, in a chemical phenomenon, with 3 terms, and, perhaps, with a still greater number. I do not speak here of physical circumstances; it is evident that none must be neglected.

ON THE COMPOSITION AND PROPERTIES OF THE COLORING MATTER OF THE BILE.*

BY PROFESSOR SCHERER.

This chemist extracted the coloring matter of the bile from the urine of a patient labouring under icterus. To obtain it, the recent urine was filtered in order to separate the mucus and the deposit of uric acid, which already began to be formed, and it was afterwards mixed with chloride of barium. The light-green precipitate obtained was washed with water, thrown on a filter, and decomposed by digestion at a gentle heat, with alcohol and hydrochloric acid; the alcholic solution obtained was evaporated, the product of the evaporation was washed with water on a filter, redissolved in a mixture of alcohol and ether, and again evaporated.

The coloring matter, washed with distilled water, in order to free it from hydrochloric acid, was in the form of a very fine dark-green powder, almost insoluble in water, very soluble in proof-spirit and alcohol, and rather less soluble in pure ether. By adding to water a small quantity of caustic or carbonated alkali, it dissolved abundantly, and communicated to the liquor a color varying from brown to yellow; it is likewise more soluble in water containing alkaline salts than in pure distilled water. Put for a long time in digestion, with an excess of hydrochloric acid, it gradually changed its color from green to a blackish brown; it was the same when it was exposed for a long time to a gentle heat with alkalis. In both cases it loses its great solubility in alcohol, and becomes on the contrary rather more soluble in water; it loses at the same time the property of undergoing, by the action of nitric acid, the known characteristic changes of color.

The coloring matter of the bile, turned green by hydrochloric acid, no longer presented this change of color; but it is reproduced with great beauty, when it is redissolved in an alkaline liquor. The author ascertained that the coloring matter of bile, well purified, does not contain any trace of hydrochloric, sulphuric, and phosphoric acids.

Analysis gave it the following composition:—

Carbon	68·192
Hydrogen	7·473
Nitrogen	7·074
Oxygen	17·261
	100·000

It loses much *carbon and hydrogen* in its prolonged treatment by the acids and alkalis in contact with the air.

The coloring matter of biliary calculi, previously washed with hydrocholoric acid, furnished—

Carbon	62·421
Hydrogen	6·148
Nitrogen	8·169
Oxygen	23·192
	100·000

It results from this that the coloring matters contained in the black biliary calculi, which are very friable and poor in cholesterine, very much resembles in its composition that which is formed in a prolonged treatment of the ordinary coloring matter of the bile by acids or alkalis in contact with the air. Perhaps, besides this coloring matter, submitted to analysis, and forming the greater part of biliary calculis there exists also another probably richer in carbon and hydrogen, more nearly resembling the normal coloring matter; but the smallness of the quantity of the substance which the author had at his disposal prevented him from solving this question.

ON VALERIANIC ACID AND ON THE MODE OF EXTRACTING IT FROM THE VALERIAN ROOT.

BY PROFESSOR G. RIGHINI.

In a memoir read at Milan, before the Chemical Section of the Sixth Italian Scientific Congress, Prof. Righini showed that

* *Annalen der Chemie und Pharmacie.* N .S., Vol. III.

valerianic acid does not exist in the free state in valerian root; but that a temperature of 230° F. is required for its formation. which temperature is necessary for the development of the volatile oil, from which, as is known, pure valerianic acid may be extracted. Professor Righini added, moreover, that he is certain that the proximate products of which the valerian root is formed, conjointly with the elevated temperature at which the volatile oil is developed, react on a portion of the latter, determining its decomposition, and that from this, valerianic acid results. In support of this assertion, the author has made known the following means by which he is enabled to procure the acid in question:—

Ten killogrammes of the root of wild valerian, collected in the month of October or November, are taken, and after being washed with water and cut into very small pieces, are bruised in a marble mortar, gradually adding a sufficient quantity of water to extract all the juice, with the aid of pressure. The juice obtained is placed in a small basin and heated to ebullition, then carbonate of lime in very fine powder, or lime water, is added; valerianate of lime is precipitated, and, after two or three hours' repose, water, acidulated with hydrochloric acid, is poured on this precipitate. The latter, in combining with the lime, gives rise to chloride of calcium, and liberates the valerianic acid. The whole is subjected to a gentle heat; and, after the evaporation has been continued almost until a pellicle is formed, the liquid is introduced into a retort, and distilled in a sand-bath, to extract the valerianic acid, whilst the chloride of calcium remains as a residue in the retort.

The distillation should be continued to dryness.

The quantity of root above-mentioned yielded, by this process, 160 grammes of very pure valerianic acid.

The author has also discovered another method of obtaining the same acid economically and in considerable quantity, which he will shortly publish.

NEW AND ECONOMICAL PROCESS FOR PREPARING OXIDE OF CARBON.*

BY M. FILHOL.

In the memoir which he published a short time ago on lactic acid, M. Pelouze noticed the following reaction as being very curious:—When we make a mixture of lactic acid and concentrated sulphuric acid, the latter acid being in great excess, we obtain, by gently heating the vessel which contains it, an abundant disengagement of pure oxide of carbon.

In reflecting on the relation of composition which exists between sugar and lactic acid, I was led to think that it was not impossible that a similar action might be established if sugar was substituted for lactic acid in the foregoing mixture.

The formula of crystallised cane sugar is $C^{24}\ H^{22}\ O^{11}$; that of hydrated lactic acid is represented by

$$\frac{C^{24}\ H^{24}\ O^{12}}{2};$$

thus, therefore, if the elements of 1 equivalent of water be subtracted from the elements of 2 equivalents of lactic acid, we have the formula of crystallised cane sugar.

If we admit, with M. Juérin Vary, that the formula of anhydrous sugar of starch is $C^{24}\ H^{24}\ O^{12}$, it is evident that 2 atoms of lactic acid would just contain the elements of 1 atom of anhydrous sugar of fecula.

I heated in a matrass 20 grammes of cane-sugar with 80 grammes of concentrated sulphuric acid; the mixture being maintained at a very gentle heat, a very brisk reaction was manifested, a very abundant disengagement of gas was produced, the mass swelled up, turned black, and became almost solid; I then stopped the operation. I had collected about 2 quarts of gas.

This gas was formed, as was proved by the analysis which I made, of a mixture of carbonic acid and oxide of carbon. I ascertained that it contained about one-fifth of its volume of carbonic acid. I should say, moreover, that the composition of the mixture varies in each operation.

Deprived, by washing with lime water, of all its carbonic acid, this gas is colorless; it does not redden litmus; it is decomposed at a red heat by potassium, which absorbs its oxygen and produces a positive carbon; it burns with a pure blue flame. I proved by eudiometrical analysis that it consisted solely of oxide of carbon. Sugar of starch and fecula furnished analogous results, as it is evident, the reaction of sulphuric acid on sugar or starch differs from that which the same acid exerts on lactic acid, inasmuch as, with the latter acid, the oxide of carbon is obtained without the mixture of carbonic acid, but the separation of the latter is so easy that it will always be more economical to employ sugar.

This process, which is much more economical than all those which have ordinarily been used, is so easy of execution that I have reason to hope that it will be adopted in all laboratories for the preparation of oxide of carbon.

* *Journal de Pharmacie*, August, 1845.

EXPERIMENTS ESTABLISHING THE EFFICACY OF THE DAVY LAMP IN MIXTURES OF INFLAMMABLE VAPORS, ARISING FROM EXTREMELY VOLATILE LIQUIDS; EMPLOYMENT OF THE LIGHT PRODUCED BY THE GALVANIC PILE IN AN EXPLOSIVE ATMOSPHERE.*

A LETTER FROM M. BOUSSINGAULT TO M. ARAGO.

THE 16th of last June, an explosion of gas took place in one of the galleries of the Magdalen mine, at the distance of 108 metres from the shafts, and where there could be no suspicion of the presence of gas. In addition to this, it was the first serious accident that had happened at these works. Since the unfortunate event prudence has required the employment of the Davy-lamp, the use of which had been hitherto limited to the lighting of several parts of the works supposed to be dangerous.

Before placing these lamps in the hands of all the miners, I proposed to prove the whole of them, and it is of these experiments I wish to inform you, since, it is possible, the observations I made may not be without their utility.

The wire gauze that surrounded the wick in these Davy-lamps contained 144 meshes in a square centimetre. All these lamps plunged into, and kept in, different mixtures of air and hydrogen gas, completely stood the test.

These essays, satisfactory in themselves, were scarcely concluded before a scruple arose in my mind. I asked myself whether these lamps that answered so well in air mixed with hydrogen, would be equally secure if they were placed in mixtures of air and inflammable vapors, arising from extremely volatile liquids. The scruple arose in the possible presence of vapors of naptha in the atmosphere of mines of petrolium. My first experiments were made upon air in contact with one of the most volatile and combustible liquids—sulphuric ether. This is how I operate: I take a cylindrical vessel of tinned plate, 35 centimetres in depth, and 11 centimetres in diameter; on the side of this cylinder, two centimetres above the botton, a very short tube is fixed, one centimetre in diameter; this tube allows the entrance of air. I pour the volatile liquid into the cylinder until the bottom is covered to the height of one centimetre; things being thus arranged, and the surrounding temperature at 22°C., I ascertained that the base of the flame of a common lamp, when let down into the cylinder, set fire to the mixture of vapor and air when it arrived at the distance of 25 centimetres above the surface of the liquid; an explosion then took place.

If under the same circumstances you introduce into the cylinder the same lamp, covered with its envelope of wire-gauze, it does not light the vapors of the ether; when the flame enters the inflammable zone, a series of feeble detonations are heard; passing this first zone, you see the flame lengthen and almost entirely fill the space comprised within the gauze, the detonations are rather louder, and the lamp becomes much heated. Lower down, even just above the liquid, the flame disappeared, but it was sufficient to raise the lamp to cause the light to reappear; but if it be kept for some time in the inferior zone, where the flame had become weaker, it ends by being extinguished completely. These are very nearly the same effects as those produced by a safety lamp when it is carried into the explosive atmosphere of the gallery of a mine. Naptha offered phenomena exactly resembling those observed in the instance of ether. Alcohol and essential oil of turpentine acted in a similar manner: but as these liquids have a very feeble expansive power, it is necessary to warm them to obtain any decided effects. To sum up all, I am convinced, by numerous experiments, that the flame of the Davy lamp does not inflame the vapors of ether, naptha, alcohol, or essential oil of turpentine, whether these vapors arise from the liquids at the ordinary temperature, or from the same liquids in a state of ebullition. These results could, no doubt, have been foreseen; but you must nevertheless agree that it was well to prove their truth; and if I have considered them worthy of your attention, it is because I believe them susceptible of useful application. The apparatus I made use of appears to me to have been very well adapted to prove the Davy lamps. We cannot, in fact, deny that the accidents that happen in our mines where these lamps are employed, arise in some cases from rents or other injuries done to the wire gauze. Besides, every one must see how appropriate it is to verify from time to time the efficacy of safety lamps, and if this is not done, it is because the means are seldom at hand to prepare these mixtures of gaseous explosive compounds. But in a cylinder like that I have described, you may rapidly prove a great number of lamps, and that without any trouble, almost without any expense, by using a substance found everywhere, and easily preserved—sulphuric ether.

One of the most common causes of fire is, as

* *Comptes Rendus.*

you know, the imprudence with which flame is brought into the neighbourhood of inflammable liquids, too frequently the fire commences in cellars where spirits of wine and essential oils are bottled off. The facts I have mentioned prove that accidents of this nature would not happen if the simple precaution were taken of never approaching these matters without a Davy lamp; by the light of such a lamp, I am satisfied, you might bottle off even ether itself without incurring the least danger. The following is an instance connected with this subject, which is not without interest:—

I drew off a quantity of ether, contained in a reservoir, by means of a tube about two millimetres in diameter; bringing the flame of a candle to within a few centimetres of the stream of liquid, it instantly took fire, but nothing of the same kind arose from the flame imprisoned in its covering of wire gauze. You only heard in the interior of the gauze cylinder the slight deflagrations of which I have just spoken; the flame became lengthened, but the ether continued to flow; you may even let the jet fall upon the wire gauze without lighting the ether; the interior of the lamp is filled with flame, but this flame is not communicated to the exterior. In pouring a considerable quantity of ether on a Davy lamp, well lighted, I have often succeeded in extinguishing the flame, but never in setting fire to the ether.

The security Davy's lamp presents in mines subject to the escape of inflammable gas, is very great, without doubt, but this security is not absolute. Too agitated an air, or a current of hydrogen gas, moving with a certain degree of swiftness, with a sudden gust, and probably other causes, yet unknown, may in an instant destroy the efficacy of the protecting covering, and produce accidents which we at times are too apt to attribute to the imprudence of the workmen, or the imperfection of the apparatus.

At a time like this, when we are actively employed in the endeavour to utilize the voltaic pile, for the purpose of lighting up towns, we may be permitted to hope that subterranean labor may soon obtain the use of a light that shall originate, and be maintained in a vacuum, without requiring for its support the necessity of feeding the flame by means of an atmosphere, too frequently explosive. The galvanic pile of Grove, and that of Bunsen, which with forty-eight pairs of plates, alone, and consuming per hour materials of the value of one franc twenty centimes, gives a light equal to that of five or six stearine candles; this seems to resolve the question of economy, however great the inherent uncertainty of this method of valuation may be. Let us not also forget, that apart from the question as a matter of business, is the question of humanity. In the whole of Europe, one man a day perishes by the fire damp.

Not having one of Bunsen's piles at my disposition, I employed in my experiments a pile, the elements of which were zinc and copper, constructed in the most perfect manner by M. Münch, director of the Ecole Industrielle at Strasbourg. With this pile I was able to convince myself that the current established between two charcoal points, whether in a vacuum, or under water, produced a jet of light that might be carried without the least danger into an explosive atmosphere.

Would it not be well to construct little portable apparatuses, giving just sufficient light to illuminate a gallery? or would it be more advantageous to establish in the works, at well ventilated stations, intense foci of light, from whence the illumination might be directed in various directions by means of reflectors?

I am approaching here a subject of photometry that particularly regards yourself. If I did not extend my experiments further, it is because, my pile not having a constant current, I could not obtain a sufficiently constant light. I intend to renew my experiments as soon as I have procured proper apparatus, and above all when I shall be able to profit by the kind advice of my honourable associates.

ON THE TRANSFORMATION OF PERCHLORIDE OF MERCURY INTO PROTOCHLORIDE, BY CERTAIN PHARMACEUTICAL PREPARATIONS. COMPOUND PREPARATIONS OF MERCURY NOT INCOMPATIBLE WITH THESE PREPARATIONS.*

BY M. LEPAGE.

More than forty years since M. Boullay, at the present day one of the most learned professors of French pharmacy, pointed out the reduction of perchloride of mercury (corrosive sublimate) by contact with the syrup used in cookery, and its transformation under these circumstances into protochloride or calomel.

The observations of M. Boullay have, since the time he made them public, been confirmed by many experimentalists. Very recently, also, a distinguished chemist, M.

* *Journal de Pharmacie*, July, 1845.

Mialhe, read a paper before the *Societé de Pharmacie*, on several experiments of his own, during which he also recognised the transformation of sublimate into calomel by mixture with several pharmaceutical preparations, and particularly with the cook's syrup, a phenomenon this chemist attributes to the *glucose* the preparation contains.

M. Mialhe has also discovered that the perchloride of mercury remains unaltered by the contact of pure syrup of sugar.

Having had occasion to devote ourselves for some time past to experiments on the same subject, we, also, believe it to be our duty to inform the *Societé de Pharmacie* of the results. Our experiments teach us :—

1. That corrosive sublimate placed in contact with the cook's syrup, is completely decomposed and converted into calomel. The transformation begins immediately the mercurial salt and the syrup are mixed, and it is first perceived by the syrup losing its transparency. As fast as the calomel is generated it falls by little and little, to the bottom of the vessel, and after some time it may be collected to be subjected to particular examination.

2. That sal-ammoniac, and other alkaline chlorides, do not prevent the change taking place, and that, in as short a space of time, contrary to the opinions of some writers, who pretend, that in such a case, in the instance of sal-ammoniac, for example, when added to the mercurial salt, it gives stability to it, and prevents the reduction taking place.

3. That with the cook's syrup, previously acidulated by a few drops of hydrochloric acid, the reduction of the sublimate into calomel, equally takes place.

4. That the simple syrup of sarsaparilla (prepared from the hydro-alcoholic extract of the root, according to the formula) also transforms the sublimate into calomel.

5. That the syrup of honey, completely freed from the wax by means of chalk, also effects its reduction; but that with this and the preceding preparation, the complete transformation appears to be much slower in being effected, for we have found it possible, after a contact of two months, to separate a small quantity of sublimate by shaking it up with ether, while we have been unable to discover that salt when mixed with the cook's syrup, after the same lapse of time, and by resorting to the same means.

6. That the pure syrup of sugar does not decompose the sublimate, as M. Mialhe has also ascertained.

In the presence of there being well-established facts of which a good many medical men hold no account, for we still see them, we may almost say, daily prescribe sublimate mixed with cook's syrup, or some analogous preparation, we have asked ourselves if, among the soluble preparations of mercury possessing medicinal properties analogous to those of sublimate, there may not be some which could be mixed with syrups loaded with extractive matter, without undergoing decomposition? We have made experiments on this subject, and have discovered that bicyanuret of mercury* and the iodhydrargyrate of ioduret of potassium, which, as it is known, possesses properties very analagous to those of perchloride of mercury, are not affected by the above-mentioned preparations, which transform sublimate into calomel. We possess mixtures of these two substsnces with the cook's syrup, made more than a year back, in which the cyanuret and the iodhydrargyrate have experienced no decomposition.

The iodhydrargyrate of the ioduret of potassium, discovered by the late Polydore Boullay, has been, as we know, frequently employed with success, for several years, by Dr. Puch, physician to the *Hôpital du Midi*, as a remedy in syphilitic diseases. It is usually prepared by combining equal parts of ioduret of mercury and ioduret of potassium.

We entreat those physicians who, for the future, wish to combine a vegetable with a mercurial treatment, to consider the facts we have here related.

* The basic cyanuret of mercury (oxycyanuret), however, deposits after a time almost inappreciable traces of metallic mercury, by contact with the cook's syrup.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

IMPORTANT TO POTATO GROWERS.

BY WILLIAM HERAPATH.

My attention has been given to the disease which has shown itself so extensively amongst growing potatoes. I find, in almost every instance, that the epidermis of the stalk, below the surface of the ground, is more or less in a state of decay often disintegrated, and completely rotten; the leaves and branches accord with the state of that part of the stalk below the ground. The tuber, beneath the outer skin, is first spotted brown (like a bruised apple); these spots extend and penetrate towards the centre, quite changing the nature of the potato. Those near the surface are most injured; in some cases the lowest on the root are not at all affected, while the upper ones are useless. I should therefore expect that the longer the crop remains in the land, the greater the injury will be. It seems, from the microscope appearances, that the starch escapes injury for a long time after the skin and cellular parts are gone; and as the whole of the nutritive powers of the potato reside in the starch, I should recommend that wherever the disease has shown itself to any extent, the crop should be dug whether ripe or not, and the starch extracted by the following simple process:—After washing the roots, let them be rasped fine and thrown into a large tub or other vessel, pour a considerable quantity of water, and well agitate and rub the pulp with the hands; all the starch or fecula will, from its great weight, fall to the bottom, while the skin and fibrous matter will be carried away by the water; wash the starch with or two more waters, allowing it to fall after each washing; spread it upon cloths in a warm room to dry. In this way about 20 lbs. or 21 lbs. will be obtained from every 100 lbs. of potatoes, and it contains as much nourishment as the original roots; it will keep any length of time, and might be used with flour to make bread, pies, puddings, &c., as well as farinaceous spoon meat. This is much better than throwing away the diseased roots, and will furnish food for tens of thousands who might otherwise want it.

The following correspondenee has just taken place between Lord Portman, President of the Royal Agricultural Society, and William Herapath, Esq., the eminent analytical chemist, in reference to SEED POTATOES for 1846. The subject is of vital importance, and is worthy of the deepest consideration:—

"Bryanston, Sept. 13, 1845.

"Sir—I observe in the newspapers that you have directed your attention to the potato disease, and have advised as to the use of starch, &c. As I am specially bound, during this year of my holding the office of President of the Royal Agricultural Society of England, to promote inquiry and to notify observations on subjects relative to the produce of the soil, I trouble you with this letter, and ask if any method has occurred to you by which the potato may be preserved for the planting of 1846? I have found that potatoes, apparently sound and free from disease, though in a field or garden which has been partially diseased, have, after being stored away, shown signs of the disease and rotted off; and I fear that the greatest quantities of potatoes will thus perish, and so continue the distress of the poor to another season. I have directed some potatoes to be stored in slaked lime, in the hope that it may preserve them, but have, of course, yet had no time to judge of the effect. I, therefore, ask for your opinion, as one of our most eminent chemists, upon this point, and would ask leave to make known your reply, if you are able to offer an opinion sufficiently explicit to be useful.

"I remain your obedient servant,
"PORTMAN."

"W. Herapath, Esq."

"TO LORD PORTMAN, PRESIDENT OF THE AGRICULTURAL SOCIETY.

Brighton, Sept. 17, 1845.

"My Lord—In reply to your letter of the 13th inst., I must say that I do not think it would be either safe or prudent to depend upon the infected potatoes of the present season as seed for the next year; as, in all instances, I have found the diseased parts to extend when the potatoes are kept in a damp situation; I should therefore expect that if any diseased seed was kept so dry as not to rot before setting time, yet upon being planted and left in the damp soil, the rotting process would then begin, and the hopes of the husbandman be dis-

appointed. I have no doubt that some potatoes, apparently sound, have (as stated by your lordship) been found to be affected after stowing away; but I do not consider this to have been an origination of it, but merely that that which was noticed when dug has become apparent after storing.

"When a potato is first affected, the diseased parts are scarcely visible, but upon keeping it in a dark place the spots soon become dark and consequently more apparent, but the spots do not extend; if, however, the tuber has been kept in a damp place, the spots not only extend rapidly over the surface, but penetrate into the interior, and in a short time it will be entirely rotten.

"As far as the slaked lime, which you have used in your potato stores, has a tendency to prevent the tubers from touching each other, or, by its power of absorbing water, of keeping them dry, it will answer a good end; but it must not be expected to have any chemical effect upon the diseased parts of their juices.

"Anything which, like dry sawdust or sand, would prevent contact, would prevent the propagation from one tuber to another, and any substance capable of absorbing the moisture of the air in which the potato is stored, would prevent the extension of the disease in each diseased root.

"Our best microscopists and cryptogamists are divided in opinion as to whether the cause of the calamity is a fungus or not. After all the examination I have given to the subject, and a careful review of all the evidence brought before me on the two sides, I believe that it is; and I am daily confirmed in the opinion originally expressed, that the only advantageous way of treating the diseased potatoes is to obtain from them, by rasping and washing, the starch which they contain—by which process all their nutriment can be retained; and if it is well dried it will keep for any length of time.

"The operations can be performed in the cottage or manufactory alike, as no apparatus beyond a tin rasp (a nutmeg grater), a tub, and clean water, are required; and I have ascertained that, however far the disease might have extended, even if the root is rotten, yet the starch can be separated, and in a state fit to be eaten, if it shall be well washed, as all the bad parts come away with the water, while the great weight of the starch carries it to the bottom of the vessel.

"If it is required that the fecula should have all the qualities of the best foreign arrow-root, it is only necessary to wash it last in water containing a little chlorine, when it has unrivalled color and quality, and this I can speak of practically, having made many tons of the article. I will add, that an opinion has been circulated that the disease is owing to the introduction of guano as a manure: this, I feel no hesitation in contradicting, as I have seen it in situations where no guano has been used, and where every variety of manure has been resorted to.

"I am,

"Your Lordship's most obedient servant,

"WILLIAM HERAPATH."

The annexed letter on this important subject has been addressed to the Editor of the *Times*, by Mr. Erasmus Wilson:—

"Having had my attention called, during a short visit in Kent, to the destructive changes at present taking place in the potato crop, I beg to offer a few observations on the actual state of the tuber, in the hope that they may be of service to some of your readers, as tending to the preservation of so important an article of food.

"The real cause of the destructive changes at present taking place, appears to be the unripeness of the tuber, and the consequent imperfection of elaboration of the juices. When examined with the microscope, the cells of the potato are found to be not more than half filled with starch-cells, many of which are incomplete, the remaining portion of the cell being occupied by water. Hence the actual condition of the potato may be stated as follows:—First, deficiency of starch; second, imperfection in the tissue of the cell-walls; and third, excess of water, to which may possibly be added, imperfectly elaborated starch. As a consequence of th, imperfection of the tissue of the cell-walls, and its state of maceration in a superabundance of water, it falls speedily into decay, the change beginning at the surface and proceeding inwards, and being indicated by a brown discoloration of the cells. The starch-cells, which are at first unaffected, are soon enclosed in the decayed cellular tissue, and becoming involved in the decay, are thereby destroyed."

"Taking this view of the state of the potato, two modes suggest themselves of preventing the loss which must necessarily result from the recurrence of the above described changes. The first is that recommended more than a week since in your journal by Mr. Herapath, namely, of separating the starch by reducing the potato to a state of pulp, and collecting the washed precipitate. When it is recollected that the starch embodies the whole of the nutritive part of the potato the importance of this plan will at once be perceived. But, practically, there exists a great obstacle to the prosecu-

tion of the plan in the inconvenience of employing it on a small scale.

"The second mode, that which I am now about to suggest, seems to me to be calculated to meet the exigencies of the case at the same time that it is free from the objection stated above; it is, to dry the potatoes in an oven or kiln at a moderate temperature, and thus drive off the excess of water which they contain, the water being a chief agent in the decomposing process.

"With regard to the statements of the potato being a cause of disease to man and animals, I beg to remark, that, so far as my investigations have gone these are unfounded. In preparing the potato for table the discolored parts should, of course, be cut away; the potatoes should be boiled in two waters, and salt should be mingled with both. The instances which have been reported of pigs being destroyed by eating them are referable to the quantity, and not to the quality of the potato, and would as certainly occur with the best potatoes, taken in excess, as with those of inferior quality."

COMMON SALT TO KILL WEEDS.

BY C. A. A. LLOYD.

YOUR correspondent who killed his weeds on a pavement with salt, will find in the end that he has manured the ground, and that the weeds will grow more vigorous at some future time. Sulphate of iron in solution is very injurious to weeds upon walks, and especially in dry weather.

ELECTRICITY AND AGRICULTURE.*

I HAVE been looking for your republication of the accompanying letter for the last few weeks; but, as it has not appeared, I infer that you have not seen it. The "additional pickle dung" explains part of the astonishing results of Mr. Forster's experiments, and if the thrashing and weighing of his crop were conducted with equal care or good faith, we may form an idea of the mode in which 13½ quarters of barley per acre were raised.

I am not aware that Dr. Forster has replied to Mr. Cowie's letter, but the "additional pickle dung" was either applied by his orders, or it was not. In the former case, remark is superfluous; and in the latter, Dr. Forster is almost equally culpable, as a person who takes it upon himself to set at nought all that has been ascertained during the last century and a half of the laws which regulate the distribution of electricity, and to induce farmers to spend money upon the faith of his representations, ought to have personally superintended the preparations for his experiments, so that no mistake of the kind could occur.

The following are extracts from the letter alluded to. It appeared in the *Caledonian Mercury*, August 7th:—"I have been induced to send you this communication on account of my having a few days ago visited the northern counties, where I had an opportunity of seeing and examining into the *modus operandi* and results of Dr. Forster's experiments. I was accompanied on the occasion by two crack practical farmers. We drove our vehicle to the stables of Findrassie, where we intended to put up our horse for a short time.

"While I was engaged in searching for Dr. Forster, who, however, had left home, my friends got into conversation with a very communicative lad, a servant on the farm, respecting the experiments on electricity. The lad seemed rather astonished that we had come to see what was thought nothing of in the neighbourhood. On being asked if the crops were better where the poles and wires were placed than on the rest of the field, he answered, 'Weel, the crap sud be better, considering the additional pickle dung it got beside the wires, but that he could not say there was really any difference observable.'

"After this *exposé* our expectations were very moderate, but we determined to have ocular demonstration on the subject, notwithstanding the absence and want of permission of the lord of the manor, whose public announcements have, however, laid that portion of his grounds under experiment, in some measure, open to public exhibition.

"The poles and wires are placed in two very small fields, one of which is in pasture, and the other is a crop of barley. The first had not a living animal upon it, and humane and considerate it certainly was, for the total want of anything in the shape of grass beyond the roots would have starved any hill ewe—nibble she ever so eagerly. The devoted field, instead of being electrified, seems to be paralysed, and will to all appearance require some more 'pickles of additional dung' to revive its sensibilities after the shock it has sustained. Then, as to the barley, it seems neither to have suffered nor been ameliorated by the magic wires, for no perceptible difference can be seen over the field. The crop, with what electricity, the 'pickle additional dung,' and

* *Agricultural Gazette.*

all, looks at less than four quarters per acre."

Mr. Cowie adds—"Dr. Forster may have been himself deceived, and we must give him credit for wishing to enlighten his brethren, but he should now either acknowledge the failure of his experiments or submit them to the inspection of some of those shrewd Morayshire farmers by whom he is surrounded." Here we quite agree with him; for, judging from all the similar experiments reported to us, the extraordinary results obtained last year at Findrassie, must surely have been owing to some oversight or error.

IMPROVEMENTS IN THE PREPARATION OF VARIOUS SUBSTANCES FOR THE PURPOSE OF DYEING AND PRODUCING COLOR; ALSO IMPROVEMENTS IN THE APPLICATION AND USE OF SEVERAL CHEMICAL COMPOUNDS FOR THE PURPOSE OF DYEING AND PRODUCING COLOR.*

BY JOHN SWINDELLS.

These improvements in dyeing and producing color consist, in the first place, in preparing (when madder, madder root, and munjeet is used) the madder in the following way:—Take any given quantity of madder and reduce it to a fine powder, then mix it with as much of a solution of caustic ammonia, potash, or soda, as will thoroughly carbonise the yellow or fawn coloring matter therein: different kinds of madder will require varying proportions. The best French madder will require one-eighth part of its weight of caustic alkali, or of ammonia, as much of the solution as will be equivalent in saturating a given weight of an acid, as one-eighth of potash. The powdered madder, when mixed with any of these solutions, is exposed to a heat not exceeding 175°F.; it is then dissolved in water, and is ready to be used in dyeing, or, forming madder lakes or pinks.

If preferred, the madder may be first treated with sulphuric acid, as in making garancine, and the alkali afterwards applied. By this method the operation of heat upon the alkalies will not be required, but they may be dissolved in a solution of any of the alkalies, or their carbonates, or other salts thereof; for these purposes the patentee prefers the caustic solution of ammonia, as producing the best effects.

Cotton and linen fabrics which are required to be dyed with the prepared madder, or with the common kinds, or even with other vegetable matters, are prepared in the following manner:—After the fabrics are bleached, and thoroughly cleansed from impurities, they are steeped in a solution of gelatine or albumen, of a specific gravity of 1·04, for several hours; they are then removed, and steeped in a strong solution of tannin for twelve hours; after which they must be wrung out and thoroughly dried. This process may be repeated, or not, according to the depth of color required; and the usual process of dyeing may be proceeded with in the ordinary way.

The second part of the invention consists in preparing for dyeing blues and similar colors, the compounds of cyanogen and ammonia, as follows:—

Instead of horn, and hoof, and other animal matters, commonly employed for this purpose, the patentee grinds to a fine powder common coal, cannel coke, or charcoal, or other carbonaceous matter, and mixes therewith a solution of gelatine or albumen, or both combined; and after thoroughly drying the compound, he uses it in the same way as the horn and hoof is used. Another method of producing the compounds of nitrogen, viz., cyanogen or prussiates and ammonia, consists in combining nitrogen gas or the oxides of nitrogen with carbon, as follows:—When nitrogen is to be used, it may be obtained by passing atmospheric air, or any of its compounds or mixtures through heated carbonaceous matter; but charcoal or coke is preferred, so as to form oxygen, if it be atmospheric air, into carbonic acid. In order to accelerate the process, the mixed nitrogen and carbonic acid is forced through lime, either in a semi-fluid or hydrate state; or it may be passed through an alkaline solution, or through alkaline earth, which will take up the whole of the carbonic acid, and any aqueous vapor that may be present, leaving the nitrogen gas moderately free from other gases. The nitrogen gas is then passed through charcoal, previously saturated with potash or soda, kept to a full red heat in a close retort, until the required quantity of cyanide is produced. If ammonia is required to be obtained, nitrogen gas is passed, together with one-fourth its volume of steam, through charcoal kept to a red heat; the ammonia so produced is condensed by the usual process. Or atmospheric air may be used and passed with steam through carbonaceous substances kept to a red heat, either in open or close vessels. When the protoxide, deutoxide, or nitrous vapor is employed for producing cyanogen, the same process may be used as that described in

* *London Repertory.*

operating with nitrogen gas. In forming ammonia, or its compounds, from the oxides of nitrogen or nitrous vapor, they are passed with their own volume of steam through charcoal, at a red heat, either in close or open vessels; but if cyanogen is to be produced, the apparatus containing the charcoal must be air-tight, in order to exclude oxygen, carbonic acid, or aqueous vapor.

The third part of the invention, viz., improvements in the application and use of several chemical compounds for the purpose of dyeing or producing color, consists in operating upon a class of salts not hitherto used in producing color.

The cyanides, ferro-cyanides, or other salts of cyanogen, are employed for the above purpose, by separating the acid, which is required to produce the color, from its combination with barium, strontium, or calcium: the patentee prefers to operate on a salt of barium; and he precipitates the barium by its equivalent quantity of sulphuric acid, diluted with as much water as may be necessary to produce the required strength of acid to be used in dyeing. The chromates and bichromates of barytes, strontia, or lime, are also treated with sulphuric acid and water, to separate their bases from the chromic acid. The manganates of barytes, strontia, or lime, are likewise treated in the same manner. The acid solutions thus obtained are employed for raising, dyeing, and oxidating various colored fabrics.

The patentee claims, firstly, the use of ammonia, or other alkalies, for preparing a madder dyeing liquor. Secondly, the process of preparing cotton and linen, by subjecting them to the action of gelatine, or albumen, and tannin. Thirdly, the mode of preparing materials for the manufacture of cyanogen or prussiates, by using gelatine or albumen mixed or ground together with carbonaceous matter of any description. Fourthly, the production of cyanogen and its compounds by operating on nitrogen gas, after having removed any other gas or aqueous vapor therefrom, before introducing it to the heated carbonaceous matter. Fifthly, the production of cyanogen and its compounds, by operating on the oxides of nitrogen or nitrous vapor, however procured, by bringing them in contact with heated carbonaceous matter, either after having separated the oxygen or without separating it. Sixthly, the production of ammonia, or its salts, by operating on the nitrogen of the atmosphere, or any other oxides of nitrogen, in conjunction with aqueous vapor, and passing the same through heated carbonaceous matter. Seventhly, the application or use of the cyanides, ferro-cyanides, or chloro-cyanides, or any other combination of cyanogen and barium, strontium, or calcium, in dyeing, printing, or producing color. Eighthly, the use and application of the chromates or bichromates of barytes, strontia, or lime, for the purpose of dyeing, printing, and producing color. Ninthly, the use and introduction of the manganates of barytes, strontia, or lime, or the same purposes.

AN ADDRESS TO THE AGRICULTURISTS OF GREAT BRITAIN, EXPLAINING THE PRINCIPLES AND USE OF HIS ARTIFICIAL MANURES.

BY PROFESSOR JUSTUS LIEBIG.

If we compare the experience of farmers regarding the fertility of the soil and the quantity of its productions, we are surprised by a result which surpasses all others in general application and uniformity.

It has been observed, that in every part of the globe where agriculture is carried on, in all varieties of soil, and with the most different plants and modes of cultivation, the produce of a field on which the same or different plants have been cultivated during a certain number of years, decreases more or less in quantity, and that it again obtains its fertility by a supply of excrements of man and animals, which generally are called manure; that the produce of the fields can be increased by the same matters, and that the quantity of the crop is in direct proportion to the quantity of the manure.

In former times scarcely any attempt was made to account for the cause of this curious property of the excrements of man and animals. Without taking into consideration the origin of the excrements and the relation they bear to the food, it is not astonishing that their effect was ascribed to a remnant of vital power which should qualify them to increase the vitality in plants. Ascribing their influence on the fertility of the fields to an incomprehensible occult cause, it was forgotten that every force has its material substratum, that with a lever, in a mathematical sense, which possesses no extension and gravity, no effect can be produced, no burden raised.

Guided by experience, which is the fundamental basis of all inductive science, and which teaches us that for every effect there is a cause, that every quality, as, for instance, the fertility of a field, the nourishing quality of a vegetable, or the effect of a manure, is intimately connected with and occasioned by something which can be ascertained by weight and measure; modern science has succeeded in enlightening us on the cause of the fertility

of the fields and on the effects which are exercised on them by manure. Chemistry has shown that these properties are produced by the composition of the fields; that their fitness for producing wheat or any other kind of plant bears a direct proportion to certain elements contained in the soil, which are absorbed by the plants. It has likewise shown that two fields, of unequal fertility, contain unequal quantities of these elements; or that a fertile soil contains them in a different form or state from another, which is less fertile. If the elements are contained in the soil in sufficient quantities, it produces a rich crop; if it be defective even in one of them only, this is shown very soon, by the impossibility of growing on it certain kinds of plants.

Moreover, it has proved with certainty what relations these elements of the soil bear to the developement of the plants. Chemical analysis has demonstrated that a certain class of these elements is contained in the seeds; others, in different proportions, in the leaves, roots, tubers, stalks. They are mineral substances, and as such, are indestructible by fire, and consequently remain as ashes after the incineration of the plants or of their parts. Many of these elements are soluble in pure water, others only in water containing carbonic acid, as rain water; all were absorbed from the soil by the roots of the plants in a dissolved condition. It has been shown that, if in a field, those elements which remain after the incineration of the grain or seeds, are not present in a sufficient quantity, no wheat, no barley, no peas—in a word, none of those plants can be cultivated on that field which are grown on account of their seeds. The plants which grow on such a field produce stalks and leaves; they blossom, but do not bear fruit. The same has been observed regarding the developement of leaves, roots, and tubers, and the mineral elements which they leave behind after their incineration. If, in a soil in which turnips or potatoes are to be cultivated, the elements of the ashes of these roots are wanting, the plants bring forth leaves, stalks, blossoms, and seeds, but the roots and tubercles are imperfect. Every one of the elements which the soil gives up to the plants is in a direct quantitative proportion to the production of the separate elements of the plants. Two fields which, under otherwise equal circumstances, are unequally rich in mineral elements of the grain, produce unequal crops. One containing them in larger quantity produces more than another containing them in less. In the same manner, the capacity of a soil to produce tuberculous plants, or such as have many leaves, depends upon its amount of those elements which are found in the ashes of such plants.

It results from this with certainty, that the mineral substances which are furnished by the soil, and which are found again in the ashes of plants, are their true food; that they are the conditions of vegetable life.

It is evident, that from a field in which different plants are cultivated, we remove with the crop a certain quantity of these elements; in the seeds those mineral parts which the soil had to provide for *their* developement, and in the roots, tubercles, stalks, and leaves, those elements which are necessary for *their* production. However rich the field may be in these elements, there can be no doubt that, by several cultures, it becomes more and more impoverished; that for every plant a time must arrive when the soil will cease to furnish, in sufficient quantity, those elements which are necessary for a perfect growth. Even if such a field, during many years, had produced twenty-five or thirty fold the amount of the seed, for instance, of wheat, experience shows that the crop gradually decreases, until at last the amount will be so small as to approach the plant in its wild state, and not repay the cost of cultivation.

According to the unequal quantity in which the mineral elements of grain, tubercles, roots, seeds, leaves, are contained in a soil, or according to the proportions in which they have been removed in the crop, the land may have ceased to be fertile for roots and tubercles, but it may yet produce good crops of wheat. Another may not produce wheat, but potatoes and turnips may thrive well in it. The mineral substances contained in a fertile soil, and serving as food to the plants, are taken up by them with the water, in which they are soluble. In a fertile field they are contained in a state which allows of their being absorbed by the plant and taken up by the roots. There are fields which are rich in these elements, without being fertile in an equal proportion; in the latter case, they are united with other elements into chemical compounds, which counteract the dissolving power of water. By the contemporaneous action of water and air—of the oxygen and carbonic acid of the atmosphere—these compounds are decomposed, and those of their constituent elements, which are soluble in water, but which had been insoluble by the chemical affinity of other mineral substances, re-obtain the property of being absorbed by the roots of the plants.

The duration of the fertility of a field depends on the amount of the mineral aliments of plants contained in it, and its productive

power for a given time is in a direct proportion to that part of its composition which possesses the capacity of being taken up by the plant. A number of the most important agricultural operations, especially the mechanical, exercise an influence on the fertility of the fields only thus far, that they remove the impediments which are opposed to the assimilation of the mineral food into the vegetable organism. By ploughing, for instance, the surface of the fields is made accessible to air and moisture. The nutritious elements contained in the soil in a latent state, acquire, by these operations, the properties necessary for their transmission into the plants. It is easy to conceive the useful influence which, in this respect, is exercised on the produce of the fields by the care and industry of the farmer. But all these labors and efforts do not increase the amount of mineral elements in the field: in rendering soluble in a given time, a larger quantity of the insoluble substances, and obtaining by these means a richer crop—the time is merely hastened, in which the soil becomes exhausted.

The experience of centuries has shown that, with the help of manure, of the excrements of animals and man, with which we supply those fields which have ceased to produce crops of grain, &c., serving as food for man and animals, in a sufficient quantity, the original fertility can again be restored; an exhausted field which scarcely yielded back the seed, is made to produce a twenty and more fold crop, according to the proportion of the manure provided.

Regarding the mode of action of manures, it has been observed, that all excrements do not exercise an equal influence on plants. The excrements of sheep and cattle, for instance, increase in most fields the crop of roots and herbaceous plants to a far greater degree than those of man and birds (guano). The latter act far more favorably on the production of grain crops, especially if they are added to the animal excrements, and are given to the fields at the same time.

A field, for example, which has lost its fertility for potatoes and turnips, but on which peas and beans still thrive, becomes far more fertile, by a supply of the excrements of horses and cows, for a new crop of potatoes and turnips, than by manuring it with the excrements of man or with guano.

The most accurate experiments and analyses have pointed out that the excrements of man and animals contain those substances, to the presence of which the fertility of the soil is due. The fertilising power of manure can be determined by weight, as its effect is in a direct ratio to its amount in the mineral elements of the food of plants. The truth of the result of these chemical analyses must be evident to every one who inquires into the origin of excrements.

All the excrements of man and animals are derived from the plants of our fields; in the oats and hay, which serve as food to the horse, in the roots which are consumed by a cow there are a certain quantity of mineral ingredients. A horse, in consuming 15 lbs. uf hay and 4½ lbs of oats per day, consumes 21 ounces of those substances which the hay and the oats took from the fields; he consumes annually 480 lbs. of these constituent elements of the soil, but only a very small portion of them remains in his body. If a horse, during one year, increases 100 lbs. in weight, this increase contains only 7 lbs of those mineral substances which were contained in the food. But what has become of the 473 lbs. which we cannot detect in his body?

The analysis of the fluid and solid excrements which the horse gives out daily, shows that the ingredients of the soil which do not remain in the body of the animal are contained in its excrements, it shows that in an adult animal, which from day to day neither increases nor decreases in weight, the amount of the mineral ingredients of the excrements is equal in weight to the mineral ingredients of the food.

As with the horse, so it is with all animals. *In all adult animals the excrements contain the ingredients of the soil according to the quantities and relative proportions in which they are contained in their food.*

The mineral substances of the food which have remained in the body of the animals, and served to increase their weight, are found again in the bones and excrements of man who consumes the flesh of these animals.

The excrements of man contain the elements of the soil, of bread or of grain, of vegetables and meat.

These discoveries explain, in a most simple and satisfactory manner, the fertilising effect which manure produces on our fields.

It is now obvious why manure renders again fertile the exhausted fields; why, by its means, their productiveness can be augmented; why the latter is in a direct ratio to the quantity of manure administered.

The exhaustion of the soil, by subsequent crops,—its decrease in fertility,—is produced by the gradual removal of the mineral elements, in a soluble state, which are necessary for the development of our cultivated plants. By a supply of manure they are again restored to the state suited to serve as nourishment to a new vegetation.

If the supply of the removed elements of the soil, by means of manure, be sufficient,

if the quantity taken away be restored, the original fertility re-appears; if the supply be greater, the produce increases; a defective supply gives a smaller produce.

It is now explained why the different kinds of manure exercise an unequal effect upon the fields.

The excrements of man and guano containing especially the mineral ingredients of grain and of meat, exercise far greater influence on the amount of produce of grain in a field in which these ingredients are wanting, even if those of the leaves and stalks are present in suffieient quantity, than the excrements of an animal which feeds on roots or green fodder. The excrements of the latter contain the mineral elements of the leaves, stalks, and roots in prevailing quantity, and have a greater value for the production of roots and foliaceous plants than those of man or of birds, which contain only a small quantity of those mineral substances which they require for their development.

If we compare, for instance, the composition of guano with the excrements of the cow—solid and fluid excrements in the same state of dryness—it is found, that in an equal weight, the latter contain five to seven times more of the mineral ingredients of turnips and potatoes, than the former. If, in a soil, which is deprived of all these mineral substances, we wish to force a crop of turnips by means of guano, we require at least five times more of guano than dung of cattle.

The same thing happens, though *vice versa*, if we wish to produce a rich crop of grain by means of animal excrements; in this case, one part of guano and five parts of animal excrements produce the same effect as 13 to 15 parts of animal excrements.

To understand the proper meaning of these numerical proportions, it is sufficient to mention, that 400 pounds of bones contain as much phosphoric acid as 1,000 pounds of wheat; these 400 pounds of bones can furnish sufficient phosphoric acid to 8 acres.

If we take the importation of bones into Great Britain, in the last ten years, to amount to one million of tons, enough phosphoric acid has been supplied to the fields for 25 million tons of wheat; but only a small proportion of the phosphoric acid of the bones is in a state to be assimilated by the plants and applied to the formation of the grain. The plants, in order to apply the other far greater part of that phosphoric acid to their formation, must find a certain quantity of alkaline bases besides the bone-earth, which are not given to the plants in the bones, because they contain neither potash nor soda.

To have increased the fertility of the fields in the right proportion, 800,000 tons of potash ought to have been added to one million tons of bones, in a suitable form.

The same is the case with guano: 60 to 100 pounds of it are sufficient to furnish phosphoric acid to one acre of turnips; but the four to eightfold quantity is required to furnish the turnips with the necessary alkaline bases, and it is still doubtful whether they can be at all provided with the latter, by means of the salts with alkaline bases, which the guano contains.

At a time, when the necessity of the mineral substances for the growth and development of the plants, and the direct relation which the effect of manure has to its amount of the same substances, had not been ascertained, a prominent value was ascribed to the organic matters which it contained. For a long time it was thought that the produce of a field of those substances, contained ing nitrogen, which serve as food for man and animals, stood in a direct proportion to the nitrogen contained in manure. It was believed that its commercial value, or its value as manure, might be expressed in per cents by its proportion of nitrogen, but later and more convincing observations have induced me to contradict this opinion.

If the nitrogen and carbonic acid formed by the decay and decomposition of the vegetable ingredients of manure, were the cause of its fertilising power, this ought also to be seen if the mineral substances were excluded. Direct experiments have shown, that the nitrogen of the excrements can be assimilated by the plants, in the form of ammonia, but that ammonia as well as carbonic acid, although it is indispensable for the development of all plants, can accelerate the growth of plants and increase the produce of a field of grain, roots, and tubercles *only*, if, at the same time, the mineral ingredients contained in the manure which is applied, are in a state in which they are suited for assimilation. If the latter are excluded, carbonic acid and ammonia have no effect on vegetation.

On the other hand, experience has shown that, on many fields, produce which is rich in carbon and ammonia, can be increased to an extraordinary amount, without any supply of such matters as furnish these substances.

On fields which are provided with a certain quantity of marl or slacked lime, or with bone earth and gypsum—substances which cannot give up to the plants either carbon or nitrogen — rich crops of grain, tubers, and roots are obtained in many places, entirely in contradiction with the view which ascribes the effect of the manure to its

amount of ingredients containing nitrogen or carbonic acid.

To explain this process, which is so opposite to the common opinion, the marl, the lime, the gypsum, the alkalies, and the bone earth, were regarded as stimulants, which acted on the plants like spices on the food of man, of which it was believed that they increased the power of assimilation, and allowed the individuals to consume larger quantities of food.

This view is contradicted if we consider that stimulants mean such substances as do not serve for the nourishment of the organism or for the formation of organic elements, and can only increase the weight of the body, if at the same time a certain increase of food is given. In supplying the fields with the above-mentioned substances, the weight of the plants became increased in all their separate parts, without their having been provided with the quantity of food which, according to theory, was necessary to this extraordinary increase, viz., with carbonic acid and ammonia.

Chemical analysis shows that these so-called stimulants are either actual ingredients of manure, as gypsum, bone earth, and the active substances of the marl, or that they are the means by which the mineral elements contained in the soil are resolved into a state adapted for being assimilated by the plants; this is generally effected by the application of slacked lime. They consequently exercise on the vital process of the plants not a mere stimulus like the spices, but are consumed for the development of the leaves, seeds, roots, &c., they become constituent parts of them, as can be shown with certainty by chemical analysis.

The success which has followed the application of these substances to the fields has explained in a most striking manner, the origin of the carbon and nitrogen in the plants.

In the marl, in the bone earth, in the gypsum, in the nitrate of soda, no carbon is provided to the fields; and yet, in many cases, the same produce, in some even a higher one is obtained, than by the application of a manure containing carbon and nitrogen. As the soil, after the crop, does not contain less carbonaceous or nitrogenous substances, it is evident that these products, which had been obtained without any carbonic or azotic manures, must have got the carbon and nitrogen of their leaves, roots, and stalks from the atmosphere; it follows, therefore, that the productiveness of the fields cannot be increased in proportion with a supply of carbonaceous and azotic substances, but that the fertility depends only on the supply of those ingredients which should be provided by the soil.

The soil not only serves the purpose of fixing the plants and their roots; it participates in vegetable life through the absorption of certain of its elements. If these elements are present in sufficient quantity, and in appropriate proportions, the soil contains the conditions which render the plant capable of absorbing carbonic acid and ammonia from the air, which is an inexhaustible storehouse for them, and render their elements capable of being assimilated by their organism.

The agriculturist must, therefore, confine himself to giving to the field the composition necessary to the development of the plants which he intends to grow; it must be his principal task to supply and restore *all* the elements required in the soil, and not only one, as is so frequently done; the ingredients of the air, carbonic acid and ammonia, the plants can, in most cases, procure without man's interference; he must take care to give to his field that physical condition which renders possible and increases the assimilation of these ingredients by the plant; he must remove the impediments which diminish their effect.

The favorable influence which bone earth, gypsum, nitrate of soda, exercise on the fields has induced many farmers to the belief that, in applying them they can dispense with manure or with the other elements of the soil; it requires, however, only little attention to see the great error of this opinion. We observe that the effect of these substances is not equal on all fields; in one place the amount of produce is increased by the lime, by the bone earth, and by gypsum; in another country, or on other fields, these substances in no way favor vegetation. From this arises the contradictory views of farmers regarding these matters as manures. If one farmer thinks the liming of his fields quite indispensable for rendering them fertile, another declares that lime produces no effect at all.

The reason of this difference is very simple. The examination of a soil upon which lime has had no effect, shows that it was already rich in this substance; it further shows that its effect extends only to those kinds of soil in which lime is wanting, or in which it is found in too small a quantity, or in a condition which is not suited to its assimilation by the plant. Lime especially serves for resolving the silicates of alumina (clay), and, consequently, it cannot fertilise soils in which clay is wanting—for instance, sandy soils. It must be apparent to every one, that on the calcareous and gypseous fields of France and England one-half per cent. of gypsum or

lime can have no influence at all on vegetation. This can be said with equal justice of bone ashes, and of every other mineral substance serving for the nourishment of plants.

If these substances exercise a favorable effect, some of the constituents of the soil or manure are restored, which are indispensable to the nourishment of plants, and which have been wanting in the soil. If this be the case the other bodies, equally necessary, must be present in sufficient quantity. On a field, in which sulphate of lime has acted favorably, but in which clover had been cultivated without it, the crop was 2,200 pounds of clover hay, in which 53 pounds of potash were removed. On the same field, after it had been dressed with gypsum, 8,000 pounds of hay were produced, which contained 191 pounds of potash. If this potash had not been present in the soil, the gypsum would have had no effect, the crop would not have been increased. On fields, which are richly provided with all other mineral ingredients, with the exception of gypsum, the latter is applied with the greatest success. But if gypsum is present in the soil, the same effects are produced by ashes and lime, as is the case in Flanders. On fields, in which phosphate of lime is wanting, bone ashes increase the produce of grain, clover, or grass, and on argillaceous soil, lime produces a decided improvement. All these substances act only on those fields which are defective in them, and if the other elements of the soil are present. The latter cause the former to come into action, and *vice versa*. The farmers, who thought that by using lime, gypsum, bone-earth, &c., they might dispense with animal manure, very soon observed that their fields deteriorated. They observed that after a third or fourth successive manuring with those simple substances the produce decreased; that, as is the common expression, the soil became tired of the manure, that at last the field scarcely produced the seed.

It is evident from this, what is the action of the mineral elements in the soil. If in fact, in the first years, the produce of the soil had increased by the application of bone ashes, or by a single element of the manure —if this increase was dependant on the amount in the soil of the other mineral elements, a certain quantity of those was annually taken up by the plants and removed in the harvest, and a time must at last arrive in which it is exhausted by the repeated removal; the soil must become barren, because of all removed elements, only one or the other, and not all of them, in a right proportion, have been restored.

The right proportion of the supply is, however, the only true scientific basis of agriculture.

If we subject the fluid and solid excrements of men and animals to an exact analysis, and compare the elements of them according to their weight, some constant relations between these elements impress themselves upon the mind, the knowledge of which is of some importance.

If the excrements of an animal are collected with some care and left to themselves for some days, their nitrogen appears to have been converted more or less perfectly, into ammonia. In the fluid excrements, in the urine, the salts of the food, which are soluble in water, are found in the form of alkaline carbonates, or of sulphates, phosphates, and other salts, with alkaline bases. In the solid excrements or fœces, silica, if it was contained in the food, earthy carbonates, and phosphates are the principal ingredients.

The quantity of alkaline carbonates bears a certain proportion to the amylum, sugar, pectine, or gum of the food. The urine of an animal which has been fed with potatoes or turnips, is rich in alkaline carbonates; the potatoes, however, consist principally of amylum; the chief ingredients of the turnips are sugar and pectine. The urine of a horse, which has been fed with hay and oats, is comparatively poor in alkalies, if compared with the former.

It is further shown, that the ammonia or the nitrogen of the excrements bears a certain proportion to the phosphates; the azote increases or decreases with the quantity of the phosphates in a manner that both can serve as a measure for each other, although not quite as an accurate one. It is not quite accurate, because the gum and the amylum also contain a certain, although small, quantity of phosphate of lime, as has been proved in my laboratory.

The ammonia of the excrements is of course derived from the nitrogenous substances in the food; the phosphates are likewise constituents of the latter. In the composition of the food an equally constant proportion exists between both. A given weight of gluten or caseïn in peas or in grain always corresponds with a certain weight of phosphates; if the grain or the vegetable is rich in these nitrogenous products of vegetable life, it is also rich in phosphates; if it is deficient in them the quantity of the latter decreases in an equal ratio.

As the amount of nitrogen in manure is a measure for its amount of phosphates, and as the manure contains besides these the other ingredients of the soil which are required by the grain or by the other vegetables for their development, and taken up by them from

the soil, it is easily conceived what was the cause of the error in regarding the nitrogen of the manure as the principal cause of its efficacy. The reason was, that the ammonia of the manure is always accompanied by the mineral elements which affect its nourishing qualities, because they render its assimilation into the organism of the plant and its transition into a nitrogenous constituent possible. Without phosphates, and without the other mineral elements of the food of plants, the ammonia exercises no influence whatever upon vegetable life.

If it has been shown that the fertility of the soil depends on certain mineral substances; if the restoration of the fertility of exhausted fields by means of the excrements of man and animals depends on their proportions of these matters; if the effect of the manures *accelerating* the vegetation depends upon their proportions of ammonia, it is clear that we can only dispense with the latter when we provide *all* efficacious elements exactly in those proportions and in that form most proper for assimilation by the vegetaole organism in which they are found, in the most fertile soil or in the most efficacious manure.

According to our present knowledge of the effect of the constitueut parts of manure, I feel convinced that it is indifferent to the plants from which source they are derived. The dissolved apatite (phosphate of lime) from Spain, the potash derived from felspar, the ammonia from the gas works must exercise the same effects on vegetable life as the bone earth, the potash, or the ammonia, which we provide in manure.

We live in a time when this conclusion is to be subjected to a comprehensive and accurate trial, and if the result corresponds with the expectations which we are entitled to form, if the animal excrements can be replaced by their efficacious elements, a new era of agriculture must begin.

I invite the enlightened farmers of England to unite with me for that purpose, and to lend me their aid. Whatever may be the result of these experiments, it is necessary for the future prosperity of agriculture that they should be made. They will enrich us with a number of valuable facts—we shall ascertain where we have wasted efficacious matters in the common course of farming—we shall acquire an exact knowledge of those substances which are necessary, and of those which are dispensable.

For a number of years myself and many talented young chemists have been occupied with the analyses of those mineral substances which are constituent elements of our plants of culture, and with the examination of the excrements of man and animals, as well as of a great number of soils acknowledged as fertile. These labours have been laid before the scientific world long since, but only a very confined application has been made of them in agriculture.

The farmer is by his position not in the condition to procure and to command the efficacious elements necessary for the restoration and increase of the fertility of his fields in a right proportion and suitable form. For this purpose science and industry must combine their aid.

I have been so fortunate as to remove the difficulties which are opposed to the application of a mere mixture of the elements of manure. If we employ the different elements of manure exactly in those proportions in which they are necessary according to experience, for a rich crop of wheat, peas, turnips, potatoes, &c., and if, at the same time, we leave them in their common state, they do not produce that effect which we might have expected; the cause of this is, that the different elements of manure possess a very unequal stability—the ammonia evaporates, the soluble elements are carried off by the rain, and the effect is more in proportion with the amount of those ingredients of the manure which are less soluble.

I have found means to give to every soluble ingredient of manure, by its combination with others, any degree of solubility, without altering its effect on vegetation. I give, for instance, the alkalies in such a state as not to be more soluble than gypsum, which, as is well known, acts through many years, even as long as a particle of it remains in the soil.

The mixture of the manures has been adapted to the mean quantity of rain in this country; the manure which is used in summer has a greater degree of solubility than that used in winter. Experience must lead to further results, and in future the farmer will be able to calculate the amount of produce of his fields, if temperature, want of rain, &c., do not oppose the manures coming fairly into action.

I must, however, observe that the artificial manures in no wey alter the mechanical condition of the fields, that they do not render a heavy soil more accessible to air and moisture. For such fields, the porous stable manure will always have its great value; it can be given together with the artificial manure.

All manure which is to be used during next winter contains a quantity of ammonia corresponding with the amount of nitrogen in the grain and crops which are to be grown. Experiments, in which I am at present engaged, will show whether in future times the

cost of this manure can be greatly lessened by excluding half or the whole amount of ammonia. I believe that this can be accomplished for many plants, as for clover and all very foliaceous vegetables, and for peas and beans; but my trials are not so far advanced as to prove the fact with certainty.

Giessen University, 1845.

ON THE PRECAUTIONS TO BE TAKEN AGAINST BUNT AND SMUT.*

From a variety of considerations, it has always appeared to me strange that practical agriculturists are accustomed to pay so little attention to the raising of pure seed crops. There may be reasons which I do not properly appreciate, that would render it inexpedient to cultivate a seed-crop apart from the rest of the produce raised on a farm; but I should have thought that it was always worth while for every farmer to set aside some portion of ground to be more carefully tended than the rest, for the purpose of securing good and perfectly clean seed. Among other reasons for such a practice, he would then be able to weed his crop from every plant infected with bunt or smut, before the fungi ripened.

A parcel of thoroughly bunted or smutted seed should be divided into a number of small packets, each of the same weight; or if very small, each containing the same number of grains. When any of these packets are steeped or washed, the floated grains and the fungi which rise to the surface may be kept apart for separate experiments, being carefully labelled "F. 1," "F. 2," &c., to show that they were obtained from the packets of seeds, "No. 1," "No. 2," &c., by this process.

No. 1. Seeds unwashed, to serve as a comparative experiment.
— 2. Washed in cold water only—(2*), in scalding water.
— 3. Washed in water with lime, the proportions specified.
— 4. Washed in water and brine.
  (*a*) Mixed in the proportion of 2 water to 1 saturated brine.
  (*b*) ,, ,, ,, 1 ,, 1 ,,
  (*c*) ,, ,, ,, 1 ,, 2 ,,
  (*d*) Saturated brine.
— 5. Washed in sulphate of copper.
— 6. Sprinkled, but not washed, with lime.

Another set of experiments may be prepared with clean corn, to be infected with the floated fungi obtained in washing the above.

No. 7 rubbed with—
  (*a*) F. 2, to serve as a comparative experiment.
  (*b*) F. 3.
  (*c*) F. 4.
  (*d*) F. 5.

Another set may be prepared with clean corn, which should first be steeped in different solutions, and then rubbed with the fungi obtained by simply washing in water (No. 2).

No. 8. Steeped in lime, and rubbed with F. 2.
— 9. ,, brine and do.
— 10. ,, sulphate of copper and do.

These last may be repeated by washing the seeds clean after they have been steeped, to remove any of the mixture that may here adhere to the *surface*.

No. 11. Prepared as No. 8.
— 12. ,, 9.
— 13. ,, 10., &c., &c.

* Prof. Henslow in *English Agri. Soc. Journ*, vol. ii.

IMPROVEMENTS IN THE MANUFACTURE OF SODA.*

BY GEORGE BROWN.

The object of this invention is to transform the sulphuret of sodium, produced in the manufacture of soda, into sulphate, sulphite, or hyposulphite of soda, and so make soda free from those impurities which render it less fit than it otherwise would be for soap-making, bleaching, and other uses. The patentee proposes to effect this by fluxing the soda, containing sulphuret of sodium, with a sufficient quantity of nitrate, chlorate, or chlorite of soda, or other body possessing an oxidising power: but, in most cases, he prefers to use the nitrate of soda.

One mode of carrying this invention into effect is as follows:—The liquor drawn from crude soda is boiled down in a pan or furnace, until about four-fifths of the salt are precipitated; the whole is then removed from the furnace, and the wet pasty salt allowed to remain for some time in a vessel to drain; by this means the greatest proportion of the liquor that remained in the mass (commonly known as red liquor) will

* *London Repertory.*

be separated from it. The red liquor (which consists chiefly of caustic soda and sulphuret of sodium, with a certain proportion of neutral salts, such as sulphate of soda and common salt) is put into a metal pan, and evaporated to about the consistency of tar; then the proper quantity of nitrate of soda is added, and heat applied to expel the remaining water, and flux the mass. The application of heat causes copious fumes of ammonia to be exhaled, and the mass froths and bubbles up, which serves as a guide to the operator in adding the nitrate of soda; as when no ebullition is produced by the addition of a fresh portion of nitrate of soda, it is a proof that the proper quantity has been added. When the mass is brought to a quiet flux, it is run into moulds; and when removed therefrom, it is ready for use.

Another mode of proceeding is, to boil down the whole quantity of crude soda liquor; and during the boiling, when the liquor is brought to about the consistency of tar, to add the nitrate of soda, or other oxidising body; and, the evaporation being completed, flux the soda containing the sulphuret of sodium.

The patentee claims as his invention, the improvement in soda-making by fluxing the soda, containing sulphuret of sodium, with bodies possessing an oxydising power.

ON ARTIFICIAL MANURES.

BY JUSTUS LIEBIG.

TWENTY-FIVE years ago, when the manufacture of spa and mineral waters began, they met with violent opposition from the members of the faculty, as being deprived of all the good qualities of the natural ones—as wanting, in a certain *conditio, sine qua non* —in a *spiritus rector*, or vital power, which alone gave them any medicinal qualities. Those times have passed now—chemistry has demonstrated to a certainty what the constituents of those various waters are, and under what forms and compounds they are united in them. It has succeeded in combining them exactly in the same proportions, and in rendering them not only equal to the natural ones, but even more effective. Only from that time physicians were induced to connect certain effects on the human body with certain elements in the waters, and were enabled, by the light of science, to add more of this element, or more of that; nay, to apply, instead of the waters themselves, the one active element alone, as is, for instance, the case with iodine in indurations and struma. It is well known, that at this moment there are extensive manufactures of mineral waters in England, at Berlin, at Dresden, at Vienna, etc.

Now, I believe, that the same principle may be applied, partially, at least, to the use of manufactured manures, which, in England, has just been called into existence. Guano, that powerful manure, the efficacy of which, in a judicious application, has been clearly demonstrated by the testimony of the most intelligent farmers, cannot be supplied for a much longer period, because the rich stores in Chilli and Africa must be shortly exhausted. As it is only in very dry countries that it is found, we cannot expect to discover many more places containing it, and what are we *then* to do? My attention has often been directed to the question, whether, according to our experience, and the present state of science, a manure might not be composed which could replace the genuine guano in its effects, and whether I could not, by a series of experiments, point out a way of preparing one equal to it in all its chemical and physical properties? You are well aware that we know with certainty all the elements of the guano, as well as of the urine and solid fœces of men and animals. In like manner it seems to have verified the opinion which I have laid down in my work on agriculture, that the salts manufactured in the laboratory have the same effect on the growth of plants, if they are embodied to the fields, in the same forms in which the animals furnish them in their excrements. This must be evident to every one who knows, that to produce these compounds in the laboratory, the same agencies and means are made use of which are employed by nature. The fabrication of a manure, equal in its composition and effects to the solid and fluid excrements of animals and men, seems to me one of the most essential demands of our time—more especially for a country like England, in which, from various circumstances, a rational agriculture without a supply of manure, in some shape or other, *from without*, seems nearly impossible. Our reasoning will appear the more correct, if we remember how different are the results which have been obtained by the numerous analyses of the different sorts of guano—how little the farmer can depend upon producing from a given *quantity* a certain *effect*, as the latter naturally varies according to the composition of the former. There are scarcely any two samples in the market with the same composition—nay, not even similar. The following salts may be regarded as the essential constituents of a powerful manure applicable to all descriptions of soil:—

Earthy Phosphates. The most important of these is *phosphate of lime*, which occurs in nature as a mineral called *apatite*. It is the principal component in bones, which, it may be observed, have been found most efficacious if calcined, consequently deprived of their animal matter. The rapidity of the effects of phosphate of lime on the growth of plants depends upon its greater or lesser solubility. Its amount of glue (gelatine) diminishes this solubility if the soil is rich in vegetable matters, which furnish carbonic acid by their decomposition, and which acid is required for rendering the phosphate of lime soluble in water, and introducing it into the organism of the plants. In the calcined state the bones act sufficiently quickly; but in those soils in which this cause of solubility is wanting their action is slower. In my work I had recommended the addition of a certain quantity of sulphuric acid, both in order to render the bones more soluble, and to change the neutral phosphate of the bones into gypsum, and into a phosphate which contains more acid—super-phosphate of lime. I have been informed that this advice has been most extensively adopted, that the super-phosphate of lime has been found to be a most efficacious manure, and that it forms already a most important article of commerce. A second earthy phosphate, not less important, is the *phosphate of magnesia*, which it is well known enters in a still larger proportion than the *phosphate of lime* into the composition of the grain.

The *Alkaline Phosphates*, although not originally found in nature, are important elements of the seeds of grain, of peas, beans, &c. A rational farmer must provide them in sufficient quantities to those plants which require them for their development, from knowing that human excrements increase the produce of grain in a far greater proportion—because they *contain* alkaline phosphates—than the animal excrements, in which they *do not exist*.

The *Alkalies*—potash and soda—must be constituents of every rationally composed manure, because, by them the original fertile condition of the fields is preserved. A soil which contains the *alkalies* in too small a quantity is, perhaps, fertile for grain; but is not so necessarily so for turnips or potatoes, which require a great quantity of alkali. By supplying an alkaline manure, fallows, or the cultivation of those plants which are grown during the time of fallowing, becomes less necessary.

Sulphate of Potash is a constituent of all plants, although in small quantity, as well as *common salt* and *chloride of potassium* which are found in milk in rather a large proportion. The *salts of lime*, especially *gypsum*, are important nourishment for the leguminous plants. *Silica* is never wanting in all sorts of soils—it is a constituent of all rocks, by the decomposition of which all productive soils are formed, and the cerealia find it everywhere in sufficient quantity, and in a form capable of being taken up by the plants, *if the alkalies are provided*, wherever they are present in too small a quantity.

Salts of Ammonia.—It may be regarded as certain, that the nitrogen of the plants is derived either from the ammonia of the atmosphere, or from the manure which is provided in the shape of animal fluid and solid excrements, and that nitrogenous compounds exercise an effect on the growth of plants, only in so far as they give up their nitrogen in the form of ammonia during their decomposition and decay. We may, therefore, profitably replace all the nitrogenous substances with compounds of ammonia.

Decaying vegetable matters, which contain carbon, are useful to the fields in so far as they provide a source of carbonic acid; but they are quite dispensable in manure, if it be rationally combined, as the atmospheric air is an inexhaustible source of carbonic acid, from which the plants derive their carbon, *i.e.*, if in the manure, the mineral substances are provided which are necessary for the assimilation of the carbonic acid. These are the substances which *together* give fertility to the soil; but, although each of them may, under certain circumstances,—viz., where the soil is defective, or where it is not indifferent to the plant to take up one instead of the other, as, for, instance, may be the case with soda instead of potash,—increase the fertility, no *one* of them can be regarded as manure, according to the common meaning of the word, for the simple reason, that only *all of them in certain proportions*, will fulfil the purpose for which the common manure is applied. This purpose is the restoration, or an increase of the original fertility, and by manure, we must replace all the constituents of the plants which have been taken away in the harvest, or which are contained in the plants which we are desirous to cultivate.

What, then, are the constituents of the soil which we remove by the straw, seeds, tuberculous roots, stalks, &c., of our plants of culture? It is obvious that we must know these first, in order to *restore* them in sufficient quantities. To this we answer, by giving the analysis of the ashes of plants and their seeds. Hundred-weights of the ashes of the following plants contain—

	Beans.	Straw of Peas.	Potatoes.	Clover.	Ashes of Hay.
Alkaline carbonates	22·38	12·43	4·34	31·63	3·0
Carbonate of lime	39·50	47·81	43·68	41·61	6·9
Phosphate of lime	6·43	5·15	5·73	1·180	40·8
Phosphate of magnesia	6·66	4·37	7·82	0·91	....
Sulphate of potash or soda	12·40	10·15	....	2·23	8·84
Magnesia	....	....	....	....	21·8
Chloride of sodium or potassium	0·28	4·63	2·8	2·27	3·06
Phosphate of iron Phosphate of alumina, &c.	....	....	....	....	1·27

In these analyses silica has not been taken into account, as it is found in all soils, and need not be supplied. One hundred weight of the ashes of potatoes, and the seeds of the following plants, contain—

	Potatoes.	Wheat.	Beans (Vicia faba).
Alkaline phosphates	15.75	52.98	68.59
Phosphate of lime and magnesia	9.00	38.02	28.46
Phosphate of iron	0.20	0.67	0.00
Sulphate of potash	15.07	0.00	1.84
Carb. of potash and soda	51.70	0.00	0.00

What is wanting in the 100 of the above analyses is sand, coal, or loss. From these researches it appears, that for stalks and leaves we require other elements than for seeds. The former contain no alkaline phosphates, but they require for their development and growth a rich supply of alkaline carbonates and sulphates. On the other hand the carbonates are entirely wanting in the seeds, which, however, are very rich in phosphates. It is sufficiently obvious that a rational farmer must supply both, as well as all the others. If he supplies only phosphates, and does not restore the alkaline carbonates, his soil will become gradually barren—it will be exhausted in those necessary elements for the development of stalks and seeds, without which no formation of seed can be expected. If he supplies the alkalies, lime, and sulphates alone, in a given time he will get no more grain. All constituents of the manure, if they are supplied *alone*, have this great defect, that by them the soil is impoverished in other equally important substances. No *one* of itself can maintain the fertility. Keeping this in view, we may easily judge of the comparative value of artificial and natural manures, and all the various *arcana* which have been praised as *panaceas* for exhausted soils.

It is not less easy to understand, why the farmers have such different opinions on the relative value of the constituents of manures—why one, whose farm is rich in phosphates, produces an uncommon fertility by the application of nitrate of soda, or the supply of alkalies, while another does not see any favorable effect at all—why bones—phosphates of lime—produce in many fields wonders, and are not of the slightest benefit to others, which are deficient in alkalies or alkaline salts. From the composition of animal manures, it results with certainty, that by applying the latter—solid and fluid excrements of men and animals—we supply to the soil not one but all the elements which have been taken away in the harvest. Fertility is perfectly restored to the field by a corresponding supply of this manure, and it may be increased by it to a certain limit. This will be the more intelligible, if we compare the mineral elements of the urine of horses and cattle with the mineral elements of herbs, straw, roots, &c., of our cultivated plants. It will be found that in their quality they are perfectly identical.

	Urine of a Horse.	Of another.	Of Oxen.
Carbonate of lime	12·50	31·00	1·07
,, magnesia	9·46	13·07	6·93
,, potash	46·09	40·33	77·28
,, soda	10·33		
Sulphate of potash	13·34	9·02	13·30
Chloride of sodium	0·55	..	0·30

These salts in the urine of horses amount to nearly 4 per cent.: in that of oxen to 2½ per cent. of their weights. If we compare the composition of these different sorts of urine with the composition of the straw of peas, beans, and potatoes, of clover and hay,

it will at once be obvious that in stable dung we replace by the urine, the alkaline carbonates which we have removed in harvest. What in this urine is wanting in phosphates and carbonate of lime and phosphate of magnesia, forms the principal constituents of the solid excrements of animals: *both together*—solid excrements and urine—restore to the field its original composition, and thus a new generation of cultivated plants meet with the mineral ingredients necessary for their development. If we further compare the guano and the fæces of men with the composition of the animal urine, the analysis shows—*cf.* my book on agriculture—that both are entirely defective in *alkaline carbonates*—they contain phosphates and sulphates as well as chloride of sodium, but no free alkali—they contain phosphate of lime and phosphate of magnesia, in short, their elements are in *quality* identical with the important mineral elements of the seeds of wheat, peas, beans—*cf.* the analysis. The urine of swine is in its composition intermediate between the urine of man and horses.

Analysis of the Urine of Swine.

Carbonate of potash	12·1	The solid excrements of swine contain principally phosphate of lime.
Phosphate of soda	19·0	
Sulphate of soda	7·0	
Chloride of sodium Ditto of potassium	53·1	
Phosphate of lime Ditto of magnesia	8·8	
Traces of iron	—	

What the practical results of a knowledge of the composition of these manures are, is clear. If it were possible to provide our fields with the dung of swine in sufficient quantity, we would replace by it, in a soil which contains *silica* and *lime*, all the remaining elements of the plants—the field might be made fertile for all kinds of plants—we have in it not only alkaline phosphates, the principal elements of the seeds, but also alkaline carbonates, which are required by the leaves, stalks, and roots. This purpose cannot be attained, however, by manuring with guano or human excrements alone, but perfectly so by stable manure, from its containing alkaline carbonates. If I have said that stable manure contains the mineral elements of the nurture of the plants, exactly in a state and condition in which they are furnished by nature—that a field manured by it resembles the primitive state of America and Hungary, this assertion will not be found exaggerated. It is certain that stable dung contains no alkaline phosphates, but nature does not furnish these to the plants even in the most fertile soil, although we find them in large quantity in all the seeds of wild plants. It is obvious that, notwithstanding their absence from the soil, the phosphates are formed in the organism of the plants, and they are produced from the phosphate of lime and magnesia and the supplied alkalies, by an exchange of the elements of each. The alkalies are necessary for forming *alkaline phosphates*, which cannot originate in the phosphate of lime alone. Both together are present in stable dung. In human excrements and in guano the alkaline carbonates are entirely wanting. The practice of the farmer, in some places, of supplying to the field not pure guano, but a mixture of it with gypsum, shows clearly that the phosphates of alkaline bases are really formed in the organism of the plants from the phosphate of lime and magnesia, because this mixture (guano and gypsum) contains less phosphate of potash or soda than the guano itself; or, in certain proportions of gypsum no alkaline phosphates at all; the soluble phosphates in the guano decomposing the gypsum into phosphate of lime and magnesia, and into sulphate of potash. I am far from asserting that we should not provide the fields with alkaline phosphates; the excellent effect of the guano, and of the human excrements, is too well known to question it, and we perceive, from this fact, that plants are in this respect like domestic animals which, with a normal food, are healthy and strong, but do not fatten. On the contrary we know that if we prepare the food of these animals artificially, so as to render it more easily digested and assimilated, they are enabled to consume, in a given time, a greater quantity of it, by which all their parts increase in weight. The same happens with plants: if we give them their nourishment in a state most appropriated for assimilation; their capability to attract the gaseous elements from the atmosphere increases, and their development is accelerated. If we recollect that the favorable effect of guano upon our fields depends on its amount of *ammoniacal salts*, of *alkaline phosphates*,

and the *other mineral constituents* of the seeds, but that it is deficient in *alkalies*, the principal constituents of the *herbs*, *straw*, and *roots*, it is easily understood why the opinions of farmers on the value of guano, as a manure, are so very different. On a soil which is defective in alkalies, its effect is small; on a rich soil in them it increases the produce in a remarkable degree; but, as I have already observed, the continued application of guano must gradually diminish the fertility of our fields for a number of plants, because the elements of those organs, of the leaves, stalks, roots, &c., without which the plants cannot be developed, and cannot produce seeds, are taken off in the harvest without any restoration of them. I think it, therefore, certain, that the stable dung can replace the guano to a certain degree, but not *vice versâ*. A rational agriculturist, in using guano, cannot dispense with stable dung.

During my excursions in England I have repeatedly directed the attention of the agriculturists, as Messrs. Pusey and Miles will, perhaps, recollect, to the necessity of supplying the alkalies, and not merely the phosphates and other salts; by a partial supply the equilibrium of fertility is not restored, and if we supply guano alone we do not act wisely, because we consume our capital by rich interests, and leave to our children an exhausted soil.

And now, the principles above-mentioned must guide us in the manufacture of an artificial manure. If they are neglected—if the artificial manure is defective in one or two of the necessary ingredients—the farmer, in making use of it, will, in a very short time, discover the fact, by the injury he will have sustained.

In the manufacture of an artificial manure, it must be kept in view, that the application of stable dung, of human excrements, and of guano, is attended with great loss, in consequence of the too great solubility of their most efficacious constituents; and this must be prevented by artificial means. This is evident, if we remember those countries whence guano is derived. It is known that the collection and preservation of the excrements on the African islands, and the coasts of Peru and Chili, depend upon the scarcity of rain in those countries. The best sorts of guano contain, in fact, more than one-half of their weight of soluble salts, which, if exposed to the rain, are in exactly the same condition, as under similar conditions, a heap of *salt*. They dissolve in water, and are removed. Some months of rain would deprive those countries of all their riches. The remainder would have lost the greater part of its fertilising power. Such effects, however, take place upon the guano with which our fields are manured. Only a small portion of its efficacious salts produce the beneficial effect they are capable of doing, the greater part being carried off by the rain. The stable dung is, in this respect, in the same condition as guano; indeed, its principal compounds are already in a dissolved state, and, therefore, are carried off more easily than those of guano.

A covering for those places in which stable dung is preserved, in order to shelter it from the effects of the rain, has been regarded in Germany as essential for preserving its manuring power. In consequence of the experience, that the soluble elements of stable dung are the most efficacious, it has, in some cases, been drawn out with water, and it has been found advantageous to carry *only this fluid* to the fields. I need only refer to the foregoing analyses of the urine of animals, in order to see upon *which elements* of it this effect depends.

The reason why, in certain years, the influence of the best and most plentiful manuring is scarcely perceptible, is, that during the moist and rainy springs and summers, the *phosphates* and *other salts with alkaline bases*, as also the *soluble ammoniacal salts* are entirely or partly removed. A great amount of rain and moisture removes, in the greatest quantity, the very substances which are most indispensable to the plants at the time that they begin to form and mature seeds. The system of draining, which, of late, has been so extensively followed in England, brings the land into the state of a great filter, through which the soluble alkalies are *drawn off* in consequence of the percolation of rain, and it must, therefore, become more deficient in its *soluble* efficacious elements.

Attentive farmers must have observed that after a certain time the quality of the grain on land laid dry according to this principle, deteriorates; that the produce of grain bears no due proportion to the produce of straw.

What is more evident, after these remarks, than that intelligent farmers must strive to give to the soil the manuring substances in such a state, as to render possible their acting favorably on the plants during the whole time of their growth? Art must find out the means of reducing the solubility of the manuring substances to a certain limit, in a word, of bringing them into the same state in which they exist in a most fertile virgin soil, and in which they can be best assimilated by the virgin plants.

The attention which I have paid to this subject has been crowned with success. I

have succeeded in combining the efficacious elements of manure in such a manner as that they will not be washed away, and thus their efficacy will be doubled. Owing to this the injurious consequences of the present system of draining are removed, agriculture is placed upon as certain principles as well arranged manufactories, and instead of the uncertainty of mere empiricism, the operations of agriculture may be carried on with security, and in place of waiting the results of our labors with anxiety and doubt, our minds will be filled with patience and confidence.

OBSERVATIONS ON THE DISEASE IN POTATOES.*

BY M. PAYEN.

The fields of potatoes are at the present time attacked in Germany, Belgium, and France, by a kind of disease which destroys a portion of the crops.

Some persons attribute these fatal effects to a microscopic fungus; others believe that the cryptogamic vegetation was secondary, and one of the natural consequences of a change in the organisation.

The difference of opinion is greater, I imagine, as to the principal effects of the disease in the tubers; not having an opportunity of consulting the original papers which have been promulgated on the subject, I shall abstain from giving the names of the authors of these varying observations, for fear of attributing to them opinions which they have not given, but ready to return to the subject when I shall be better informed.

I have employed myself, on my side, in studying the phenomenon as soon as it was noticed in the neighbourhood of Paris, in a communication made by M. Elizée Lefebvre to the Central Society of Agriculture, and by means of specimens of the diseased roots selected from his crops.

M. Rayer, our colleague, had observed the disease at Audilly, Boulogne, Epinay, Enghieu, and Ormesson, and has been kind enough to send me specimens from two or three localities. I have procured some myself from MM. Pourette, nurseryman, and Delamarre, a proprietor of land at Brunoy, where the disease attacked large fields at the same time, but here and there sparing crops of the same description.

The following are the results of the observations and experiments I have been able to make, up to the present time.

In all cases I have seen the leaves and stems attacked before the tubers themselves; it, therefore, appears to me that the disease is transmitted from the stem, above the ground, to the tubers.

This appears more evident, when we perceive that the peculiar change that takes place in the tubers is visible at, and extends from, those points that are nearest to the stems, round the tuber, beneath the epidermis, and then by degrees it attacks the cortical layer, advancing from the periphery towards the centre.

It often happens that the portion representing the rind, and which is more abundant in fecula than the other parts, is attacked through its whole substance, while the medullary portion still remains sound.

More rarely the disease advances towards the centre, without being propagated through the greatest portion of the rind, this, however, happens more particularly in kidney than in round-shaped potatoes.

After minutely studying the effects of this disease, I think I have ascertained its origin with precision, and in accordance, on two principal points, with the observations of M. Decaisne.

If you cut one of the tubers in a plane, passing along its axis, or across its centre, you may recognise, with the naked eye, the diseased portions, by the reddish color they have acquired; the strong fungus smell which they give out reminds one of the peculiar odor that, in 1843, arose from the army bread, which was so rapidly spoiled by an extraordinary cryptogamic vegetation.

Wherever these appearances manifested themselves, the substance was softened, and was separated with greater ease than the sound parts that were white and solid.

Very thin slices, when examined by the microscope, enabled you to observe at the limits of the progressive disease, a liquid, slightly clouded with yellow, which insinuated itself into the intercellular interstices; this liquid gradually enveloped nearly the whole periphery of each of the cells; in the parts most energetically attacked, sometimes it increased n quantity and sometimes destroyed the adherence of the cells to each other, a fact that explains the easy disintegration of the tissues in those spots.

The corpuscules that were carried along by the yellow liquid formed darker colored granulations on the sides of the cells; and several chemical re-actions enable us to compare them with sporules of extreme tenuity.

A great number of the cells that were carried away by the liquid preserved their grains of fecula untouched.

When the separation of the cells has made a certain progress in the mass, the tissue

* *Comptes Rendus.*

becomes pulpy, half fluid, and it is sufficient to touch it with the rounded end of a tube to take up enough for a microscopic observation; when in this state of disintegration, the substance is whitish, or of a brownish tinge, more or less deep. Almost all the cells are torn, and at times so much destroyed as merely to leave their large ragged membranes visible in the angles where the adherence was still maintained between some of the cells; frequently, also, were seen, at this time, myriads of animalcules about the hundredth part of a millimetre in length, and the tenth of that measurement in width, moving about in the most active manner, and attacking or agitating the minute fragments of the organization. But there was a remarkable fact which proves the perispherical and peculiar change in the cells, when attacked to this extent; the grains of fecula are still untouched, their substance is insoluble even in water heated to 50°, only they are more easily divided mechanically and operated on by iodine, sulphuric acid, and diastase, like fecula in its normal state: still a portion of the amygdaline substance, feebly combined, has disappeared.

How is it, then, that many persons have imagined they perceived a general dissolution of the amygdaline substance, when they observed the cells empty, and attributed these effects to the disease of the tubers?

I think I have discovered the cause of the error. For instance, some of the tubers may be observed exhibiting a similar state of emptiness, but these generally are free from the symptoms now under consideration. They are also as frequently found on roots free from the disease as on those that are attacked by it. They are, in fact, tubers whose growth had been arrested, and in which the vegetation of the stems and leaves had exhausted the elements of nutrition and development, as in the case of the parent potato.*

The fecula being to a great extent untouched in the diseased tubers, we might believe it could be easily extracted by the usual processes. This, however, is not the case, for a great number of the cells adhering more or less to each other, as in the case of frost-bitten potatoes, are separated from each other by the action of the rasp, without opening, and the fecula remains thus enveloped along with them on the sieve.

Another method, therefore, must be resorted to, by extracting part of the fecula, and afterwards the produce of the transformation, by dissolving the remainder.

The proceeding offers no difficulty, for the sides of the cells have become more easily permeable than in their normal state, the granulous pulp that remains on the sieve being worked, ultimately becomes hydrated, and attacks the fecula, either by water and *diastase*, at the temperature of +75 degrees, or by water acidified by means of sulphuric acid, at the temperature of 100 degrees. I made several trials that satisfied me on this point. The syrup formed, passes through the cellular membranes; these may be separated by filtration. Collect the saccharine liquid next, or it may be used at once by submitting it to spirituous fermentation, and then to distillation.

CONCLUSIONS.

Several ideas, immediately applicable, are deducible from the facts I have just mentioned.

In the case of most of the tubers slightly affected, it will be sufficient to cut off a paring more or less thick, to remove the diseased parts.

It can be easily ascertained if the more deeply seated parts are sound, by cutting each of the tubers into four parts.

Many observations induce a belief that those potatoes which are little affected if boiled, taking care to throw away the water in which they were cooked, may be used as food for animals; but it would be prudent to make an experiment, on several, in the first instance, and in every instance not to supply them exclusively with this food, unless, indeed, it were for an experiment, and for a few days.

As for the tubers in which the disease is in an advanced state, they certainly can be made available by rasping them to pieces, washing the pulp on the sieve, extracting the fecula from the water in the usual manner, submitting the washed pulp immediately to the process of saccharification, or drying it, for the purpose of disposing of it to the manufacturers who are engaged in similar processes

Even those potatoes which have changed so rapidly as to have become entirely disintegrated may be treated in the same manner.

But it would be improper to wait for fresh spontaneous changes, for the attack of insects or certain larvæ would certainly bring about the disappearance of the greater portion of the fecula.

More important questions are, no doubt, attached to the precautions to be taken in

* M. Rayer and myself observed in the tissue carried off by the reddish fluid, cells filled with fecula, and surrounded by other cells, in which the grains of fecula were reduced in number, or had even disappeared.

respect of the next crops, with regard to this subject, conjectures alone appear to be allowable, perhaps they may not be useless.

Would it not be proper to plant the potatoes on every farm inland, as far as possible distant from that on which they were grown this year?

Many facts induce us to believe that the forward sorts, whose term of useful vegetation should be accelerated as much as possible, might escape the disease.

An active surveillance as they approached maturity, would allow the detection of the first signs of disease, at certain parts of the stems; it might be useful to cut these off, and to burn them immediately to preserve the rest, so as to make use of the first tubers before the attack of the disease.

In all cases it would be extremely desirable that cultivators should note down their observations in detail, their attempts at steeping the roots, particular modes of cultivation, &c. That they should take care to transmit these documents to the local agricultural associations, and thus assist in forming a complete history of the disease, and the means of ultimately ameliorating its deplorable effects.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

DEEP VIOLET COLOR OF THE URINE FOLLOWING THE EMPLOYMENT OF LIME-WATER.*

Dr. Velseu relates the case of an old man affected with chronic inflammation of the bladder, who made use of lime-water mixed with milk, upon which his urine became turbid, highly fœtid, and of a deep violet color, staining the utensil into which it was passed. Upon suspending the use of the lime-water for a single day, the urine lost its peculiar violet tint. The chemical analysis of the urine was not undertaken.

ON THE PREPARATION OF BALSAM OF TOLU.†

BY M. ARTLIER.

Within a very short space of time various methods of preparing balsam of tolu have been published in different journals. I take the liberty, sir, to communicate to you the manner in which I prepare this medicament.

I take of balsam of tolu four grammes, dissolve it in 2·8 of alcohol at 33° C., when the solution is filtered I incorporate it with a killogramme of syrup of sugar, by shaking the mixture violently, and after a few hours' time I filter it through paper.

By this method I always obtain a perfectly limpid syrup, the smell and taste of which leave you nothing to desire.

This syrup must not be confounded with the syrup of balsam of tolu of the French pharmacopœia, to which it bears no resemblance.

* *Heller's Archiv.*

† *Journal de Chimie*, Sept. 1845.

POISONING BY HYDROCYANIC ACID.—RECOVERY.*

BY EDWARD BISHOP, ESQ.

As cases of poisoning by hydrocyanic acid have so frequently of late been brought before the profession and the public, and as the effects of the acid upon the human subject, when taken in small doses, are so little known, I think it the duty of every medical man to place on record any case of this kind that comes under his observation. With this view, I send to the *Lancet* the notes of a case which occurred in my practice a few weeks ago. It is extremely interesting, as regards the symptoms, the quantity of acid taken, the length of time during which the patient remained sensible after swallowing it, and the effects of the remedies used in the treatment of the case.

On the 30th of June, about half-past 11 at night, I was sent for, in great haste, to attend Mr. W. J——, a book-keeper in a large iron-forge establishment who was said to have taken poison. I put some sulphate of zinc in my pocket, and immediately went to the patient's house, a distance of only a few hundred yards.

On my arrival in his bed-room, which is on the ground floor, I found him undressed, and lying in bed quite senseless, the countenance of a ghastly pale appearance; his face, however, looking much swollen, and covered with large drops of perspiration; the breathing exceedingly slow, laborious, and accompanied with a hissing noise; he had the corner of the sheet between his teeth, along the sides of which, at each expiration, issued

* *Lancet*, Sept. 20, 1845.

a large quantity of thick white foam; the eyes wide open, fixed, and glazed; the pupils widely dilated; the pulse slow, and scarcely perceptible; the heart's action very weak; the whole muscular system in a state of rigidity; the jaws tightly closed; the arms bent and the hands clenched; the legs straight and stiff; the efforts of respiration gradually becoming more laborious. In this dangerous state of affairs, I asked the patient's wife, who was standing in her night-dress by the side of the bed, if she had any idea what kind of poison he had taken. She said she had not, but that she suspected he had poured something out of a small bottle, which she put into my hands; it was labelled *prussic acid.* I immediately ordered cold water to be brought, and in the meantime attempted to remove the sheet from his mouth, in order to give him a dose of sulphate of zinc; in this, however, I was unsuccessful, as the jaws were so tight I could not open them in the least. I then prepared him for the cold douche, by tearing off his shirt and flannel waistcoat, and directing two assistants to hold him off the side of the bed. A bucket of cold water was soon brought, and with a vessel holding about a quart, I commenced pouring the water upon him, so that it might flow over his face, chest, and abdomen.

It produced a decided shock; he gave a start, after which the muscular system became relaxed; the sheet dropped from his teeth, and the hands opened; respiration was more quickly performed, and he sobbed violently. I applied it a second time; he again started, sobbed, began to roll his eyes about and utter the most agonising cries. He evidently knew me, and in a second or two said, "God bless you, Mr. Bishop!" and begged of me not to douche him again; but as respiration was still slow, and the pulse weak, I continued, and found that after each application of the water he breathed quicker, and the pulse also became quicker and stronger. I now dissolved two scruples of the sulphate of zinc in a tea-cupful of hot water, and asked him to drink it, which he did very well.

As soon as he had swallowed the emetic, ammonia, which I had sent for on ascertaining that hydrocyanic acid had been taken, was brought to me; it was very strong, and I applied it to the nostrils, and rubbed the temples and palms of the hands with it. The pulse rose, the face became red, and at each inspiration of the ammonia, the patient said, "Oh, I am better now; this has done me good;" indeed, he became very loquacious, and it was with difficulty he could be restrained. In about ten minutes, vomiting took place, and the ejected matters smelt so strongly of prussic acid, that it was almost impossible to remain near it.

The pulse was now 90, and full; the countenance bloated, with redness of face; and he occasionally complained of crampy pain in his arms and legs, and said, "It is coming on again."

I continued the application of the ammonia, and every minute or two caused him to inhale it.

A vein was next opened in the arm, and sixteen or eighteen ounces of blood extracted, *pleno rivo.* It was of a dark color, and I thought at the time emitted the odor of prussic acid. Afterwards I was convinced this was an error, and that it arose from the room being so impregnated with the smell from the vomited matter. He occasionally complained of cramp in different parts of his body, but no other unfavorable symptoms followed.

About half-past twelve, an hour after I had been called in, I was told that Mr. Nunneley (who is an eminent practitioner in Leeds, and well known to the medical profession) was in the neighborhood, having been summoned in haste to one of his patients. I went and desired him to see my patient, which he did, and he took great interest in the case, having since reported it in the *Provincial Medical and Surgical Journal*, for August 13th. Mr. Nunneley has had some of the acid analysed, and the result is, that it contains only about three and a quarter per cent. of real anhydrous prussic acid, although sold for Scheele's. He has also had the blood analysed, but no traces of the acid could be discovered.

The patient is about forty years of age, of a plethoric habit of body, and much given to intoxication; and although not tipsy at the time of taking the poison, he had taken during the day three pints of beer and one glass of brandy and water. He has once before attempted to destroy himself, and for some time had been sulky, and when spoken to, peevish; and his wife says, when he came home on the night in question, he was in a very bad temper: she was in bed, but got up and followed him into an adjoining room, as she thought he was going to cut his throat; but he told her to go to bed, and that he would neither harm himself nor any one else. He then undressed, went round to the other side of the bed, where his daughter, a girl of seven years of age, lay, and embracing her, said, "You will very soon have no father." He then returned, and poured some common gin into a wine-glass, emptied the prussic acid into it, corked the bottle, laid it down on the mantel-piece, and then drank off the contents of the glass, and proceeded to get into bed; he threw down the bed-clothes, got in, and covered himself. His wife thought he rather seemed to stagger and fall on to one shoulder as he was getting

into bed, but under ordinary circumstances she would not have noticed anything peculiar in his manner. He now said to his wife, "Well, Bessy, I have taken something, and it will be all over with me in a few minutes."

This, along with her previous suspicions, alarmed her very much, and she immediately jumped out of bed and ran screaming to the door. When she got out of bed, the child was frightened and crept to her father, as he says, and said, "Don't you leave me, father." And he replied, putting his arms round her, "No, my dear, I will not leave you."

He can remember nothing else until the water was dashed upon him. When his wife got to the door, a man was passing by, who immediately came to her assistance; and he says, that when he saw him, he was lying quite senseless, and black in the face; so that, as near as can be told, he remained sensible about two minutes after swallowing the poison.

There is no doubt the man meditated suicide when he purchased the acid, although he declares his intention was to experiment on some cats and rabbits, which he had a desire to do since reading Tawell's trial, in which he took great interest. He asked the druggist if he had a sufficient quantity to kill a child, and he replied, "Yes, as soon as that;" clapping his hands together.

The acid was not measured, but both the patient and the druggist, unknown to each other, marked the same place on the bottle (which holds two drachms), as the point to which the acid reached—viz., the lower edge of the label. On measuring this, I found it to contain exactly forty minims.

There are several very interesting and important particulars in this case, not the least of which is the appearance of the countenance, which was quite pale. In all the cases I remember to have seen recorded, just the contrary occurred; but it is worthy of notice, that when the messenger who came for me was called in by the patient's wife, he was at that time almost black in the face; this was two or three minutes after taking the poison. I saw him five or six minutes after this, and in the meantime the change must have taken place; but then when I saw him, although the face was pale, it seemed much bloated and swollen. The length of time he remained sensible is also another important point. This, from the patient's own account, must have been at least two minutes. He says, as soon as he swallowed the acid, he felt a dizziness, and he thought it would be over with him in a few minutes. He perceived himself becoming stiff, and his jaws getting tighter and tighter, and he put the sheet between his teeth "that he might have something to hold by." He had no pain, but gradually became senseless; nor was he aware of anything until the water was dashed upon him, when the painful sensation it occasioned was beyond expression.

Again: the amount of acid taken is as large, if not larger, than any recorded as having been recovered from. It is well known that Scheele's acid ought to contain 5·16 per cent. of real anhydrous prussic acid, whereas some which I got from the druggist, out of the same bottle, contained only 3¼ per cent. Forty minims of this would be equal to nearly *a grain and a half* of real acid.

To ascertain the effects of the same preparation the patient took, it has been made the subject of experiment both by Mr. Nunneley and myself. Mr. Nunneley found that twenty minims were sufficient to destroy dogs. A small, half-grown mongrel was dead in two minutes and a half after swallowing twenty minims, and a full-sized terrier bitch in one minute, after swallowing the same quantity. I gave forty minims to a bull-terrier, a large animal, and he died in thirty-five seconds. Twenty minims destroyed a kitten in a minute and a half, but another lived three minutes after the same dose. Since performing these experiments, it was suggested to me by my friend, Mr. Johnson, surgeon, of Bromley, that the gin might have had some effect in neutralising the acid. In order to put this to the test, I intend, the first opportunity that presents itself, to mix some prussic acid with common gin, and to observe its effects upon animals.

TREATMENT OF CHILBLAINS WITH FUSED NITRATE OF SILVER.*

Dr. Lauer, a military surgeon of Berlin, recommends fused nitrate of silver as a most valuable remedy for chilblains. His mode of treatment is as follows:—Simple chilblains are first moistened, and then slightly touched over their surface by the nitrate of silver. Broken chilblains are strongly cauterised, and the humid places afterwards allowed to dry in the air. As soon as pus has gathered under the cuticle, it is removed by pressure, and then with a pointed piece of nitrate, the spot under the cuticle is again cauterised. The ulcer may be considered as cured the moment the cuticle falls off. Sometimes deep and large chilblains are cured by a single cauterisation; sometimes two or three, but rarely are more applications required. The unpleasant sensation of itching soon disappears after the cauterisation.

* *Med. Zeitung.*

FALLACY OF DR. BIRKBECK NEVINS' TEST FOR ASCERTAINING THE PURITY OF DISULPHATE OF QUININE.*

BY G. M. MOWBRAY.

THE following test has been suggested by Dr. Birkbeck Nevins, as appropriate for readily ascertaining the purity of disulphate of quinine.

"To one or two grains of the suspected salt† add three or four drops of sulphuric acid in a white evaporating dish, and twice as many drops of water; if the salt contains either starch or fatty matters they will remain, whilst if they are absent the whole will be dissolved. Let heat be next applied to the solution, and as it becomes concentrated, the acid will char any sugar which may be present, which will be indicated by a black stain round the edge of the solution, and the whole will speedily assume the same color."

Allow me to submit, that this test is valueless, and for the following reasons:—Dr. Nevins appears to have overlooked a fact well known to chemists whose investigations have been directed to organic compounds, that salts may be readily recognised as belonging either to the organic or inorganic class, by heating on platina: if the compound under examination, after heating, yield a carbonaceous residue, then it belongs to the former class; if a whitish ash be left after ignition, then an inorganic compound has been acted upon. Now, Dr. Nevins directs us to add sulphuric acid to the disulphate; the effect of this is to convert the salt into the soluble sulphate, and on the application of heat, this soluble sulphate, in common with all organic salts, is decomposed, yielding a carbonaceous residue.

Could Dr. Nevins have shown, that which is opposed to all experimental results with organic compounds, that in the presence of sulphuric acid quinine is not readily carbonized—and the reverse of this is the fact, as may readily be ascertained by heating a crystal of the soluble sulphate by the side of a sample of quinine purposely adulterated with sugar or gum—his test might be so far admissible; but as Dr. Nevins has not shown this, and it cannot be shown withal, therefore his test is fallacious.

* *Medical Gazette*, Sept. 19, 1845.

† If the acid be added first, the quinine will not be dissolved by the subsequent addition of water: the water should first be added, and then the acid, as many dispensing chemists have found to their cost.

CITRATE OF IRON AND DISULPHATE OF QUININE IN CHLOROSIS AND OTHER ANÆMIC STATES.*

BY DR. MEIGS.

I HAVE made most advantageous use of a citrate of iron conjoined with sulphate of quina, as in the following formula, which I subjoin as a very convenient and successful one in the disorders dependent upon anæmia:—

Take of citrate of iron 2 drachms; sulphate of quina ½ drachm; water 1 fluid ounce.

Mix, and direct from 20 to 30 drops for the dose, in syrup and water. Agreeably to M. Raciborski's method, I advise the patient to take the draught after each meal, within half an hour of the breakfast, dinner, or supper, so that it may be carried with the chyme along the course of the bowels. The addition of the quina to the ounce of water renders the citrate and the sulphate perfectly soluble, as does also the addition of a few drops of ammonia to a solution of the citrate alone; without some addition, a part of the citrate of iron is not dissolved.

OBSERVATIONS ON THE OIL OF CROTON TIGLIUM, EMPLOYED IN FRICTIONS.

BY M. LEPAGE.

IN the last number of the *Journal de Pharmacie*, under the head of "Remarkable Property of Oil of Croton Tiglium," Doctor Ern. Boudet has added a note respecting the phenomenon he frequently had occasion to observe, when he employed that medicine in frictions. The following is an analogous fact, which I observed about six months' since, and which well confirms the facts related by M. Boudet:—M. B., my colleague and friend, professor at the college, had been affected for some time with an irritation in the chest, which ordinary measures were unable to relieve. The physician then advised him to employ friction on the pit of his stomach, using a few drops of croton oil; the first friction took place in the evening, and the morning after, what was the surprise of M. B., when he perceived that his organs of generation (principally the penis and scrotum) were covered with a kind of erysipelas, that caused him much pain for several days, and yet, in this case, M. B. had taken the precaution of not touching the above-mentioned parts with his fingers impregnated with the oil.

* *Medical Gazette*, Sept. 19, 1845.

TO ASCERTAIN THE PURITY OF MYRRH.*

BY M. BIGHINI.

LET it be finely pulverised and saturated for a quarter of an hour, with an equal weight of sal-ammoniac in powder, then add gradually fifteen times its weight of water. If the mixed powder dissolve quickly and entirely, the myrrh is pure.

MANNA.†

BY A. LEUCHTWEISS.

MANNA has been examined by Fourcroy and Vauquelin, Thénard, and Buchholz. The latter found in the best manna:—

Mannite	60
Mucous sugar and colouring matter	5·5
Gummy extractive	4·8
Gum	1·5
A glutinous matter	0·2
Water	30

Thénard describes manna as containing mannite, 10 per cent. of fermentable sugar, and a nauseous uncrystallisable substance; the latter may be separated by means of ether. To obtain pure *mannite*, the sugar must be separated by fermentation, the filtered solution evaporated, the residue dissolved in boiling alcohol, of 0·833 specific gravity, when the mannite separates on cooling. By washing with cold spirit, it is obtained beautifully pure and white. Manna yields in this way from 32 to 42 per cent. of mannite.

The analysis of three specimens gave:

	I.	II.	III.
Water	11·6	13	11·1
Insoluble constituents (impurities)	0·4	0·9	3·2
Sugar	9·1	10·3	15
Mannite	42·6	37·6	32
Mucilage resin, an acid and nitrogenous matter	40	40·8	42·1
Ash	1·3	1·9	1·9
	100	100	100

The ashes of manna consist of potash, lime, magnesia, peroxide, of iron; phosphoric, muriatic, sulphuric acids, in minute quantities; and alumina and silica, probably from impurities.

The mucilage in manna is the same as ordinary mucilage. The sugar is crystallisable grape sugar. The resin and acid is soluble in ether.

* *Journal de Chem. Med.*, and *Lancet*.

† Liebig's *Annalen*.

A NEW CAUSTIC FORMED BY THE MIXTURE OF SAFRAN (PEROXIDE OF IRON) WITH SULPHURIC ACID.

BY M. VELPEAU.

DURING a series of experiments, to which he had for some time devoted himself, on the action of various powerful caustics, M. Velpeau at length arrived at sulphuric acid, solidified by safran, a combination that appeared to have the most happy results. To concentrate the sulphuric acid under the form of a ductile paste, or rather a soft pommade, not capable of extending beyond the limits he had traced out, he had to make a host of attempts and trials; it was necessary to discover a body or bodies which, combined with the liquid mineral, could form a concrete, without depriving it of its caustic properties. Asbestos, charcoal, flour, and various other substances failed. At length he came to safran, which incorporates easily, and in sufficient quantity to make a pommade of a proper consistence. This substance being carbonised by the acid, a paste of a beautiful black color is the result, reminding one of China ink or dried blacking. This paste is placed in a china vessel; the surgeon takes it up with a spatula and spreads it over the ailing spot as he would soft cerate; he forms a layer from two to four millimetres in thickness, more or less, makes the edges round, and circumscribes the limits to the actual seat of the disease, leaving it exposed to the air for some time until it is dry. A crust is soon formed, and it is then covered with a compress and bandage. The caustic that remains in the vessel cannot be preserved for any length of time. The sulphuric acid attracting powerfully the humidity of the air; but that which has been applied to the flesh forms a hard crust, and sounds, when struck, like charcoal, perfectly dry, bounded by the limits to which it has been applied, and of a depth equal to the thickness of the layer that was applied.

The slough began to detach itself from the eighth to the tenth day in the case of an individual who had submitted to its limited application. In the instance of a patient who had a cancer, and in whose case 100 grammes of the caustic pommade had been used, it was noticed with satisfaction that no symptom of re-absorption took place, and that, in addition, the caustic had the effect of completely removing the disgusting smell the cancer had hitherto possessed, and with which it infected the sick

person and those near him. The cauterized tissues even gave out an odor rather agreeable than fetid. While waiting until additional instances shall enable us better to appreciate the value of the new agent, we ought to direct attention to the three important conditions it offers: namely, 1st, the exact circumscription of its action to the limits marked out by the pommade; 2nd, the quick separation of the slough; and 3rd, the absence of serious re-absorption.

PHYSIOLOGICAL EFFECTS OF INHALING TURPENTINE.*

In experimenting upon turpentine, M. Bouchardat was instantly exposed to the vapour in his laboratory, and for five or six six hours was little incommoded, but at night he became sleepless and agitated, with a hot skin, and his pulse rose from 65 to 86, whilst his urine gave pain on emission, and had the characteristic turpentine odour. On the following morning he experienced great langour, heaviness, pain in the loins, and incapacity for work, continuing two or three days, and he was obliged, by these symptoms, to suspend his inquiries several times. M. Bouchardat thinks the turpentine is the cause of painter's colic, and that it should be ranked amongst the most virulent poisons.

ON THE USE OF CROTON OIL IN THE TREATMENT OF ASCITES.†

BY G. F. STICKINGS, ESQ.

M. T., 40 years of age, the mother of a large family: had been ill two months prior to my attendance; the abdomen had increased in size rapidly, and contained a great quantity of fluid. She was laboring under the usual symptoms consequent upon distension. I adopted the usual course of treatment in such cases; no benefit, however, resulted from it. Believing nothing short of an operation would remove the fluid, I suggested the propriety of her submitting to it, but she would not consent although repeatedly urged. At the end of three months from my first visit, my patient, finding that all the symptoms became much more urgent and distressing, at length submitted to be tapped. The operation was performed without delay, and I drew off twenty-five pints of fluid of a deep green color. After its removal I made a very careful examination, but could discover no organic disease existing, and could not arrive at any satisfactory conclusion as to the cause of the effusion. I immediately adopted an active course of treatment after emptying the abdominal cavity, but was disappointed in finding that effusion again took place rapidly; for at the end of a week the abdomen was nearly as much distended as before the operation. She was subject to obstinate constipation, and I had recourse to croton oil, one drop of which I gave in a pill three times a day; and after a few successive doses I was much gratified to find that it had not only produced a decidedly beneficial effect on the bowels, but on the kidneys also. At my next visit (two days after) I found that the fluid had much diminished in quantity. Seeing that so favorable a change took place in so short a period, and ascribing it entirely to the croton oil, I continued it in the same doses daily for a month, at the end of which time all traces of the disease had completely vanished, and my patient has remained well up to the present period (now a year since), without showing any symptoms of a dropsical character. It may be worthy of remark, that the daily use of the croton oil in this case did not produce a drastic purgative effect, but simply kept up a steady action the bowels and kidneys. I have had no opportunity of testing its value in similar cases, none having fallen under my notice. Perhaps it may be well to observe, that I attended the daughter of the above female, a girl about 15 years of age, a few months before, with ascites, which yielded very readily to appropriate treatment, but had no effect on the mother.

RAPID DEATH FROM OXALIC ACID.*

Dr. Ogilvy, of Coventry, has communicated to the *Lancet* a case of poisoning by oxalic acid, in which it is probable that death took place within *three minutes* after the poison had been swallowed. The sister of the deceased had been absent from the room about that period, and on her return found her dying. The quantity of poison taken could not be determined. The only other remarkable circumstance in the case was, that the coats of the stomach were so softened, that on an attempt being made to remove the organ, they were lacerated by the weight of the contents. This was probably a chemical action post-mortem. The intestines and left lobe of the liver were also found softened, as if by transudation. We believe this to be the most rapidly fatal case on record.

* *Comptes Rendus*, June 30, and *Lancet*.

† *Medical Gazette*, Sept. 26, 1845.

* *Medical Gazette*.

MODE OF PREPARING EPISPASTIC PAPER.*

BY WISLIN.

The great use made at the present day of epispastic paper, in the healing of blisters, the good effects that have arisen from it, in addition to the facility of its application, have brought this preparation into great favor; still, as the mode of manufacturing it was unknown, it remained a monopoly for a few of the pharmacopolists of the capital. I am only speaking here of the method employed for spreading the composition on the paper, not of the composition itself, most books of instruction containing several formulæ.

Every treatise on pharmacy, even the most recent, direct the employment of the common spatula, an impossibility when you consider the consistence of the matters employed, which will always be, according to the temperature, either too fluid or too solid, and they can never preserve their fluidity like various compositions employed in the making of plasters.

As we know that all these epispastic preparations have for their base hog's-lard and bees'-wax, a substance which has never more than middling consistence, and passes, without transition, from excessive fluidity to a consistence too solid to be spread by the spatula. On this account, after having vainly attempted this method, I proceeded in search of another, and the following is a plan by which I was successful:—

After having prepared a number of strips of fine white paper, not much sized, 6 centimetres 3 millimetres in width, and 45 centimetres in length, I melted the epispastic composition in a plate, placed above a vessel of boiling water, I took the extremity of one of the bands of paper in the left hand, while, with the other hand, I held the other end of the paper raised in such a manner that described the arc of a circle; then I brought the under side of the paper close to the composition, and made it glide rapidly over the surface, taking particular care that the composition, which ought only to cover one side of the paper, does not touch the other. A little practice enables one to avoid this easily.

While the paper, guided by the left hand, passes over the surface of the composition, the right hand follows the movement, and lowers the other extremity when it arrives at the end; the paper, still supported by the left hand, finds itself thus placed horizontally above the plate, in which position it is held until it grows cold, the work of an instant only.

As fast as the papers are prepared they are ranged on a table by the side of each other, but in a few minutes afterwards they may be placed on the top of each other, without fear of their adhering. They are then cut up with a pair of scissors, and each strip of paper, of the dimensions we have described will just make 5 pieces.

By varying the temperature you can change this paper at your will with more or less of the composition; so that, when you wish for a thin layer, you take care to keep the water boiling; if, on the contrary, you require a thicker layer, the heat must be below 100° C., which can be easily managed by interposing a plate between the boiling water and that which holds the composition; if it be required to be still more loaded you pass it a second time over the composition.

* *Journal de Chemie*, Sept. 1845.

EMPLOYMENT OF THE YEAST OF BEER IN BURNS.

BY DR. SZERLECKI.

M. Szerlecki, in consequence of observations made in his private practice, points out the employment of yeast of beer as the most simple and efficacious method possessed by medicine to counteract the effect of burns. He spreads a layer of yeast upon cloth, and with it he covers the burnt surfaces, and causes the applications to be renewed as soon as they become dry. According to him, the action of this medicament is much more efficacious if resorted to immediately after the occurrence of the burning.

ON TINCTURE OF MATICO IN HÆMATEMESIS.*

BY G. F. STICKINGS, ESQ.

About three months ago I received a hasty message to see a female, æt. 66, the mother of a very large family, who was supposed to be dying from having just ruptured a blood-vessel, and bleeding profusely from the lungs. On my arrival it proved to be a case of hæmatemesis, and the quantity of blood vomited exceeded two quarts of a dark colour, and grumous. I found the woman just recovering from a fit of fainting; she was exceedingly pale, and the features much contracted. The patient (whose general health was bad) had felt more than usually unwell for three days prior to the attack of

* *Medical Gazette.*

hæmorrhage, with the usual preliminary symptoms of hæmatemesis. Having heard much of the value of matico in cases of hæmorrhage, and thinking this to be a favorable case to test its value as a styptic, I had recourse to it. A tea-spoonful of the tincture was administered every three hours in a glass of water. At my visit the following morning I learned that the patient had had another fit of vomiting shortly after I had left her the previous evening, before taking the medicine. In this attack at least a quart of blood of the same character as the former had been discharged; and once during the night the hæmorrhage returned in a slight degree, but from that period, after taking the medicine, no further hæmorrhage occurred. The tincture was continued in the same doses, but at longer intervals for three days, with a light nourishing diet, and under due attention to the bowels, by the use of mild aperients, she soon recovered her usual state of health.

THE URINE OF THE COW AS A REMEDY.*

In a paper of M. Boussingault, is a fact which, he observes, will surprise chemists and physiologists; it is, that "the urine of herbiverous animals contains bicarbonate of potass, and not, as is generally believed, subcarbonate. With the urea and hippuric acid this urine curiously resembles an alkaline mineral water. It might be employed to dissolve uric acid calculi. I speak more seriously than you will be disposed to believe, when I say that I should have more confidence in the urine of one of my cows than in an alkaline solution prepared by many celebrated chemists."

ON COPAHINE.†

BY G. JOZEAU.

Being a subscriber to the *Journal de Chimie Médicale*, and of which you are the editor, I perceive in the number for May, 1845, a formula for the preparation of copahine, but containing errors which, I know not by what means, have been also admitted into the report laid before the *Académie Royal de Médicine*, by M. Cullerier.

Having a desire that this medicament, which, without being universal in its power, is still I believe the best preparation of copaiba, may without difficulty be prepared by my brethren, I wish to point out those errors that they may be rectified in your next number.

M. Mege and I have correctly said that in the case of a continental blockade we can obtain from the turpentine of Bordeaux, and by the same process, an analogous product, possessing the same properties, but with a less powerful action; but we consider that we ought not to employ this indigenous turpentine, except in cases where the balsam of copaiba is not to had.

We have also never pretended that the balsam of copaiba, nor the turpentine, should be deprived of their essential oil, because the action of nitric acid upon this body consists principally in giving up its oxygen to it, and rendering it more easily digestible, by taking from it its nauseo-drastic properties.

Correct as it may be to evaporate the product, previously washed in boiling water, to the consistence of a soft extract, it is preferable not to push the evaporation too far, and to finish the soldification by the addition of a twentieth part of calcined magnesia, and one-tenth of catechu, terra japonica, in powder.

For the ferruginous preparation you may substitute for balm of mecca, the copahine I have just described, and you will obtain an equally active compound.

* *Lancet.*

† *Journal de Chimie*, Sept. 1845.

TO CORRESPONDENTS.

"L. S." (Southampton).—The analysis will be forwarded in a few days.

"G. R." (Leeds) will receive an answer by post.

"M. and Co." are thanked for their communication.

"P."—The article "On Aqua Regia, &c., by Dr. Köene," will be concluded in our next number.

Macord's Isinglass Plaster.—The superiority of this plaster consists in its being more adhesive, and more easy of application than any hitherto offered to the public. Nothing can speak more strongly in its favor than that it is recommended by some of the most eminent of the faculty.—*See Advt.*

Notice.—*All Communications and Books for Review must be addressed* "To the Publisher of The Chemist, 310, Strand, London." *Communications must be prepaid, and sent before the* 15*th of each month; Books for Review before the* 10*th.*

THE CHEMIST.

I. CHEMISTRY.

ON AQUA REGIA; ON HYPONITRIC ACID, AS A MEANS OF OXIDATION; ON THE CONSTITUTION OF THE SAME ACID, AND THE PART IT ACTS ON BEING PUT IN CONTACT WITH ORGANIC SUBSTANCES.*

BY DR. KŒNE.

PROFESSOR OF CHEMISTRY AT BRUSSELS.

(*Concluded from page* 362.)

ON THE PART WHICH HYPONITRIC ACID ACTS WHEN IN CONTACT WITH ORGANIC SUBSTANCES.

22. The property of hyponitric acid to convert most bodies into oxides of the highest stage of oxidation, is attributable to its sparing degree of stableness. This property is one of the causes owing to which the acid in question is devoid of the principal character of acids. It has, therefore, no other function than that of oxidating; as it cannot combine with chlorine, whatever be the circumstances under which these two bodies are brought together; be it in a nascent state, in the presence or absence of water, at a moderate or low temperature, and even in the presence of a powerful base,† chlorine will invariably be disengaged as soon as the hydrogen of the hydrochloric acid begins to act on the oxygen of the nitric acid (19). Of this body, no other combination with an elementary substance is known than that with oxygen; and it may be anticipated, that it will never be possible to form any, as this cannot be brought about with chlorine, which, owing to its great analogy with oxygen, relative to polarity, is able to act a part analogous to that of the said metalloïd.

The hyponitric acid, therefore, does not act in the manner of compound radicals. May it be supposed that in some organic combinations it acts the part of a single body? In the present state of science, this is a question of the highest importance; and it may even be anticipated, that the future state of the chemistry of compound radicals will depend upon the precision with which this question will be settled.

23. By reacting upon an organic substance, and by oxidating one equivalent of hydrogen, according to the principles of the theory of substitution, nitric acid forms $(M-H^2+\dddot{N})$, it appearing, from the analysis of the fresh combination, that M by losing H^2 has combined with N^2O^4. According to Berzelius, however, the new combination has to receive the formula:

$$(M+O-H^2)+\dddot{N};$$

and combinations could even be obtained as follows:—

$$[(M+O^2-H^2)+\dddot{N}^2]\,[(M+O^3-H^4)+\dddot{N}^3]*$$

Hence, according to the first hypothesis, the fresh combination is corresponding with that on which the nitric acid acted; whereas, according to the second, it is composed of nitrous acid and an oxide of a compound radical.

We have, therefore, in the above instances two hypotheses, leading to different theoretical views, as they emanate from an arrangement of combinations, which is not one and the same in both. Investigations calculated to support one or the other of these hypo-

* Poggendorff's *Annalen*, March 1845.

† We satisfy the latter condition by directing a current of hydrochloric acid into nitre. If the salt is quite dry, at a common temperature, no action takes place; on applying, however, a gentle heat, or on making use of nitre which is not dry, chlorine, water, and nitrous vapors, are formed, products resulting from the influence of the water on the vapors; and, finally, chloride of potassium is left behind.

* As it is impossible to enter now into the details of the above two hypotheses, we leave unmentioned the circumstances under which the said process appears to others, when nitrogen or nitrous oxide enters into the compound radical. Our object was merely to bring into a parallel the principles of the views relating to the matter.

theses are highly important in the present state of science, and the more so as the establishing of one of them will be the means of ascertaining the constitution of organic combinations, and thus raise the study of these combinations to the rank of an accurate science.

24. *That the hyponitric acid in organic bodies does not act the part of a radical, appears from the foregoing experiments and considerations, especially from the action of hydrochloric acid on nitrate of potassa.*

Previous to having established this fact, I gave credit to the hypothesis of MM. Dumas and Conërbe. What resulted from other experiments relative to the nature of oxysulpho-sulphuric acid, did not remain without influence on my views of that of hyponitric acid. Not knowing, however, which is the precise part acted on by this acid, and considering it as a dehydrogenating body kindred to chlorine, I was of course induced to consider this as a radical, as is done with the latter metalloïd, which in organic substances may frequently displace the hydrogen. It is owing to the dehydrogenating power of the hyponitric acid, that its elements have been found in several combinations formed by its influence, in the manner of chlorine, which is found in a body, that by substitution yielded hydrogen to that metalloïd.

Though the hyponitric acid, however, in the presence of several organic bodies may act in the manner of chlorine, it is yet not proved, that it really acts like the said metalloïd, under circumstances in which it behaves like a simple body. The hypothesis of the function of this acid, therefore, reposing only on theoretical observations, and not being warranted by a single well-established fact, it became necessary to base it on rigorously examined facts. It having, however, appeared, from our investigations, that the hyponitric acid does not act the part of a radical, not even with reference to the body, which, relative to polarity, offers the greatest analogy with oxygen, we attempted to explain the action of this acid on organic bodies in another manner, considering it as a means simultaneously oxidizing and dehydrogenating. Free, therefore, from any preconceived opinion, we have examined Berzelius's theory, which we have found to agree with the present state of science, and to explain most of the facts ascertained in a satisfactory manner, to such an extent that, with some restrictions, we do not hesitate to give it our sanction.

25. *That the hyponitric acid, relative to organic bodies, acts the part of an oxidating and dehydrogenating means, is a conclusion directly to be deduced from its oxidating power*, as an organic body, by being transformed, yields a combination, which is the more stable the more complete the change is. The hyponitric acid, if it partially dehydrogenates a body, or if it forms a combination, on which, under the circumstances then taking place, it no more exerts any dehydrogenating influence; it may convert this body into an oxide, an acid, and sometimes even into a salt.

26. The oxide, at the moment of its formation, may combine with nitrous acid, the oxidating power of which is less than that of the two other acids of nitrogen.*

If the oxide formed does not enter into a combination, it continues to remain under the influence of the oxidating agent, it is raised to a higher stage of oxidation, and converted not either into a simple nitrogenous acid (effect of nitric or hyponitric acid on ethereal oils, on parafin, &c., substances free from oxygen), or into a nitrogenous double acid (nitro-naphthalinic acid, &c).

If, finally, this acid reacts on an oxysalt, produced by its influence, a salt may be formed raised to a higher stage of oxidation (effect of nitric acid on naphthalin, benzin, &c).

Even by other acids than those formed by the influence of the said acids, the oxidation may be brought about (effect of hyponitric acid on neutral fatty bodies).

The formation of organic, not nitrogenous, acids, produced under circumstances as above, and the effect of hyponitric acid on fatty matters are most important facts, serving as evidence that this acid acts, indeed, the part of an oxidating as well as of a dehydrogenating means.

If, however, an oxyacid, with compound radical, containing hydrogen, may be produced by the oxidating action of hyponitric acid, it may, likewise, happen, that by the action of the same, or of that of nitric acid, on other organic bodies bases may be formed, acidity and basicity being respectively dependent on an opposed tendency, resulting from the various manners in which the molecules are arranged, or the circumstances under which this grouping takes place, and on those under which these bodies combine, &c. Nothing, therefore, stands in the way of the formation of a base, which combining with the least powerful oxidating means, resulting from the partial deoxidation of the nitric and hyponitric acids, it yields a nitric salt.

27. On allowing concentrated nitric acid to act, for instance, on acetone $C^6H^{12}O^2$,

* From instances to be adduced in the sequel, it will appear that these processes are, indeed, of the kind as stated.

two combinations are obtained ($C^6H^6O + \dddot{N}$), ($C^6H^6 + \dot{H}$) which result from the dehydrogenating action of this acid; the elements of the base C^6H^6O, in this case, are present in the acetone.

If, however, this acid is allowed to act on mesitylen, it likewise produces an oxidating effect; there results,

$$C^6H^8 + \dddot{N} = (C^6H^8O + H) + \dddot{N},$$

because the radical C^6H^8 requires oxygen to form a base. That the reaction takes place in the manner indicated by the above equation is proved by the fact, that the chlorine forms with the same substance a base corresponding to nitrate of oxide of pteleyl, acting simultaneously as a dehydrogenating and chloridising means.

Owing to the existence of the radical pteleyl forming an oxide with oxygen, which sometimes combines with nitric acid, and on other occasions with water, chlorine, and nitric acid with mesitylen yield combinations, which bear no parallel, though the reaction of these agents is in fact one and the same; this, however, is merely meant according to the hypothesis of Berzelius, as, according to that by which the hyponitric acid is considered as a radical, we cannot account for this fact, since the nitric acid, by reacting on the mesitylen, gives no combination, into which the hyponitric acid enters as a constituent; hence it does not yield a combination constituted in a manner analogous to that of the product resulting from the reaction of chlorine or mesitylen.

28. If water be added to a hot solution of benzin in nitrous acid, it will be formed—

$$C^{12}H^{10} + \ddot{\dddot{N}} = (C^{12}H^{10}O + \dddot{N}).$$

That this fresh combination is not a corresponding one to ($C^{12}H^{10} + \ddot{S}$), but a salt formed by the combination of the protoxide of benzin with nitric acid, appears from the fact, that when the solution of this salt in the nitrous acid is allowed to boil, the protoxide of benzin is converted into the oxide of benzin, and the salt ($C^{12}H^{10}O^2 + 2\dddot{N}$) is formed.

29. Toluin, by being treated with boiling nitrous acid, likewise forms nitrite of oxide of toluin; whereas the nitrite of protoxide of toluin is formed, if the above treatment takes place at the common temperature (Deville).

30. If naphthalin is allowed to boil with nitric acid until the oil formed on the surface of the liquid ceases to exhibit the properties of naphthalin, according to Laurent the combination ($C^{20}H^{14} + \ddot{\dddot{N}}$) is obtained. To this Berzelius objects, that if the said combination would be thus constituted, the nitrite of oxide of ethyl ought to be considered as consisting of ($C^4H^{10} + \ddot{\dddot{N}}$). To this I add, that under similar circumstances the paranaphthalin yields a combination free from nitrogen, and that if the action of the nitric acid be somewhat longer continued, the nitronaphthalese would be converted into nitrite of oxide of naphthyl—

$$2(C^{10}H^7 + \ddot{\dddot{N}}) = C^{20}H^{14}O^2 + 2\dddot{N}).*$$

In the same manner as nitrite of the protoxide of benzin or toluin, under similar circumstances are respectively converted into the oxides of benzin and toluin. The nitronaphthalese ($C^{20}H^{14}O + \dddot{N}$) and the nitronaphthalese ($C^{20}H^{14}O^2 + 2\dddot{N}$) are, therefore, saline combinations, which may be respectively called the nitrites of protoxide and of oxide of naphthyl, designating, by the term naphthyl the radical, $C^{20}H^{14}$ of naphthalin.

31. It appears from the above instances, that by the combined influence of the application of heat, and the action of either the nitric or the hyponitric acids, an oxide of a radical may be raised to a higher stage of oxidation. This radical, however, being formed, by the influence of a dehydrogenating action of these acids, the oxide produced by the oxidating action of the same agents, it may easily be conceived, that if the reaction is longer continued, or made to work more energetically, the radical may simultaneously be changed, and oxides formed, containing a larger amount of oxygen than those previously produced. M. Laurent has, indeed, observed, that if the acid solution of the nitrite of oxide of naphthyl is allowed to boil until an oily body separates, a combination is formed, which the French chemist designates by the name of nitronaphthaleïse, which, however, is more properly called nitrite of oxide of naphthaleïn (azolite naphthaleïsique) considering the formula ($C^{40}H^{24}O^5 + 5\dddot{N}$) which probably ex-

* From the analysis of this combination resulted 2·90 of hydrogen. According to calculation it ought to have been only 2·41, supposing in the combination to be present six atoms of hydrogen for ten atoms of carbon. But 2·41 : 2·90 : : 6 : 772, it follows that the empirical formula is not $C^{10}H^6N^2O^4$, but $2C^{10}H^7N^2O^4 = C^{20}H^{14}N^4O^8$.

presses its combination. If naphthalin, and nitric acid are boiled together, for four or five days, the above combination is converted into nitronaphthalese, or into the combination ($C^{20}H^{10}O^8 + 3\dddot{N}$), which in the manner of that just quoted contains as many equivalents of acid as are equivalents of oxygen contained in the base. The fresh salt, therefore, is the nitrite of oxide of naphthalin.

The combinations in question, instead of being composed of an oxybase and nitric acid, contain, as the electro-negative constituent that of the three oxyacids of nitrogen, which is the least endowed with the power of oxidating, an energetically oxidating body being unable to expel the nitrous acid from its combination without changing the radical of the base.*

After this change, however, has once taken place, the nitric acid acts more intensely; the fresh radical is invariably more stable than the foregoing one (25), it may form with oxygen a more stable base, and the oxide thus produced may combine with a more energetic acid, even if the latter body be of the most powerful oxidating agents. In this manner, owing to a material change, by nitric acid, two acids are produced from *indigo*, in which the presence of nitric acid seems probable, and by a change similar to that which naphthalin undergoes, when treated with this acid a double acid is formed—($C^{16}H^6O^5 + \dddot{N}$) + 2 $\dot{H}$. It is owing to the same cause that, by the direct action of nitric acid on wood spirit, the oxide of methyl, without being altered, may combine with this acid, whereas the base of alcohol, being less stable, reduces the nitrous acid to the stage of a less powerful oxidating agent to that of nitrous acid (21), which may combine with another portion of the oxide of ethyl, that which has not been destroyed.

33. We have hitherto met with no difficulties regarding the explanation of the phenomena generally perceived by the reaction of either nitric or hyponitric acid on organic bodies. The reason of this is, that we admitted as correct Berzelius's hypothesis, according to which the organic combinations containing oxygen, consist of the latter metalloid, and a radical. This hypothesis, however, is far from being opposed to facts, with which organic chemistry has been endowed in latter days; it is rather confirmed by the constitution and properties of the products formed by the action of several oxidising agents on a great number of hydro-carbonaceous substances; by the constitution of ethereal combinations, and others analogous to the latter; by the various stages of oxidation of acethyl, formyl, &c.; by the circumstances under which the artificial valerianic acid is formed; by the analogy of the constitution of margarylic or hypo-margarylic acid; by that of the protein, and of the two higher stages of oxidation, serving to regenerate the latter substances; by that of the œnauthic and azoleïnic acid; by that of the bitter almond oil, and that of the sulpho-pioramyls; by the constitution and properties of xanthoxide and uric acid, and by the circumstances under which these bodies are produced; by the analogy of constitution of several chlorides of compound radicals, destitute of oxygen, and of the oxides of similar radicals; by that of the indenigen and indenoxide; by the various stages of oxidation of isaten, &c.; by the action of sulphuretted hydrogen on the nitrites of naphthalin and protoxide of benzin, of which are produced the amidures of the radicals; of these bases are produced—($C^{20}H^{14}O + \dddot{N}$) + 6 H^2S = ($C^{20}H^{14}$ + Ad) + 4 $\dot{H}$ + 6 S, ($C^{12}H^{10}O + \dddot{N}$) + 6 H^2S = ($C^{12}H^{10}$ + Ad) + 4 $\dot{H}$ + 6 S. Finally, it is known to all chemists that the constitution of the combinations formed by kakodyl, was understood by the illustrious Swedish chemist, at the time, when similar combinations were considered as composed in a manner totally different from that now believed to be the true one; induced by which circumstance the celebrated Bunsen attempted to isolate this radical.

34. Great, however, as the number of facts be, apparently warranting the truth of the hypothesis in question, it may, perhaps, not hold good in all cases, as it starts from a principle admitted in organic chemistry, viz., that *each oxygen combination of the first order is an oxide of a radical*, a principle found to be incorrect with reference to—$\ddot{W}o$, $\ddot{U}r$, $\ddot{C}r$, ($\ddot{K}Cr + \ddot{C}r$), $\ddot{S}$, ($\ddot{S} + \ddot{S}$), and in many others.* Notwithstanding,

* It is with these salts, as with the compound kinds of ether, they cannot be brought under the law of Bertholet, because the nitric acid employed to expel the nitrous acid from its combinations, must act with an energy calculated to decompose the radical either wholly or partially. The discovery of the cause of this anomaly is indisputably one of the most important which in organic chemistry ever could have been made.

* The sign ' means one equiv. of chlorine.

therefore, the most plausible analogies, Berzelius, not to be contradicted by his own hypothesis, considered these bodies as combinations of the third and second order. The combination of H. Rose, for instance, is, according to the Swedish chemist, composed of sulphuric acid, and chloro-sulphuric acid.

$$3(\dot{S}+\dot{S}) = S+5\dot{S}.$$

And Peligot's salt would consist of

$$(\dot{K}Cr+\dot{C}r) = (K+2\dot{C}r).$$

With reference to Pelouze's acid, it might have been considered as something analogous to the sulphate of oxide of nitrogen; but since I have shown that the sulphuric acid does not enter into a combination with the oxide of nitrogen, and that the sulphurous acid with regard to sulphur, acts the part of a radical, it became necessary to consider these combinations as well as oxy-nitro-sulphuric acid as being constituted in a manner analogous to that of the oxy-sulphuric acid.

35. Like the combinations of this kind, the benzamid also, and the oxamid, salicylamid, phtalamid, and other analogous oxidated combinations are composed of two radicals. Oxygen, therefore, may contribute to the production of compound radicals, and these, by combining with oxygen, chlorine, and sulphur, &c., may form oxy-acids, oxy-chloro-acids, oxy-sulpho-acids, oxy-bases, oxychloro-bases, oxysulpho-bases, and, finally, combinations also, in which either the oxygen, the chlorine, or the sulphur are no less acidifying agents than as means of reproducing bases.

The chlorine or the sulphur, however, being able to behave as the electro-negative element with reference to the oxygenous radical, and the oxygen itself, relative to a similar radical, acting the same part, it follows that, in an organic combination, the whole amount of oxygen is not employed in the formation of bases and acids, into the composition of which enters neither chlorine nor sulphur, nor any other other substance acting relative to a radical as an electro-negative principle. The hypothesis, therefore, laid down by Berzelius, like the principle upon which it is based, may sometimes be found insufficient. A strange relation between two sciences, both purporting the study of bodies, which, though dissimilar in nature, yet are formed and changed according to the same laws, the same principles, and often, even, according to the same theories. This stamps the hypothesis of the Swedish chemist with a degree of probability which is wanted in that of his opponents; asserting that the hyponitric acid in compound radicals, may displace the hydrogen without being able to show that this acid acts the part of a simple body.

ON THE CONSTITUTION OF THE HYPONITRIC ACID.

36. Comparing the property of nitrous acid to combine with sulphuric acid, with that of the said acid to form with nitric acid hyponitric acid, and considering the fact that the latter acids may be decomposed by bases, Berzelius at first supposed that these combinations are composed of nitrous and nitric acids.

37. When H. Rose, however, was believed to have prepared the sulphate of oxide of nitrogen, the Swedish chemist, in accordance with the German, considered the hyponitric acid of a constitution analogous with that in question. Both these views not well agreeing with the laws of multiple proportions, it remains to ascertain whether the hyponitric acid is a particular stage of oxidation of nitrogen.

38. If from motives on which the first hypothesis is based, the hyponitric acid is composed according to $\dddot{N}+\ddot{\dddot{N}}$, the nitrous acid ought to consist of $2\dddot{N}+\ddot{\dddot{N}}$, owing to the influence which an excess of oxide of nitrogen has on nitric acid, and to the property of the acid formed in this reaction, by which, on being put together with water, it may be resolved into the binary combinations of which it was formed. The nitrous acid, however, in contact with the hydrocloric acid, forms no chlorine. It, therefore, contains no nitric acid, from which we may conclude that the hyponitric also contains none.

39. If the hyponitric acid would be composed according to formula $\underline{\dot{N}}+2\underline{\ddot{N}}$, then, by the action of hydrochloric acid on a solution of this acid in concentrated sulphuric acid, oxide of nitrogen ought to be disengaged, which, however, does not occur.

40. The question may be resolved in another manner, by starting from stableness, when two points are to be considered. The first is the relative stableness of combinations $\underline{\ddot{N}}$ and $\underline{\ddot{\dddot{N}}}$, and the second that of the acids $\underline{\dot{H}}+\underline{\ddot{N}}$ and $\underline{\dot{N}}+\underline{\ddot{N}}$ or $\underline{\dot{N}}+2\underline{\ddot{N}}$.

The greater stableness of $\underline{\ddot{\dddot{N}}}$ relative to $\underline{\ddot{N}}$ may be attributed to a cause similar to that by which the relative stableness of combinations $\underline{\dot{N}}$, $\underline{\ddot{N}}$, $\underline{\dddot{N}}$, $\underline{\ddot{\dddot{N}}}$, is produced. It may

likewise be described to the affinity of $\dddot{\underline{N}}$ to $\dddot{\underline{N}}$ or $\ddot{\underline{N}}$. In the latter case, the hyponitric acid is a particular stage of oxidation, but it is not such a one in the second case.

If this acid is composed according to the formula $\dddot{\underline{N}} + \ddot{\underline{N}}$ or $\ddot{\underline{N}} + 2\dddot{\underline{N}}$, it must be more stable than $\dot{\underline{H}} + \dddot{\underline{N}}$, as the former may be formed by the action of oxide of nitrogen on the second, and because the stableness of a combination depends on its affinity; or it is proportionate to the affinity of the binary combinations, of which these combinations are constituted.* The foregoing observations and considerations, having shown that the hyponitric acid is less stable than the hydrated nitric acid, it follows that it is not a combination of nitric acid with nitrous acid or oxide of nitrogen.

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ON SOME NEW COMBINATIONS OF PERCHLORIDE OF TIN.†

BY M. B. LEWY.

Perchloride of tin, so remarkable for its physical properties, and for the interesting reactions to which it gives rise, has already been the subject of a great number of investigations. However, the combinations which it forms with water, with the basic chlorides, as well as with some organic matters, have not attracted all the attention of chemists.

I have undertaken a work on this subject; being on the point of absenting myself for some time, I beg the Academy to be kind enough to allow me to lay before it the results at which I have arrived.

It is known that, by adding a small quantity of water to perchloride of tin, the whole becomes a crystalline mass; by adding a greater quantity of water, the hydrate, thus formed, is dissolved, and, by slow evaporation, I again obtain crystals, but whose form could not be determined on account of their great deliquescence. These crystals gave, by analysis, results corresponding to 5 equivalents of water; their formula is, consequently, represented by

$$Sn\,Cl^2 + 5HO.$$

On exposing these crystals, *in vacuò*; over sulphuric acid, they lose a certain quantity of water of crystallisation, and a hydrate is finally obtained, which contains only 2 eqs. of water, and whose formula is represented by

$$Sn\,Cl^2 + 2HO.$$

Perchloride of tin possesses, as is known, properties analogous to those of the acids; it combines with the basic chlorides, forming double chlorides, most of which crystallise with great facility. They all contain equal equivalents of perchloride of tin and basic chloride.

The double chlorides of potassium and ammonium are anhydrous; but those which are formed by the chlorides of sodium, strontium, magnesium, calcium, and barium, contain water of crystallisation. According to the analysis which I have made at present, everything leads me to suppose that the quantity of water contained in these latter combinations corresponds to 5 equivalents. These bodies should, therefore, be represented by the following formula:—

$Sn\,Cl^2$, $Po\,Cl$;
$Sn\,Cl^2$, $Cl\,NH^4$;
$Sn\,Cl^2$, $So\,Cl + 5HO$;
$Sn\,Cl^2$, $Sr\,Cl + 5HO$;
$Sn\,Cl^2$, $Mg\,Cl + 5HO$;
$Sn\,Cl^2$, $Ca\,Cl + 5HO$;
$Sn\,Cl^2$, $Ba\,Cl + 5HO$.

All these combinations form beautiful transparent and very bulky crystals. M. De la Provostaye has been good enough to determine the crystalline form of these compounds, and the following is the note which he has sent me on this subject:—

"The double chloride of tin and potassium presents very beautiful crystals in the form of regular octohedrons.

"The double chloride of tin and ammonium likewise presents very fine crystals, of a larger size; and the form of this compound is represented by regular octohedrons, all of whose angles are modified by the faces of the cube.

"The double chloride of tin and sodium has not been determined; so far as could be judged, it appeared to be formed of small prisms.

"The double chloride of tin and strontium is presented under to form of elongated prisms, without determinable summits.

The double chloride of tin and magnesium seems to crystallise in rhombohedrons of about 125°. This measure is nevertheless very uncertain, and only approximative within one or two degrees. It is impossible to obtain a more accurate measure, on account of the great deliquescence of this combination.

"The double chloride of tin and calcium is still more deliquescent than the foregoing. This combination appears, at first sight, to be

* Supposing that the bodies are under the conditions stated in the said case.

† *Comptes Rendus*, Aug. 14, 1845.

crystallised in cubes. However, on placing it on the goniometer and measuring two supplementary angles, one was found to be from 84° to 86°, and the other from 94° to 96°. It is therefore probable that it crystallises in rhombohedrons.

"The double chloride of tin and barium was not determined; but, as far as could be judged, this crystalline compound presents small prisms."

In studying these double chlorides, my attention was naturally led to the beautiful combination of perchloride of tin with sulphuric ether, alcohol, hydrochloric acid, and pyroxilic spirit, the existence of which was pointed out some years ago by M. Kuhlmann.

I formed the bodies described by M. Kuhlmann, and I confirmed the accuracy of his work with relation to the preparation of these compounds. But, as M. Kuhlmann had not analysed these bodies, I thought I ought to verify the opinion which he had formed concerning their composition. I also endeavored to form some new combinations, and I proved that perchloride of tin combines very easily with oxalic ether, benzoïc ether, benozate of methyline, acetic ether, acetic acid, benzoïc acid, oil of bitter almonds, urea, camphor, ethal, &c. Most of these combinations constitute very fine crystals; but their alteration, easy in contact with the air, and even *in vacuô*, as well as their difficult purification, have hitherto prevented me from fixing the composition of these bodies in a very accurate mannner.

I will confine myself at present to detailing the analyses which have given the clearest results.

The combination of perchloride of tin with sulphuric ether forms crystals of very great beauty; this compound is obtained, as M. Kuhlmann has stated, by the contact of the two bodies, either in the state of liquid or of vapor. The crystals are presented under the form of rhomboïdal tables, of a brilliant aspect, and of perfect clearness. They are volatile without decomposition, dissolve readily in an excess of ether, and decompose in contact with water. The analysis of this compound gave the following results:—

I. 1 gr. ·164 of matter gave 0·527 of water, and 0·992 of carbonic acid.

II. 1 gr. ·010 of the same matter gave 0·373 of stannic acid and 1·397 of chloride of silver.

III. 0 gr. ·914 of another preparation furnished 0·403 of water and 0·772 of carbonic acid.

IV. 1 gr. ·891 of the same matter furnished 0·706 of stannic acid, which gives, in hundredths:—

	I.	II.	III.	IV.
Carbon....	23·24	—	23·03	—
Hydrogen..	5·02	—	4·88	—
Oxygen ..	8·60	—	—	—
Tin	—	29·02	—	29·34
Chlorine ..	—	34·12	—	—

These numbers correspond very well with the formula:—

$$2\ C^4H^5O,\ Sn\ Cl2;$$

we have, indeed,

C^8	48·0	23·57
H^{10}.....	10·0	4·91
O^2	16·0	7·86
Sn	68·8	28·88
Cl^2	70·8	34·77
	166·0	99·99

The combination or perchloride of tin with anhydrous alcohol was obtained by simply putting the two liquids in contact. During the mixture I always cool the substances below 32° F. The combination being made, I expose it, *in vacuô*, over sulphuric acid and pieces of potassa. At the end of a few days the combination is presented under the form of small prismatic crystals, which readily dissolve in an excess of alcohol, so that they may easily be made to recrystallise However, these crystals must not be kept too long *in vacuô*, as, in that case, they are easily decomposed. The analysis of this compound gave the following results:—

I. 0 gr. ·733 gave 0·23 of water and 0·382 of carbonic acid.

II. 0 gr. ·861 of matter gave 0·402 of stannic acid, and 1·148 of chloride of silver.

III. 1 gr. ·114 of matter gave 0·392 of water and 0·584 of carbonic acid.

IV. 0 gr. ·972 of matter gave 0·466 of stannic acid, which gives, in hundredths:—

	I.	II.	III.	IV.
Carbon....	14·21	—	14·29	—
Hydrogen	3·62	—	3·90	—
Oxygen ..	12·52	—	—	—
Tin	—	36·69	—	36·87
Chlorine ..	—	32·29	—	—

These numbers correspond very well with the formula:—

$$C^8H^{12}O^5Sn^2\ Cl^8,$$

which might be decomposed into

$$2\ C^4H^5O + 2\ HO + Sn^2\left\{\begin{matrix}Cl^3\\O.\end{matrix}\right.$$

We have, indeed,

C^8	48·0	14·82
H^{12}.....	12·0	3·71
O^5	40·0	12·36
Sn^2.....	117·6	36·32
Cl^3	106·2	32·74
	323·8	99 95

The combination of perchloride of tin with

oxalic ether was produced in the same manner as the foregoing. By adding small quantities of perchloride of tin to oxalic ether, a moment arrives when the whole is formed into a crystalline mass. The compound crystallises under the form of small needles, grouped round a common centre. These crystals are very easily decomposed; and it is best to analyse them immediately after having formed them. In contact with water oxalic ether is regenerated.

The analysis of this compound gave the following results:—

I. 0 gr. ·981 of matter gave 0·232 of water and 0·629 of carbonic acid.

II. 1 gr. ·776 of matter gave 0·660 of stannic acid and 2·472 of chloride of silver.

III. 1 gr. ·216 gave 0·272 of water and 0·789 of carbonic acid.

IV. 1 gr. ·422 of matter gave 0·530 of stannic acid and 1·985 of chloride of silver; which gives in hundredths—

	I.	II.	III.	IV.
Carbon....	17·48	—	17·69	—
Hydrogen..	2·62	—	2·50	—
Oxygen ..	16·27	—	16·09	—
Tin	—	29·20	—	29·29
Chlorine ..	—	34·33	—	34·45

These numbers correspond, perfectly, to a combination of equal equivalents of perchloride of tin and oxalic ether; we have, consequently, the following formula:—

$$C^4H^5O, C^2O^3 + Sn\,Cl^2,$$

which gives, indeed—

C^6	36·0	17·77
H^5	5·0	2·47
O^4	32·0	15·80
Sn	58·8	29·02
Cl^2	70·8	34·94
	202·6	100·00

To make the analysis of the different combinations which form the subject of this note, I operated as follows:—The organic elements were determined by the ordinary processes of combustion, by means of oxide of copper, finishing this combustion in a current of oxygen. To estimate the chloride and tin, I operated on a new portion of the matter; after having treated it by a great excess of water, a current of sulphuretted hydrogen was passed into the liquor which precipitated the tin in the state of bisulphuret. This precipitate, collected and washed, was treated by an excess of nitric acid, and converted into stannic acid, from which I calculated the amount of tin. The liquor from which the tin had been separated was neutralised by ammonia, then the hydrosulphate of ammonia was destroyed by the addition of the necessary quantity of oxide of copper. The filtered liquor, treated by nitrate of silver, furnished a precipitate of chloride of silver, which was collected in the ordinary manner, and whose proportion enabled me to calculate that of the chlorine contained in the matter.

I ascertained the process, which consists in immediately destroying the free hydrosulphuric acid by a salt of copper, is attended with loss in the estimation of chlorine, since there is formed, in this case, an insoluble combination, which retains a little chlorine.

I propose, if circumstances permit me, to submit to a profound examination the other crystallised combinations, whose formation I have noticed. The object of this note is only to take date for a more extended work.

ON TWO NEW OXACIDS OF SULPHUR.*

BY M. E. MATHIEU PLESSY.

THE work which I have the honor of presenting to the Academy has for its object to make known two new oxacids of sulphur, which I obtained in studying the action of sulphurous acid on the chlorides of sulphur in contact with water.

It is known that water decomposes these chlorides; the chlorine forms hydrochloric acid, and a certain quantity of sulphur which the oxygen of the water has not been able to reach is deposited. When sulphurous acid is made to intervene in this reaction, the sulphur does not separate; its affinities being then satisfied like those of chlorine, it enters into solution. This is especially evident with the perchloride of sulphur. This chloride disappears, so to speak, almost instantaneously, and entirely by agitation in water charged with sulphurous acid; it gives rise to the formation of sulphur and oxygen, by the preparation of which we shall immediately enter into the subject before us.

Sulphurous acid is passed into 1,500 grammes of distilled water until no more can be taken up; then, about 30 grammes of perchloride of sulphur are added to this acid liquor, and when the perchloride is dissolved by agitation, more sulphurous acid is passed into it, then a little more chloride is added. After having repeated this operation three or four times, it is stopped, and the acid liquor is submitted to evaporation on an open fire; it is reduced to about half, and when it is sufficiently cooled, it is saturated with powdered and sifted carbonate of lead. When the saturation is finished, it is filtered through

* *Comptes Rendus.*

linen: as the precipitate of chloride of lead retains a great quantity of liquid, it must be strongly pressed. The filtered liquid now contains a salt of lead, which is decomposed by the quantity of dilute sulphuric acid strictly necessary for precipitating the base. It is evident that a sufficient quantity of acid has been poured in when the sulphate of lead separates from a clear liquor. The acid eliminated is finally submitted to evaporation on an open fire with a few precautions, and it is continued until it marks from 12° to 15° of Beaume's areömeter; it then occupies, for the above indicated proportions, a volume of about 2 decilitres. Then, and when it is cool, it is saturated with carbonate of baryta, filtered, and its liquor is precipitated by absolute alcohol and ether.

The crystalline precipitate which is obtained, after having been expressed between folds of blotting paper, is dissolved in water. The solution, precipitated by absolute alcohol, gives a well crystallised salt, which leaves by calcination about 52·4 per cent. of residue. If the residue is greater, it is necessary to redissolve the salt and crystallise it, *de novo*, until it leaves invariably 52·3 per cent. of residue of sulphate of baryta; it is only then that it can be considered to represent the new combination in the pure state.

The salt precipitated in the first place by alcohol and ether may leave as much as 60 per cent. of residue; but this residue contains chloride of barium, which the treatment by water and afterwards by alcohol removes. The presence of a chloride in the salt itself may, moreover, be recognised by means of acetate of lead, the chloride of lead being very sparingly soluble.

A salt of silver could not be employed as a reagent, because the latter would form a yellow precipitate in which the chloride of silver could not be found.

I may now say that the prolonged ebullition which the acid is made to undergo, has for its object the separation of the acids of MM. Langlois, Fordos, and Gèlis, which are instable. The changes of reaction which the liquor presents after ebullition is a certain indication of their destruction. Before ebullition, this liquor forms a white precipitate with corrosive sublimate, and a blackish-yellow precipitate with protonitrate of mercury; after having been boiled, it no longer precipitates the first of these two reagents, and it forms in the second a fine yellow persistent precipitate, which an excess of nitric acid cannot turn black.

We have seen that water will no longer remove anything from the salt of baryta, which leaves 52·3 per cent. of residue. The precision of the re-actions of this salt, and its crystallisation, both oblige us to consider it as a pure salt.

To ascertain its composition, we decompose it by means of chlorine, which causes the sulphur to pass into the state of sulphuric acid. The chlorine, necessary for this transformation, furnishes the oxygen of the salt.

One gramme of the salt, treated with chlorine, gives 2·613 of sulphate of baryta, that is to say, a weight five times greater than that produced by calcination, which proves, that for one equivalent of the base, left after the calcination of the salt, it contains five equivalents of sulphur. We have also obtained for 1 gramme of salt, 5·802 of chloride of silver, which corresponds with 5 equivalents of sulphur, found by experiment, to 9 equivalents of oxygen; the salt that was analysed contains 6. We deduce from the preceding numbers the composition, in hundredths of the new salt.

Sulphur	37·87
Base	34·12
Water and oxygen	30·01
	102·00

These numbers are applicable to the following formula:—

$$S^5 O^6 Ba O, 2HO$$

In consequence, the new acid that results from the decomposition of *perchloride of sulphur*, would be represented by the formula $S^5O^6 + Aq$. The acid separated from the salt of baryta, by means of sulphuric acid, may be boiled, but it undergoes a slight decomposition. It does not appear to be decomposed at an ordinary temperature, and it is not affected by cold concentrated sulphuric acid. Nitric acid, on the contrary, precipitates sulphur from it. It does not precipitate the solutions of zinc, copper, or iron; it forms, in protonitrate of mercury, a permanent yellow precipitate, and in a solution of corrosive sublimate, it produces no precipitate; but after a few moments, and by slow degrees, a deposit of sulphur is formed. With nitrate of silver we obtain a beautiful yellow precipitate, which changes afterward, and pretty quickly, into a chocolate-brown. These reactions will not allow the acid that produces them to be confounded with any of the combinations of sulphur with oxygen, at present known. We have seen that they are singularly changed by the addition of a sulphuretted hyposulphite, or a bisulphuret. The salt of M. Langlois, particularly when in a very small quantity, sensibly blackens the yellow precipitate obtained by the protonitrate of mercury. As to the salt of MM. Fordos and Gélis, it is only acted on by corrosive sublimate, which yields a white precipitate.

The protonitrate of mercury is an evident test for the acid of M. Langlois; in that it has been extremely valuable to us; for this acid is frequently found mixed with the new combination, because it in some manner results from its decomposition, as we shall presently see.

It is for the purpose of destroying the acid of M. Langlois, as we have already said, that we boil the acid liquor we obtain after having decomposed the chloride of sulphur by means of sulphurous acid; because, in fact, under these circumstances it is less stable; under other circumstances, on the contrary, it has a tendency to be formed, especially in the presence of a base.

Our next business should be to describe the products of the decomposition of the acid, $S^5 O^6$, in the presence of a base. But among these products, the oxygenated combination we have obtained, the protochloride, water, and sulphurous acid, evidently places itself. It is, therefore, by studying this combination that we shall perfect that which is incomplete in the history of $S^5 O^6$.

ON THE ACTION OF SULPHUROUS ACID ON PROTOCHLORIDE OF SULPHUR IN CONTACT WITH WATER.

We pass sulphurous acid into 1,500 grammes of water until it rejects it, and as the protochloride dissolves but slowly, we add about 150 grammes at a time, and continue passing the sulphurous acid, so as to bring about the mixture of the two liquids, by the current of gas. When the chloride of sulphur has lost its fluidity, when it has become pasty, we discontinue the passing of the sulphurous acid, and consider the reaction complete; the acid liquid is separated from the chloride by decantation, is boiled for several minutes, and then saturated with carbonate of lead as before. The salt of baryta which is precipitated by pure alcohol and ether, leaves a residue of 61 per cent. We may here remark that the salt obtained under the same circumstances, employing, however, the bichloride, yields by calcination the same quantity of sulphate of baryta; and as the two are affected in the same manner by the reagents, it appears, in the first instance, that the product obtained by the protochloride of sulphur, and that which results from the decomposition of the bichloride, are identical; but if we dissolve in water the salt of baryta prepared with the protochloride of sulphur, if we precipitate it from its solution by means of alcohol, if, finally, we submit it to the same treatment as that employed to obtain the salt of the acid $S^5 O^6$, in a state of purity, its composition always remains the same, it always leaves a residue of 61 per cent.; its nature is not changed.

So that, in the first place, a salt of baryta that leaves a residue of 60 per cent., after having been dissolved in water a certain number of times, leaves, at last, a residue of only 52·3 per cent. of sulphate of baryta. In the second place, when operating with the protochloride of sulphur, we obtain a salt of baryta, yielding, by calcination, a residue of 61 per cent. of sulphate of baryta, and on which neither water nor alcohol have any effect.

We may here observe that the salt of baryta, prepared with the protochloride and precipitated by ether, contains no chloride of barium, while the corresponding salt—that is, a salt precipitated in the same manner, and prepared with the bichloride, does contain it. This difference may be partly attributed to the portion of chloride of lead that may have been dissolved.

When we operate with the bichloride, it produces, relatively, more hydrochloric acid, and consequently more chloride of lead; so that it is necessary to add a considerable quantity of water to complete its saturation with the carbonate of lead, and to avoid the loss of too great a quantity of the new product. If the quantity of chloride of lead dissolved is greater some time after the concentration and saturation by means of carbonate of baryta, the proportion of chloride of barium will have increased, and the salt may be precipitated by ether and pure alcohol.

By means of chlorine we determine the composition of a salt of baryta that leaves a residue of 61·4 per cent., and we obtain, as an average of many analyses, for one gramme of the salt.

Sulphur	33·84
Base	41·29
Water and oxygen ..	24·87
	100·00

These results lead us to the following formula for the salt so analysed:—$S^4 O^5 . BaO, HO$.

This is the composition of bisulphuretted hyposulphite of baryta, less one equivalent of water. In every case the acid we have obtained by means of the protochloride of sulphur presents too great an analogy with the combination furnished by the bichloride, to be confounded with the acid of MM. Fordos and Gélis, or to be looked upon as isomeric with the acid.

It is evidently necessary to double the present formula; but must we then make the acid it contains bibasic? This would give as a formula $S^8 O^{10} + 2HO$. Would

it not be preferable to represent it as derived from the acid S^5O^6 in the following manner?—

1 equivalent of this acid, abandoning 1 equivalent of hyposulphurous acid, S^2O^2, would form the hypothetical acid S^3O^4, which, finding the acid S^5O^6 undecomposed, would unite itself with 1 equivalent of this acid, to produce the acid S^8O^{10}.

The following equation explains this transformation:—

$$2\,(S^5O^6) - S^2O^2 = S^8O^{10} = \left\{\begin{matrix} S^3O^4\,? \\ S^5O^6 \end{matrix}\right\} = S^8O^{10} = (S^4O^5.)$$

Up to the present time I have in vain endeavored to obtain the salts of the acid S^3O^4. Whether the hypotheses I have just assumed have any value or not, we must be satisfied that the acids on which they rest is different from that with which it is confounded, as to composition and per centage. It is thus we have acquired this certainty; the acid, which we can now call the acid S^8O^{10}, offers different reactions to those which MM. Fordos and Gélis have assigned to their acid. These reactions consist in precipitating nitrate of mercury of a yellow color, and of forming in nitrate of silver a precipitate of the same color, but which soon changes to a chocolate brown. The acid S^8O^{10} forms no precipitate with corrosive sublimate, but, at the end of a short time, this reagent is decomposed with a deposition of sulphur. I will add, that the salt of baryta of the new acid differs also by 1 equivalent of water from bisulphuretted hyposulphate of baryta.

In conclusion, the acid S^8O^{10} is distinguished completely from the acid of MM. Fordos and Gélis by the remarkable decomposition its salts undergo beneath the influence of heat, when they form a concentrated solution.

When a solution of a salt of the new acid is submitted to a temperature of 104° or 120°F., it gives off sulphur, without any sensible disengagement of sulphurous acid. After some time the reagents demonstrate the presence of acids, consisting of 5 equivalents of oxygen to 3 or 4 of sulphur; the discovery of which is due to M. Langlois and MM. Fordos and Gélis. Thus, in this case, the very inverse, as it were, takes place to what occurs by the action of heat on the free acid.

In producing the combination of the acid S^8O^{10} with potassium, we have obtained the salt of Langlois in beautiful crystals, by the addition of a very small quantity of hot alcohol, and allowing it to become cold. Bisulphuretted hyposulphate is also produced by the decomposition of a salt of the acid S^8O^{10}. This salt may be distinguished from that of Langlois by means of corrosive sublimate. In a weak solution, this reagent attacks the bisulphuretted hyposulphuric acid, in the first instance, forming a white precipitate; it then acts upon the acid of M. Langlois, and decomposes it with a deposition of sulphur. It is to be understood, that we have been able to discover a certain indication of the presence of the salt of MM. Fordos and Gélis, in the acid S^8O^{10}, by the difference of reaction rather than by the analysis. After what has just been said of the decomposition of the salts of the acid obtained by protochloride of sulphur, the decomposition may be represented by the following equation:—

$$S^8O^{10} = \left\{\begin{matrix} S^5O^6 \\ S^3O^4\,? \end{matrix}\right\} \left\{\begin{matrix} 2\ BaO \\ 2\ HO \end{matrix}\right\} = S^3O^5BaO,\ Aq + S^4O^4BaO,\ Aq + S.$$

This transformation takes place in the absence of heat and water. It is then effected more perfectly, although slowly. If it is effected at a boiling heat, it produces sulphate and sulphurous acid.

In summing up the principal facts related in this paper, we arrive at the following conclusions:—

The bichloride and protochloride of sulphur, under the double influence of sulphurous acid and water, produce two new oxygenated combinations of sulphur.

These combinations contain oxygen in an even number of equivalents; they cannot therefore, enter into the series of which the acid of MM. Gay-Lussac and Walter forms the first term. They are, besides, capable of being transformed into acids belonging to this series by the influence of heat and a base.

In conclusion, I seize the occasion thus offered to express publicly to M. Pelouze, my preceptor, my profound gratitude for the advice he has given me on the subject of this work.

EXPERIMENTS AS TO THE QUANTITY OF HEAT DISENGAGED DURING CHEMICAL COMBINATIONS.*

BY C. GRASSI.

MANY philosophers and chemists have endeavored to determine, by direct experiment, the quantities of heat disengaged during chemical combinations.

These numerous labors were undertaken on account of the importance of the subject; in fact, most of the principal questions of physics and chemistry, are connected with it. We have seen, therefore, during the last century, Lavoisier and Laplace occupy themselves with this study, and employ their glass calorimeter to determine the quantities of heat produced by the combustion of many simple bodies: hydrogen, carbon, phosphorus, and also of some compound bodies, as oil, tallow, wax, &c., used daily for the purpose of giving artificial light.

In 1779, Crawford, published in London, experiments on animal heat, and the burning of combustible substances. He followed nearly the same plan as Lavoisier and Laplace, but was far from being equally accurate. In his memoir, he compares the heat furnished by an animal to that produced by the combustion of wax, tallow, oil, &c.

In 1814, Count Rumford published his experiments on the same subject, and employed an apparatus very different from the glass calorimeter. The experiments of Rumford are, without doubt, imperfect in many respects; but we are indebted to him for a very ingenious process, employed since his time by many natural philosophers, to avoid the necessity of keeping account of the heat lost or gained by the apparatus employed, either by radiation, or by contact with the surrounding air.

Since that time, many philosophers have endeavoured to elucidate the subject. We must not forget to mention Hassenfratz, Clement Desormes, Marius Bull, and Welter, who occupied themselves principally in comparing the results already obtained, and imagined they were able to establish a law which we shall discuss presently. M. Berthier, looking at the question in a practical point of view, inquired into the comparative value of different combustible substances.

In 1823, M. Despretz, published his excellent experiments on animal heat; he did not adopt the numbers found by his predecessors, for the quantity of heat disengaged during the combustion of carbon and hydrogen, but he preferred determining them by direct experiment. At a rather later date, M. Despretz extended his researches, and gave the relative co-efficients to phosphorus, zinc, and iron. This was an immense advance, for, hitherto, the heat produced by the combustion of various metals had not been calculated. This end had not been obtained by Rumford's calorimeter, because that philosopher had not even succeeded in burning charcoal. In the combustion of charcoal, M. Despretz varied the pressure of burning gas, and has proved, that in the case of this substance, which does not change the volume of the oxygen, the amount of heat disengaged is the same, let the density of the gas be what it may.

At the same time, M. Dulong read before the Academy of Science a paper on the same subject; but he always had some doubt of the correctness of his results. On that account he undertook a repetition of his experiments, when death deprived science of his services. A few figures found among his papers, prove that this skilful philosophher would not have been long before he had thrown a new light on this important part of the measurement of heat.

More recently also, Messrs. Hess, Andrews, and Graham have studied the disengagement of heat in chemical combinations, which was effected under a liquid form.

The first of these chemists was led to the discovery of two very remarkable laws. We shall see presently whether they are applicable to the chemical actions we are about to inquire into.

Notwithstanding these numerous labors, the question is not yet definitively settled. The numbers proposed by these philosophers exhibit too great a difference between each other, even in the case of substances we ought to imagine the best inquired into. While some of them, for example, based on remarkable experiments, admit that a gramme of hydrogen produces by combustion 24,000 units of heat, others consider the number 34,444 ought to be substituted.

I now publish the first series of experiments on this difficult subject.

M. Soubeiran did me the honor of making me a party to his experiments; but, unfortunately, unforeseen business prevented my continuing that series of observations. I entreat him to receive my sincere thanks for the advice he has constantly given me, which guided me in my first essays.

The chemical combinations, which are generally known under the name of combustion, that is to say, those that result from the union of oxygen with various simple and compound bodies, attracted my attention in the first instance.

* *Journal de Pharmacie.*

I was induced to make this choice by the numerous cases in which they could be observed, either by the application we could make of the knowledge of the amount of the heat disengaged during the combinations that take place in various useful arts, or in the resolution of important physiological processes, or, finally, because oxygen combines itself with most substances; the numbers already discovered enabling us to appreciate the correctness of the law laid down by Welter, or to discover some other relation, if it exists between the amount of heat disengaged during combustion, and also some of the general properties of bodies.

I endeavored, in the first instance, to determine the heat produced by the combustion of hydrogen.

The experiment consists in forming a mixture of two volumes of hydrogen and one of oxygen, in burning this mixture in an apparatus completely surrounded by water, and in determining exactly the elevation of the temperature of the water of which the weight was known.

The following is the mode of calculating an experiment, to arrive at the number we shall find, presently, laid down.

The unit of heat, which we call caloric, is the quantity of heat necessary to raise the temperature of water one degree. The experiment indicating the elevation of the temperature of the water, by multiplying the number of degrees by the weight of the water expressed in grammes; we then obtain the total number of units of heat produced by the combustion of a certain volume of gas. By dividing this number of caloric by the volume burned, we obtain that which is relative to a litre of the gas in question.

I will say but one word about the apparatus. It consists of two distinct parts; that in which the mixture of the gases is prepared and measured, a gasometer, furnished with graduated tubes, which are the means of being more exact in the measurement of the volumes. The other part of the apparatus, that in which the combustion is effected, is composed of two metal boxes, placed one within the other. It is in the inner box the combustion takes place. The irregular space that separates these boxes, contains the weight of water for the absorption of the heat produced. This water is kept constantly agitated, and its temperature is indicated by a thermometer capable of showing from 1 to 212°F.

Great care is necessary in the purification of the gases, and to measure their volumes an account is taken of their temperature and of the pressure to which they are subjected. I must add that I made use of Rumford's plan, to avoid the errors that might result from the radiation of the apparatus; this plan consisted in making use of water at a lower temperature than the surrounding atmosphere, of heating it by the effect we wished to measure, and stopping the heating process when it has acquired a temperature superior to that of the surrounding air, by the same number of degrees as it was below it at the beginning of the experiment.

Among the results obtained by the combustion of hydrogen, I will only mention those of the nine experiments that were made with the greatest care.

A litre of hydrogen at 0° under 760 produced by its combustion—

3110·8	3115·6
3123·5	3130·2
3124·9	3101·7
3105·9	3137·2
3130·7	

Average..... 3120.

It will be seen that these numbers are very nearly the same, the highest surpassing the lowest by 36 units—that is to say, about $\frac{1}{87}$th of the average value.

First, considering the density of the hydrogen 0·0693, we find that 1 gramme of hydrogen produced by its combustion, ·34666 caloric.

COMBUSTION OF CARBON.

Carbon is not the only combustible substance contained in the charcoal of ordinary wood. We know, in fact, that it always contains volatile matters in considerable quantity, and always varying according to the degree of calcination to which it has been subjected.

This description of charcoal, therefore, could not be employed in the experiments in which I was engaged. I gave the preference to that which is prepared by burning sugar in a close vessel.

This charcoal is extremely hard, very brilliant, burns with extreme difficulty, even in pure oxygen, and loses its brilliancy at the points undergoing combustion.

I lighted it by placing it in an atmosphere of oxygen, and bringing it in contact with a piece of charcoal made from spindlewood, cut in the form of a cone, which I had previously lighted.

The charcoal was weighed before and after combustion. The difference in the weight indicated the quantity burned. The experiment was completed between two and six minutes, and the elevation of the temperature of the water was from 37·4° to 59·1°.

I always adopted Rumford's plan.

The following are the results of twelve experiments.

One gramme of charcoal produced—

Calorics.	Calorics.
7753	7738
7662	7710
7630	7701
7690	7682
7740	7801
7759	7700

Average 7714

Leaving the carbonic acid, and adopting for the combustion of carbon the number 7714, it is seen that 1 gramme of carbon, combining with 2667 of oxygen, disengages 7714 units of heat, whence 1 gramme of carbon attaching itself to 0·3749 of oxygen effects the separation of 2892 units of heat.

COMBUSTION OF OXIDE OF CARBON.

The oxide of carbon was prepared in the usual manner, which consists in acting on oxalate of potassium by means of sulphuric acid. The products of this reaction are, as every one knows, oxide of carbon, and carbonic acid; the carbonic acid is absorbed by the potash; and the oxide of carbon, thus purified, is afterwards mixed with its own volume of oxygen, and its own volume of hydrogen—that is to say, I propose a mixture of equal volumes of the three gases.

It was only in this manner I was able to obtain a regular and sufficiently intense flame.

The calculation of the experiment also offers no difficulty. After having ascertained the volume of the gas which disappeared during the combustion, we deduct one-third. The quotient is the volume of the oxide of carbon consumed, as well as that of the hydrogen. From the total number of the caloric given by the experiment, we take that which relates to the hydrogen, and we thus obtain that which ought to be attributed to the oxide of carbon. The calculation is completed by dividing the number by the volume of the gas.

The following is the result of six experiments:—

One litre of oxide of carbon at 0° and 760mm., produced—

Caloric.	Caloric.
2395·3	2321·0
2360·3	2320·8
2362·0	2391

Average 2358

COMBUSTION OF PROTOCARBURETTED HYDROGEN (MARSH GAS.)

This gas is prepared by heating in a retort equal equivalents of acetate of potash and quick lime. The gas, when purified, is mixed with twice its own volume of oxygen; the mixture thus prepared may be burned, yielding water and carbonic acid.

These experiments were conducted as before.

The following are the numbers obtained by the combustion of 1 litre of gas at 0° and 760mm.:—

Caloric.	Caloric.
7956·3	7980·6
7964·1	7883·0

Average ..... 7945·8

COMBUSTION OF BICARBURETTED HYDROGEN.

This gas is prepared by the reaction of sulphuric acid and alcohol. It gives out an empneumatic odor. Its purification is tedious and difficult; to remove nearly the whole of its smell, I was obliged to wash it in alcohol, in a caustic solution, and in several of Liebig's tubes filled with concentrated sulphuric acid. This gas mixed with three times its volume of oxygen burned, and was completely transformed into water and carbonic acid. Five experiments gave the following numbers for the combustion of 1 litre of this gas, at 0° and 760mm.:—

Caloric.	Caloric.
10694	10937
10694	10781
10674	

Average 10756

COMBUSTION OF ESSENTIAL OIL OF TURPENTINE.

The essence of turpentine, properly purified, gave the following results:—

For 1 gr. of essence.	For 1 litre of vapor.
Caloric.	Caloric.
10560	68766
10429	67913
10515	68473
10496	68245
Average.. 10496	Average.. 68349

Anhydrous alcohol gave the following numbers of caloric by the combustion of 1 gramme of the alcohol and 1 litre of vapor:—

For 1 gr.	For 1 litre of vapor.
Caloric.	Caloric.
6612	13857
6503	13629
6610	13853
6499	13621
Average.. 6556	Average.. 13740

Eight combustions of spirit of wood, perfectly purified, gave the following numbers:—

Caloric produced by 1 gr. of spirit of wood.

Caloric.	Caloric.
5811	5741
5892	5922
5889	5854
5659	5751

Average 5830

By adopting this number 5839, and taking the weight of the litre of vapor, as given by M. Dumas, at 1·456 grains, we find that this volume of vapor produced by combustion 8502 caloric.

MM. Dumas and Peligot have taught us that, by treating spirit of wood with sulphuric acid, half of its water is removed, and we obtain a gas, the monohydrate of methylen, which is, with respect to spirit of wood, what ether is to alcohol.

Methylic ether is of the same composition as alcohol, and the same density as the vapor of alcohol.

These circumstances make it extremely interesting to endeavor to ascertain the quantity of heat disengaged by the combustion of this ether, that it may be compared with that given by alcohol. It is necessary for that purpose to receive the gas over mercury, on account of its great solubility in water; unfortunately, I had not the proper apparatus at hand.

Let us see, however, whether it is possible to deduce a few conclusions from the comparison of the numbers given above. According to the numerous inquiries as to the amount of heat disengaged during certain chemical combinations, M. Hess was led to lay down two very remarkable laws, which he gives as general laws. The first is as follows:—

A combination taking place, the quantity of heat disengaged is the same, whether the combination takes place in a direct manner, or whether it is effected indirectly and at several times.

Thus a gramme of charcoal, while burning, and giving out carbooic acid, produces the same amount of heat as if it first passed to the state of oxide of carbon, and that this oxide, burned in its turn, was transformed into carbonic acid.

This law is demonstrated by the experiments of M. Hess, on the hydratation of sulphuric acid.

If we take anhydrous sulphuric acid and dissolve it in a quantity of water, so that no fresh addition of liquid shall affect the temperature, we obtain a quantity I shall represent by A.

If we inquire into the amount of heat the same weight of anhydrous acid disengages during its combination with three atoms of water, for example, and afterwards the amount disengaged by the same sulphuric acid, combined with three atoms of water, with water added in excess, we discover two quantities of heat, which, added together, reproduce A.

Let us admit, as it is reasonable to do, the correctness of this law with regard to the combinations under consideration in this work, and deduce a few conclusions from the numbers given above.

A litre of carbonic vapour that weighed 1 grain 0808, produced, while becoming carbonic acid, 4716 caloric.

The difference, 3621, of these two numbers, may be taken to indicate the heat disengaged by 1 grain, 0808 of carbon passing into the state of oxide of carbon.

Thus, while the first equivalent of oxygen that attaches itself to a litre of the vapor of carbon develops 3621 caloric, the second equivalent of oxygen, which transforms the oxide produced into carbonic acid, disengages in its turn 4716 caloric.

These two numbers are in the proportion to each other as 1 is to 1·3.

This result gives the exact amount of the utility of employing oxide of carbon formed in the operations performed in lofty furnaces. This gas, by its combustion, can furnish more than half the heat a given quantity of charcoal is capable of yielding, during its transformation into carbonic acid.

M. Hess was induced to admit, that when two bodies combine in several proportions, the quantities of heat disengaged are in simple proportion to each other,

He thought that the numbers discovered by M. Dulong, for the production of carbon and oxide of carbon, afforded a confirmation of this law. According to his calculation, the heat disengaged by the first atom of oxygen bore the proportion of 3 to 2, with respect to the second atom.

M. Ebelmen has shown that there was an error in the calculation of M. Hess, and that the proportion in question was evidently 1 to 4.

The result of my experiments do not lead me to so simple a proportion.

We find, in volume xix. of the *Annales de Chimie et de Physique*, a table arranged by Welter, in which are set down the quantities of heat disengaged by the absorption of the same amount of oxygen by different bodies.

The comparison of these numbers induced Welter to imagine that the quantities of heat disengaged by the same weight of oxygen might very well be equal or in simple proportion.

Welter, however, as he himself observed, is far from being positive.

The numbers found by the combustion of carbon and hydrogen, are, for 1 gramme of oxygen absorbed,

0·3749 Carbon and 1 gr. oxygen 2892 cal.
0·125 Hydrogen and 1 gr. oxygen 4333 ,,

These numbers are to each other as 1 to 1·497, or nearly as 2 to 3.

This simple proportion might possibly be the effect of chance.

If Welter's law be correct, it ought especially to be so in the case of analogous bodies, for the two carburets of hydrogen, for instance; for example—

1 litre of oxygen, combining with ½ of protocarburet of hydrogen, disengages 3973 caloric.

1 litre of oxygen, with ½ of bicarbonate of hydrogen, disengages 3585 caloric.

These two numbers do not differ so much as those according to which Welter thought he might lay down his law. Still, their difference is, I think, too great to be attributed to an error in the experiment; I think, therefore, that Welter's law is not correct in the case before us, a case that, nevertheless, is placed in the most favorable circumstances.

M. Dulong finds, for the quantities of heat disengaged by the two carburets of hydrogen, combined with 1 litre of oxygen, the numbers 4793 and 5112.

When a compound body is burned, the quantity of heat it disengages is always less than the sum of those that result from the combustion of the separate elements.

Thus, 1 litre of protocarburet of hydrogen, or 0 gr. ·727, produces 7946 caloric.

But this volume of gas contains carbon and hydrogen, which, when burned separately, give

Carbon..	0 gr. ·549	..	4234·9 caloric.
Hydrogen	0 ,, ·178	..	6168·2 ,,
	0 ,, ·727	..	10403·1 total.

The difference of the numbers of the caloric is 2457, so that 1 litre of bicarburet of hydrogen, or 1 gr. ·2752, produces 10756. But if the elements of which the body is composed had been burned separately, they would have produced—

Carbon....	1·0962	..	8454·5 caloric.
Hydrogen..	0·1790	..	6202·1 ,,
	1·2752		14656·6

1 litre of the vapor of essence of turpentine, at 0° and 760mm., weighs 6 gr. ·512. It gives out, while burning, 68350 caloric, while, if the elements of which it is composed were burned separately, they would produce 70231.

I hope, in a future number, to publish a few of the consequences that result from these experiments, and which relate to animal heat.

ON THE POWER OF DISSOLVING OXYGEN, POSSESSED BY LITHARGE IN A STATE OF FUSION, AND ON SEVERAL PECULIARITIES WHICH ACCOMPANY THE PRODUCTION OF LITHARGE BY CUPELLATION ON A LARGE SCALE.*

BY M. F. LEBLANC.

We know that the cupellation which is practised in establishments where lead is prepared from the ore, is an operation, the object of which is to separate the silver from the lead, by eliminating the latter metal in the state of an oxide. Cupellation in the large way, or refining, as it is called, in some manufactories differs from the cupellation of the assay offices, on account of the litharge, or protoxide of lead being formed by the action of the oxygen of the atmosphere on the lead in a state of fusion at a high temperature, instead of being eliminated by absorption into the substance of cupel, it flows out of the furnace as fast as it is produced along a kind of gutter, which is constantly kept level with the surface of the melted metal. The substance that forms the cupel of the furnace, ought, as much as possible, to resist the absorption or dissolving action of litharge in a state of fusion.

The oxygen of the atmosphere is projected on the surface of the metal by a forced current of air.

During a visit I lately made to Poullaouen, I had an opportunity of being present at many of the operations for the refining of lead, containing silver, and of making several observations which perhaps will appear not devoid of scientific interest.†

We know that the litharge of commerce should possess certain characteristics, which can be produced at will to a certain extent, by properly managing the duration of the time allowed for the cooling of the litharge that

* *Journal of Pharmacie.*

† The process of refining at Poullaouen is usually performed upon about a ton of lead at a time; at the end of about eight or ten hours the fusion is complete, and they begin to skim the pot: soon afterwards the draught of air is employed, and the *abstrichs*, or black litharge, begins to pass over: it is a protoxide of lead mixed with oxysulphurets. The time occupied in the passing of the black litharge, varies according to the degree of the purity of the lead: it may continue for sixteen or twenty hours before they begin to collect the litharge of commerce. The total duration of the process for the refining of a ton of metal, is from 48 to 50 hours.

flows from the furnace. It is known that litharge when cooled quickly is yellow, or greenish yellow, and that litharge when slowly cooled, under the circumstances pointed out by M. Fournet, changes its structure and color, and acquires properties, which cause it to be much sought after in commerce.

It is to the study of the physical and chemical modifications that produce this transformation, that I devoted myself.

M. Fournet admits, in effect, that litharge in fusion can absorb oxygen, by becoming super-oxidised, and that at a higher temperature than is necessary for the decomposition of minium. This philosopher admits that the red litharge so much sought after in commerce owes its properties to an excess of oxygen.

The opinion of M. Fournet is not shared by M. Thénard; this illustrious chemist repudiates the hypothesis of the super-oxidation of lead, at the temperature of the furnaces, cupel, and considers it possible that a solution of the oxygen in the litharge, analogous to that of the same gas in melted silver, takes places. Thus oxygen unites itself to the protoxide of lead when the cooling process is slow, and disengages itself when it is rapid.

M. Pernolet, the present director of the mines and workshops at Poultaouen and Huelgoat, had previously observed that litharge in fusion held a gas in solution, varying at different periods of the process, and that this gas had a tendency to disengage itself at the instant the solidication took place. The facilities he was kind enough to grant me, enabled me to collect, carefully, the litharge, in different degrees of purity, and at various stages of the process; I also collected the gas when it was disengaged. The experiment completely confirmed the suspicions of M. Thénard, so far as regards the solution of the oxygen; for the gas collected offered me the properties of nearly pure oxygen. Its analysis indicated from 82 to 90 per cent. of oxygen. It cannot be said with certainty that the proportion of nitrogen was not due to the accidental presence of a small quantity of air; in fact, when repeating the experiment in the same manner upon silver taken out of the refining cupel, I did not find more than 90 per cent. of oxygen absorbable by heated phosphorus.

The quantity of oxygen dissolved in a given weight of litharge is too considerable to enable us to admit that the silver contained in the litharge could be the solvent of the gas; this proportion was not less than 50 cubic centimetres per kilogramme, notwithstanding the losses inseparable from the mode of operating. Besides, the last quantities of the litharge, and the richest, did not contain more than from 0·001 to 0·0015 of silver.

It appears to me, therefore, an established fact that litharge, a substance incapable of oxidation at the temperature of the furnace, might, under the influence of a current of air, dissolve the oxygen after the same manner as silver, and act as most liquids do when in contact with gases.

The black, or impure litharge is, according to my experiments, unfit for dissolving the gas; at least the quantity found was so trifling, that we may be allowed to consider its presence as accidental; analysis has also indicated oxygen and nitrogen, in nearly the same proportions as in the air. This circumstance will not be surprising, if we reflect that this excess of litharge contains sulphuretted, oxidisable elements.

Ought the solution of oxygen in a liquid in igneous fusion, and possessing no chemical action on the gas, to be considered as a general phenomenon, or merely confined to litharge and silver? It is a question the experiments I propose to make may perhaps solve. The phenomenon is connected, perhaps, with questions arising out of geology, and consequently deserves an attentive study.

Not to wander from the subject of this note, I shall confine myself, for the present, to the examination of what passes in the centre of the masses of melted litharge when it leaves the furnace, and to the discussion of the part the imprisoned oxygen may play in these masses that become solidified, and end in changing, by degrees, their internal structure.

At Poultaouen, the litharge, when it leaves the furnace, and when it has acquired a sufficient degree of purity, is collected in iron pots of a conical form, and holding about 30 litres. The litharge is not long before it becomes solid on the surface, and it is then of a yellow or greenish yellow color; at the end of a few hours, sometimes at the end of half an hour, the mass breaks and splits in every direction, becoming a friable crystalline mass, possessing a deep red color; the crust, which had become suddenly solidified, alone preserves its color and coherence. The red litharge only, which is carefully picked out, is fit for sale; the yellow litharge is placed apart, to be revivified.

Sometimes the phenomenon takes place more quickly; a sort of explosion occurs, which, in the first instance, separates the conical mass of litharge into a number of large blocks, at the same time there is a projection of a certain quantity of litharge, which had remained liquid, or in a pasty state in the interior.

It appears to me to be probable enough that the oxygen imprisoned while the solidification was going on, acts mechanically in the phenomenon of the exfoliation of the litharge.*

It is proper to point out the fact that all those circumstances that tend to diminish the rapidity of the cooling process, and the solidification of the litharge, have also a tendency to increase the proportion of red litharge that is formed; when it is poured into vessels of insufficient capacities, the litharge cools too quickly, and remains yellow, and without exfoliation.

M. Fournet admits that red litharge contains more oxygen than yellow, and that it owes its color to a certain proportion of minium, a portion of which many specimens have exhibited the undoubted presence of.

M. Thénard, and most chemists, also, attribute the color of the litharge of commerce to the presence of a small quantity of minium.

Without wishing to deny that litharge, when slowly cooled, cannot, under certain circumstances, absorb oxygen and form minium, a well established fact, I think, nevertheless, I am able to demonstrate that another explanation must be found for the exfoliation of the masses of litharge, and the production of red litharge. The following are the experiments on which I think I can rest this assertion :—

1. The red litharge I have examined gave out no oxygen by heat.

2. The same litharge, tried with much care by pure nitric acid, furnished me with no puce-coloured oxide; a minute portion of minium added to yellow litharge, scarcely modifying its tint, could be discovered under the same circumstances.

3. The red litharge, heated to a temperature at which it gave out no oxygen, and thrown suddenly into water, became yellow.

This experiment shows that the variations in the structure and colour of litharge, according to the circumstance of temperature that accompany its production, do not depend on changes in chemical composition, but much on modifications arising out of isomerism or dimorphism, like those that cause the difference between vitreous arsenious acid, and opaque arsenious acid, barley-sugar and sugar-candy, red iodide, and yellow iodide, of mercury, &c.

These modifications in the structure and color of protoxide of lead have a relation to the density of the specimens. According to the experiments I have made, red exfoliated litharge is less dense than yellow crystallised litharge. Figures having connection with various experiments made for this purpose, will be found in my *Memoirs*.

In fine, the facts related in this note appear to me to establish—

1. That oxygen can be dissolved by litharge in a state of fusion, as it is dissolved in silver, and that it does not form a superoxidized combination: the nitrogen dissolves in it, *perhaps*, in a small proportion.

2. That physical differences of structure, color, and density alone exist between yellow and red litharge, which make no alteration in their chemical composition. The different varieties may be produced at will by a dry process, according to the circumstances of temperature, and the quickness of the cooling of the mass.

* The first pots of litharge that are collected do not always exfoliate; nevertheless, I have proved that they sometimes contain oxygen in the state of gas, although in small quantities.

RESEARCHES ON THE CONSTITUENTS OF THE ACIDS OF PHOSPHORUS.*

BY M. ADOLPHE WURTZ.

PHOSPHOROUS ACID.

The great analogy that exists between hypophosphorous and phosphorous acids, have induced me to extend my researches in regard to this last named acid. I asked myself whether some intimate connection in the very constitution of these two acids was not attached to this analogy of properties.

This view of the case could only be verified by numerous analyses of the crystallised phosphorous acids, and of the phosphites. It was important to determine with exactitude, not only the quantity of base which phosphorous acid is capable of saturating, but, above all, the proportion of water contained in the phosphites.

In the same proportion in which this study has been facilitated in the case of the hypophosphites, it has been long and troublesome with regard to the phosphites, for if the first are well defined combinations, and in general assume very regular crystalline forms, it is not the same with respect to the phosphites, which, usually, are insoluble in water, and crystallise with great difficulty. Let us add, also, that phosphorous acid frequently unites itself to the same base in different proportions, and that these combinations have sometimes a great tendency to modify themselves and mix with each other, and we shall have sufficiently exposed the embarrassing nature of their study.

* *Comptes Rendus.*

I give the results of my researches on the phosphites in the following table :—

Names of the phosphites analysed.	Formulæ of the dry salts.	Water of crystallisation.	Remarks.
Crystallised phosphorous acid	PHO^4, 2 HO		
Neutral phosphite of potassa dried at 280°	PHO^4, 2 KO		
Acid phosphite of potassa	2 PHO^4, $\left\{\begin{matrix}KO\\HO\end{matrix}\right\}$ + PHO^4, 2 HO		
Neutral crystallised phosphite of soda	PHO^4, 2 NaO	+ 10 HO	At 212°F. this salt loses its water of crystallisation.
Neutral phosphite of soda, dried at 300°	PHO^4, 2 NaO		
Acid phosphite of soda	2 PHO^4, $\left\{\begin{matrix}NaO\\HO\end{matrix}\right\}$ + PHO^4, 2 HO	+ HO	This salt loses 1 equiv. of water by desiccation.
Phosphite of ammonia	PHO^4, 2 H^4A, 2 O	+ 2 HO	At 212° this salt loses 2 HO and ammonia.
Neutral phosphite of baryta (H. Rose)	PHO^4, 2 BaO	+ HO	These three salts lose 1 equiv. of water by desiccation.
Acid phosphite of baryta	PHO^4, $\left\{\begin{matrix}BaO\\HO\end{matrix}\right\}$	+ HO	
Acid phosphite of lime	PHO^4, $\left\{\begin{matrix}CaO\\HO\end{matrix}\right\}$	+ HO	
Phosphite of manganese (H. Rose)	PHO^4 2 MnO		
Phosphite of protoxide of tin (H. Rose)	PHO^4, 2 SnO		
Phosphite of copper	PHO^4, 2 CuO	+ 4 HO	At 266°F. this salt loses its water of crystallisation, and is partially decomposed.
Phosphite of lead	PHO^4, 2 PbO		
Basic phosphite of lead	PHO^4, 2 PbO + PbO ?		

It results from these analyses, and also from those published long since by H. Rose, that the phosphites do not exist in an anhydrous state. I have shown that the neutral phosphites, properly dried, contain at least 1 equiv. of water; as to the acid phosphites, the quantity of water with which they remain combined varies according to the proportion of phosphorous acid they contain.

The oxygen and hydrogen the phosphites always contain, are never disengaged in the form of water, when these salts are subjected to the action of heat.

I admit that these elements, intimately united with phosphorus and oxygen are essential constituents of phosphorous acid. Accordingly the formula of this acid, as it exists in the phosphites, becomes PHO^4 instead of PO^3.

The arguments I can bring forward in favor of this opinion are analogous to those I have employed in the case of hypophosphorous acid. It appears useless, therefore, to repeat them here. Allow me merely to discuss several objections that may be made to my theory.

It may be acknowledged in the first place that the phosphorous acid that contains three equivalents of water in a crystallised state, is a tribasic acid, and that the equivalent of water the phosphites contain performs the part of a base. According to this hypothesis, the phosphite of soda will consist of the same constituent parts, as common phosphate of soda.

$PO^3\left\{\begin{matrix}2So\ O\\HO\end{matrix}\right\}$ is analogous to $PO^5\left\{\begin{matrix}2So\ O\\HO\end{matrix}\right.$; but the analogy which is evident between

these formulæ does not really exist between the salts themselves, as the following experiments will prove.

If we decompose the neutral phosphate of soda by acetate of lead, tribasic phosphate of lead is precipitated, and the liquid becomes acid. In this instance, we see that the molecule of the water of the salt of soda is replaced by a molecule of oxide of lead.

Nothing of this sort takes place with phosphate of soda. When that salt is precipitated by acetate of lead, the liquid remains neutral, and phosphite of lead with a double base is obtained. The equivalent of the water of the phosphate of soda cannot therefore be replaced by oxide of lead. Much more than this, when we form superbasic phosphite of lead, by means of subacetate of lead, we again find in this salt the molecule of the water of the neutral salt. This water, therefore, does not possess the essential character of the basic water, that of being displaced by the influence of an excess of the fixed base.

To the arguments I have just adduced I can add others, which I hope are decisive. I have, in fact, succeeded in introducing phosphorous acid into organic combination, and the basic power of these compounds will leave no further doubt as to that of the phosphorous acid, which ought to be considered as a bibasic acid.

There now remains one objection which, at first sight, appears more serious. Phosphorous acid has been obtained in an anhydrous state. Its properties have been studied by MM. Berzelius and Steinacher. It is met with under the form of light white flakes, which can be sublimed by heat, and will not redden dry litmus paper.

Does the existence of this substance weaken the theory I have just laid down? I think not. The nature of the chemical functions of bodies, which have been called anhydrous acids, are almost unknown. Their properties have a tendency to place them at a distance from the acids, properly so called, and it is with reason that M. Gerhardt has proposed to call them anhydrides. In my opinion the composition of these bodies throws no light upon the constituents of acids, properly so called. Have we discovered the constituents of tartaric acid and of the tartrates by the analysis of anhydric tartaric acid? No. It is only by an attentive study of the salts which form an acid that we can hope to discover the true nature and intimate arrangement of its molecules.

Such are the views which have guided me in the study of hypophosphorous and phosphorous acids.

ETHEROPHOSPHOROUS ACID.

$PHO^4, C^4H^5O, HO.$

When we pour protochloride of phosphorous upon alcohol at 36°, we observe a very active reaction. The products formed in this instance are—hydrochloric acid, hydrochloric ether, phosphorous acid, and etherophosphorous acid. We get rid of the two former by heating the acid liquid with a gentle heat, and hastening the concentration *in vacuô*. The residue, saturated with carbonate of baryta, furnishes an abundant precipitate of phosphite of baryta. The etherophosphite remains in solution, and is obtained by evaporation in *vacuô*, in the form of a white amorphous and brittle mass. This salt is decomposed by heat, giving out inflammable carburetted gas, phosphoretted hydrogen, and a residue of phosphate. It is soluble in water and alcohol, and insoluble in ether; its aqueous solution is decomposed, after a time, in the form of barytic acid and alcohol.

The etherophosphite of lead, PHO^4, C^4H^5O, PbO, is easily obtained by saturating the crude etherophosphorous acid with carbonate of lead, and evaporating it in *vacuô*, it consists of very brilliant thin cakes, greasy to the touch, and soluble in water and alcohol.

AMYLOPHOSPHOROUS ETHER.

$PHO^4, {}^3C^{10}H^{11}O.$

To prepare this ether you pour, by a little at a time, one volume of protochloride of phosphorous upon one volume of amylic ether, carefully cooled. You then, very slowly, add water to the mixture, until no more chlorohydric acid is disengaged. You thus obtain an oily liquid, formed by the mixture of amylophosphorous ether and amylophosphorous acid. After having washed it several times in distilled water, it is treated with a solution of moderately concentrated carbonate of soda, which dissolves the amylophosphorous acid. To complete the purification of the ether, you wash it in distilled water, and heat it in *vacuô* several times to 100°. The water and hydrochlorate of amylen, which it still retains, are disengaged at that temperature.

Amylophosphorous ether is a colourless liquid of the density of 0·967 to 19 degrees; its smell resembles that of amylic alcohol, its taste is caustic. It is only volatilised at a high temperature when it is partially decomposed.

Preserved in contact with air, it becomes, after a time, acid. Under the influence of a boiling solution of potassa, it is decomposed in the form of phosphorous acid, and amylic alcohol. It reduces nitrate of silver.

Submitted to the action of chlorine, it becomes heated, and disengages hydrochloric acid. A viscid product is obtained, which is easily decomposed, giving out vapours of chlorohydric acid. If operated on at 32° F., and in darkness, it forms a monochloric ether, which shall be the subject of a future communication.

AMYLOPHOSPHOROUS ACID.

$PHO^4, C^{10}H^{11}O, HO$.

It corresponds with ethero-phosphorous acid, and is formed at the same time as the amylophosphorous ether.

By decomposing it by chlorohydric acid, we obtain the amylophosphite of soda, an incidental product of the preparation of amylophosphorous ether. It is precipitated as an oily liquid, which is redissolved in water. By adding a little chlorohydric acid to this solution, the amylophosphorous acid is separated as an oily liquid, heavier than water, which you dry in *vacuo*.

When recently prepared, this acid completely dissolves in water; this solution is precipitated by chlorohydric acid. It is decomposed at the end of a short time into phosphorous acid and amylic alcohol. The amylophosphorous acid, after having been prepared some time, cannot be completely dissolved in water. Exposed to the action of heat it is decomposed, furnishing inflammable carburetted gas, a small quantity of a liquid capable of reducing nitrate of silver, and a residue of phosphorous acid, which is itself decomposed if the temperature is raised, into phosphorous acid, and phosphoretted hydrogen.

It reduces nitrate of silver.

It decomposes the carbonates with effervescence, and forms very ill-defined salts, which in general do not crystalise. The amylophosphite of baryta is soluble; that of lead insoluble.

CONCLUSIONS.

I have established in this memoir the following facts:—

1. Hypophosphorous acid is a monobasic acid; the salts it forms have bases that always retain 2 equivalents of water.

2. Phosphorous acid is a bibasic acid.

3. The neutral phosphites retain, at least, 1 equivalent of water, whose elements are intimately united with those of the phosphorous acid itself.

4. Phosphorous acid has a great tendency to form acid salts, that contain, at least, 2 equivalents of water.

5. This acid forms, with common alcohol and amylic alcohol, acids analogous to phosphorinic acid.

6. It unites with molecules of amylic ether, to form amylophosphorous ether.

7. We find in the ethereal combinations of phosphorous acid, the equivalent of water necessary for the constitution of this acid.

From these facts we may deduce the following theoretical consequences.

Considered according to the theory of Lavoisier, all the acids of phosphorous form a series, the different terms of which are derived from each other by substitution. Phosphorous acid is phosphoric acid, in which 1 equivalent of oxygen is replaced by 1 equivalent of hydrogen; in the hypophosphorous acid, 2 molecules of hydrogen have taken the place of 2 molecules of oxygen.

$PO + {}^{5}3HO$, Phosphoric acid.

$P(HO^4) + 2HO$, Phosphorous acid.

$P(H^2O^5) + HO$, Hypophosphorous acid.

These proportions arise in an equally evident manner, if we look at the acids of which we are speaking, according to the theory of Davy.

MONOBASIC ACID.	BIBASIC ACIDS.	TRIBASIC ACID.
PHO^6 Metaphosphoric acid.	PH^2, O^7 Paraphosphoric acid.	PH^3, O^8 Phosphoric acid.
PH^2, O^5 Unknown.	PH^3, O^6 Phosphorous acid.	..
PH^2, O^5 Hypophosphorous acid.	..	..

Thus we see, that the hydrogen remains the same in the case of all the hypophosphorous acids, and that the basic power remains in proportion to the quantity of oxygen.

AN INQUIRY INTO THE PROPERTIES OF GLYCERINE.*

BY M. J. PELOUZE.

In a work published in 1836, I endeavored to discover the composition of glycerine, by the analysis of the sulpho-glycerates, a new class of salts, which I discovered at that time by saturating with bases the product of the action of sulphuric acid on glycerine. My analyses chiefly led me to the employment of sulpho-glycerate of lime, for which I have discovered the following formula:

$$Ca\,O,\,(SO^3)^2,\,C^6\,H^7\,O^5$$

This salt, when decomposed by an excess of lime, regenerates glycerine in a state of perfect purity; and as the latter in a free state was composed of $C^6\,H^8\,O^6$, I have assigned $C^6\,H^7\,O^5$, HO as the formula for free glycerine, and the formula $C^6\,H^7\,O^5$ for glyceric ether, or, if you will, for anhous glycerine, in the state in which we may suppose it to exist in the sulpho-glycerates, salts in every respect analogous to the sulpho-vinates.

The fertile ideas of M. Chevreul on the constitution of unctuous neutral bodies, which he proposed to assimilate to compound ethers, received, in some sort, a fresh confirmation from the existence of sulpho-glyceric acid.

It was allowable to imagine that *glyceric ether* ought to possess the same composition in unctuous neutral bodies, as in the sulpho-glycerates, and for a long time this opinion seemed to be also entertained by the different chemists who had occupied themselves on this important question. In every case, the extremely complicated molecule of unctuous bodies allowed some uncertainty to rest on the formula of anhydrous glycerine; and this uncertainty seemed to increase after the experiments of Mr. Playfair on myristine, and those of Mr. Stenhouse on palm oil; in fact, the anhydric palmitic acid $C^{32}\,H^{31}\,O^3$, the composition of which was deduced by Frémy and Stenhouse from the analysis of the hydrated acid, and a large number of salts, is united in palmatine to a demi-equivalent of glycerine, from which the elements of 3 equivalents of water had been removed. Palmatine, consequently, had for its formula:

$$\frac{C^{32}\,H^{31}\,O^3 + C^6\,H^7\text{—}H^3\,O^3}{2}$$

Myristine, according to Mr. Playfair, presented an analogous composition; if these last facts are correct, the result, is that glycerine does not exhibit a similar composition in unctuous neutral bodies, as it does in the sulpho-glycerates, or else it must be admitted to have been badly analysed in the case of these last compositions. It is particularly for the purpose of correcting my former results, that I undertook the researches I have the honor of laying before the Academy. I have confirmed their accuracy both by returning to my former experiments, and by extending them to a new series of combinations.

Perfectly colorless glycerine, which left a portion of ashes too minute to be appreciated, was dried in an oil-bath, at a temperature between 248° and 266°. It was burned with oxide of copper; and in 100 parts of this substance were found (C = 75 H = 12·5 O = 100).

	I.	II.	III.
Carbon	38·95	39·00	39·15
Hydrogen ..	8·72	8·80	8·75
Oxygen	52·33	52·11	52·10

The formula $C^6\,H^8\,O^6$ gives—

C^6	450	39·1
H^8	100	8·6
O^6	600	52·3
	1150	100·0

This formula, therefore, agrees as nearly as possible with the experiment; and there appears to me to be no doubt that the composition of free glycerine is the same as I discovered it to be ten years ago.

As to the glycerine in vinic salts, the following are the conclusions at which I arrived:—

1,000 grammes of sulpho-glycerate of lime, dried at 241°, left a white residuum, which was burned at a red heat with distilled sulphuric acid, the weight of which was 0·353 grammes. The formula

$$Ca\,O\,(SO^3)^2,\,C^6\,H^7\,O^5$$

taking 0·355 grammes for the same quantity of salt as was previously mentioned. These two numbers are, so to express it, confounded with one another. As the preparation of sulpho-glycerate of lime is tedious and difficult, and as, in other respects, I have every reason to consider the composition I have given to be perfectly correct, I have endeavored to find another order of glyceric salt, by substituting phosphoric acid for sulphuric acid.

When glycerine is combined with solid phosphoric acid (anhydrous or hydrated), the temperature of the mixture rises rapidly to more than 212°, provided the operation is performed upon 30 grammes of the substance. The mixture contains a considerable portion of a new substance, which I shall call *phosphoglyceric acid*; after being dissolved in water, it is neutralised by carbonate of baryta, and, in the last place, by baryta water. The liquid contains a large quantity of this base, in the state of a phosphoglycerate, and the precipitate consists of

* *Comptes Rendus.*

phosphate of baryta. The liquid, accurately precipitated by sulphuric acid, furnishes phosphoglyceric acid. This acid unites itself to various bases, and forms with them a series of salts, almost all of which are soluble in water, and insoluble, or to a very small extent soluble, in alcohol. Like many other acids, it can only be concentrated to a certain extent beyond which it is decomposed, even at a low temperature.

With lime and baryta it forms a series of salts, which are precipitated by alcohol in a state of purity. Those of lime are almost insoluble at 212°, but, on the contrary, very soluble at a low temperature, to such an extent, that they are almost entirely deposited from their watery solution; when the temperature is raised to the boiling point, it acts in this respect like phosphovinate of baryta. The phosphoglycerate of lime, heated to from 320° to 338°, remains unchanged. It preserves its whiteness and its solubility dried at that temperature, and calcined in a capsule of platinum, taking care to destroy the last traces of charcoal by means of pure nitric acid, it leaves a residuum of phosphate of baryta (2 BaO, PhO^5), the average weight of which is raised to 60·1 per cent. of that of the dry salt.

(We have obtained as much as 60·3, 60·1, 60·5, 59, 85, 59·80 per cent. of that of the dry salt,)

Burned with chromate of lead, it gave—

	First Experiment.	Second Experiment.
Carbon....	16·95	17·05
Hydrogen..	3·40	3·45

These numbers produce the formula

$$C^6H^7O^5, PhO^5, 2\,CaO.$$

In fact, the latter produces

2 CaO, PhO^5 ..	1600·0	60·66
C^6.....	450·0	17·06
H^7.....	87·5	3·32
O^5.....	500·0	18·96
	2637·5	100·00

In another case, 1 gr. ·916 of sulphoglycerate of baryta, after having been dried at about 302°, left 1 gr. ·246 of phosphate (2 BaO, PhO^5), or 73 per cent., a number identical with that indicated by the formula

$$2\,BaO, PhO^5, C^6H^7O^5.$$

Finally, the phosphoglycerate of lead, dried at 248°, left, after calcination, a residuum of 77·5 of phosphate of lead.

Theory points out 78 per cent.; every analysis mentioned in this note agrees, therefore, in assigning to free glycerine the formula

$$C^6H^7O^5, HO,$$

and shows that, like alcohol, it loses in vinic acids 1 equivalent of water.

A circumstance that will interest chemists and phrenologists in a most lively manner, and give to phosphoglyceric acid a very high degree of importance, is the existence of this acid in the animal kingdom.

M. Gobley, assistant professor at the "Ecole de Pharmacie," son-in-law of our lamented fellow academician, M. Robiquet, had been engaged for a considerable time in an analysis of the egg of the fowl. He knew that I was busy on the subject of glycerine, and that I had formed with that substance an acid exactly resembling phosphovinic acid. He was particularly engaged in the discovery of the form under which phosphorus exists in the yolk of an egg, in which analysis points out a considerable proportion. After many experiments, conducted with the greatest skill, this chemist succeeded in proving, by the most rigorous tests, that phosphoglyceric acid exists in the yolk of an egg, partly in combination with soda, and partly with ammonia. He extracted this acid in a state of purity, by means of simple solvents; he subjected it to the most scrupulous analysis, and his numbers were confounded, so to speak, with those I had myself discovered in the acid, when artificially produced. Chemists who will read the memoir of M. Gobley and my note, will not have the least doubt as to the identity of the phosphoglyceric acid of the egg, and that we obtain by the direct action between phosphoric acid and glycerine. The egg, independently of the phosphorus, contains, as we know, a considerable proportion of sulphur; it would be curious to inquire whether a certain amount of this element is not to be met with in the state of hydroglyceric acid.

Be it as it may, the, for the future, well-known state in which phosphorus is met with in the egg of a fowl, and most probably in those of other birds, is a well-defined truth for physiology.

ORGANIC ADULTERATIONS IN DISULPHATE OF QUININE.

To the Editors of THE CHEMIST.

GENTLEMEN—

WILL you do me the favor to insert the following reply to Mr. Mowbray's comments upon the test recently proposed by me for organic adulterations in disulphate of quinine.

If Mr. Mowbray had happened to see the September number of the *Pharmaceutical Journal*, in which the test was first published, he would have seen that I alluded to the fallacy which he has observed, and pointed out precautions, by which any error

arising from the charring of the quinine itself might be avoided. If the application of heat is stopped short of the appearance of *white fumes of volatilised sulphuric acid*, I have never observed that pure disulphate of quinine becomes black. It *may* acquire *a slight tinge of brown*, whilst if any sugar or gum is present, a *black* color is produced which cannot be mistaken after being once seen.

The results of my own experiments have not corresponded with Mr. Mowbray's statement, that the quinine is not dissolved if the sulphuric acid is first added, and subsequently the water.

I am,
Gentlemen,
Your obedient servant,
J. Birkbeck Nevins.

76, Falkner-street, Liverpool.
October 2nd, 1845.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

SECOND COMMUNICATION ON THE DISEASE OF THE POTATO.*

BY M. PAYEN.

When, at the last meeting, I communicated the results of my first observations on the disease of the potato, I left a very important question in a state of uncertainty.

I pointed out the appearance of sporules in a granular substance, with a peculiar odor, resembling that which arises from the fungi in bread, and the chemical reactions that concurred in pointing out a cryptogamic vegetation in the tissue of the tubers; but a direct observation of the forms of these organisms was wanting, and no conclusions could be drawn.

The difficulty, on this point, was so great, that it arrested the progress of several of the most skilful inquirers, and I am not aware that any one overcame it.

These circumstances caused me to decide upon employing every mode of investigation possible to ascertain the nature and composition of certain organisms in the substance of the tissues. The methods employed are described in the memoirs I have collected under the title "Development of Vegetables."

The greater number of these attempts were successful, but I shall only point out, in this place, those which are the most easily repeated, and whose elementary analysis has already had an effect on the results.

One of the diseased tubers is parted in water, and the temperature is kept up to 212°, for about three hours.

At the end of that time a very curious phenomenon is seen in all the sound parts; the swelling of the grains of fecula gives a rounded form to the cells, and destroys their adherence, and they can be separated from each other by slightly rubbing them.

It is not the same in the case of the cells in that part of the tissue which has been attacked by the red matter, that points out the progress of the disease: here, the cells, notwithstanding a similar swelling of their fecula, remain unaltered, particularly at those points where the color is deepest and less translucid; these resisting parts can, therefore, be removed without difficulty from the substance in its normal state, which yields up its grains. When all the agglomerated parts are thus obtained, they are disintegrated by trituration under water.

The fecula is then separated (it is particularly abundant in the darkest colored cells) by acting upon the mass for four hours, by means of water, made acid by the addition of one hundredth part of sulphuric acid.

It is easy then to satisfy yourself that no more fecula remains capable of changing its color by the application of iodine, by adding one drop of this test to a few drops of the mixture, previously allowed to become cold.

All the acid and soluble salts are removed by washing, on a filter, with an abundance of water, and the pulpy substance is then ready for microscopic examination, as in elementary analysis.

In fact, the fecula being removed, and the granular matter dispersed, a large number of perfect and transparent cells are visible, which can be distinctly seen by a power equal to from 500 to 800 diameters; the mode of penetrating also, and the arrangement of the various parts of these parasitic fungi, that are most advanced beyond the periphery towards the centre of the

* *Comptes Rendus.*

tubers. The sketch I produce, which was drawn under the microscope, will give a tolerably correct idea of their appearance.

The net work, drawn concentrically in the cells, originates in a few filaments which have penetrated the sides and anastomosed with, or crossed other filaments, performing the same part in the neighboring cells.

A few drops of an aqueous solution of iodine leaves the cell and its inner wall uncolored, thus showing that the cellulous substance remains, while the organic azotised matter, and the fatty substance, have disappeared, absorbed, no doubt, by the fungus.

The latter, under the influence of the test, has become of a darker color, bordering on yellow, which defines its form more clearly.

The addition of a drop of sulphuric acid, at 105°, completes the phenomena, affording a beautiful microscopic view. Then the portions of the fecula, not dissolved by the acidulated water, but too firmly attached to each other to have been affected by the iodine, are separated by contact with the more concentrated acid; you may then see them become tinged with a beautiful indigo blue color in the midst of the filamentous network of orange yellow, which envelopes all the grains of fecula.

For the purpose of ascertaining whether there was any similarity in its elementary composition between this fungus, and those I had previously analysed, I proceeded to determine the quantity of azote it contained. The following are the numbers of my analyses:—

Quantity employed, 99 milligrammes; volume of azote, 6^{cc}, 6^2; atmospheric pressure, 75·95; temperature, 40°, from whence it appears there is 7·56 per cent. of azote. Deducting the ashes, or 0·03 of the substance, we obtain 7·8 per cent. of azote; finally, removing the cells and the traces of starch, representing, according to an immediate analysis, 0·20, we find that the organic matter of the fungus contains 9·75 per cent. of azote, while the cultivated mushroom contains 9·78. This composition comes also very near to that of many microscopic fungi.

We have just seen that the cells attacked by the most forward parts of the fungus are filled, with fecula in a normal state, imprisoned within the meshes of the network which is developed in the interior; but between these most penetrating parts, and the epidermis, which never contains starch, any more than the herbaceous tissue, is found a layer of more or less thickness, containing cells more or less completely emptied of their grains of fecula. It appears then, that, with respect to the discarded layers when placed under the microscope, many observers have actually proved the presence of an abundance of grains of fecula in a normal state; while others, not less exact, have noticed, in the discarded layers, a large number of cells in which the proportion of fecula was diminished.

This difference once established, I endeavoured to ascertain the cause of these two states of the diseased parts, as well as the nature of the injury done to the fecula. On these two points, also, I believe I have solved the problem.

It is sufficient to cut into thin slices those portions of the tissue in which the starchy fecula is diminished, to follow the progress of a very remarkable disease, of which the following are the successive phases:—

First. The azotised organic substance, which has been applied to the inner side of the covering of each cell, detaches itself, and forms a kind of bag, enclosing grains of fecula; these are still in a normal state.

They soon diminish, and from that time many changes occur. Attacked at one part of their superficies, their internal substance is disintegrated and dissolved, and the sides of the cavity become marked with irregular fissures, which gradually become deeper.

The substance comprised between these corrosions detaches itself, and breaks up each of the grains as fast as their dissolved portions are absorbed.

The total remains of the starch diminishes, the detached envelope becomes narrower, and more thin by degrees, and takes itself a part in the dissolution.

Almost the whole cavity of the cell will be found empty; the bag, reduced to a very small size, merely contains a few irregular fragments of feculant matter of a rounded form.

Soon afterwards nearly all disappears, and nothing remains but the cellular, diaphanous and empty chamber.

Sometimes a few grains of fecula, attacked at the same time at a number of points on their periphery, dissolve concentrically, layer by layer, then it happens that the amylaceous kernel which had formed itself in the last instance round the axis of the grain is set at liberty. This mode of action is less frequent than the former. Some examples of the progress of disease in the fecula may be seen in the sketches I present to the Academy.

All these observations are without doubt delicate, but they offer no serious difficulty; any one can repeat them, by employing the solution of iodine, for the better discovering the amylaceous matter, which becomes blue, and the diminution of the azotised sub-

stance, which changes to an orange-coloured yellow.

You may observe, but not quite so easily, the filaments between the cells, containing a series of minute oily drops, analogous to the prolongation of the fungi in bread; the addition of a drop of sulphuric acid at 150° dissolving the cellular substance, will set free and render clear and perceptible the oily drops, which are soluble in ether.

Finally, you will succeed in discovering the filaments introduced into the cells; but here is the greatest difficulty, for the double or triple envelopes, formed by the sides of the cells, and their species of internal bags, will all tend to conceal them; they may however be discovered, but it will be necessary to moderate the light, for their transparency and tenuity are extreme.

The sketch of the second figure shows the appearance of a cell almost entirely empty, as well as the filamentous prolongations, accidentally disengaged.

These new observations appear to us to place in evidence the principal cause and the varied effects of the disease of the potato, which we shall sum up in a few words.

A peculiar cryptogamic vegetation, propagating itself, no doubt, from the stems to the tubers, is the origin of it.

The microscopic fungus, the sporules of which have followed the liquid, infiltrated round the cortical parts in particular, advances towards the medullary portion, and developes itself in the cells by anastomosed filaments, which take possession of the fourfold, oil-like, organic substance, resting on the fecula, which they enclose within their meshes.

Traversing, also, the intercellular labyrinths, from one cell to another, they cross each other and make the parts they have taken possession of more firm; they maintain their consistence, notwithstanding the cooking in water at 212°. The air-like prolongations directed towards the periphery passing through the sides of the cell to attack all the azotized, oily, and amylaceous matters they contain, that are capable of assimilation; the fecula is gradually disintegrated, dissolves, and being absorbed, it presents in the history of the active cause a series of new and rapid changes.

In the whole of these facts we recognize, then, the action of an enormous parasitical vegetation, which takes possession of a portion of the living tissue of the potato, resting on some, and exhausting another part of the substances they contain, which can be assimilated.

Such is the form of the malady brought to us, without doubt, by the sporules of this peculiar species of fungus, of which moisture and a proper temperature hastened the development.

As to several other symptoms, they are evidently secondary. It may, in fact, be calculated that the term of life of the fungi first developed has soon arrived; that then all causes of destruction act upon them; they lose their consistence, and their tissues are disintegrated; animalcules attack the remains, and destroy the cells; putrid fermentation, in its turn, brings about the destruction of the animalcules, and increases the changes in the vegetable organization.

Nevertheless, a large number of the grains of fecula, not directly attacked by the fungus, still remain.

To all these successive causes, dependent one on the other, are to be frequently added the various accidental attacks we have mentioned.

As to practical deductions, we have nothing to change, with respect to the conclusions of our former note, and we should always be inclined to resort to the precautions they point out. A fact observed by M. Ceffin D'Orsigny has verified one of them. Potatoes attacked by the fungus have yielded 14 per cent of their weight of fecula, of a grayish tint, while healthier tubers from the same locality produced 18 per cent. of white fecula.

The washed pulp of the first was also very rich in fecula, and yielded, in one experiment, syrups that were easily converted into alcohol.

We may add, as a deduction from new experiments, that consistence of the parts attacked by the fungus, after the cooking of tubers, points out to the naked eye the existence and the limits of the special disease, and that this characteristic exercises an influence on the digestion of the diseased tubers.

Up to the present time, the principal aim has been the extraction of the fecula from the diseased tubers; we may add to this the bruising of the pulp, or acting upon it by means of sulphuric acid, and finally, the organic substance of the tissue not dissolved by these last operations would be applicable to the preparation of paste, for the manufacture of pasteboard or packing paper. This residue pressed and dried in the air, might be easily laid by, to be brought into use afterwards in manufactures.

Under present circumstances, our country is more favoured than neighbouring nations, for the disease of the potato is less developed among us; and the starch manufactories, which had their origin in France, are more extensive and better fitted up than in any other country; since a short time they have been

established in England, where the evil we are now engaged upon has assumed a very serious aspect.

I am not aware whether it is beneficial for us that the produce of our untouched crops, at present abundant, should be the object of the exportation now preparing on a considerable scale.

THIRD COMMUNICATION ON THE POTATOES' DISEASE.*

BY M. PAYEN.

In continuing to observe, on a large scale of cultivation, the facts relative to the phenomenon at present occupying the attention of agriculturists and political economists, I have studied experimentally many of the questions it is important to resolve, and, in the first place, those that relate to our future crops.

These questions, I acknowledge, are perhaps important, merely because we are ignorant whether the cause of the evil has not existed for a length of time among us, and if, as some persons suppose, it acts locally every year, while it requires an assemblage of extraordinary meteorogical influences to cause it to make a general attack. Instead of these hypotheses, more or less probable, but always insufficient, we should have had positive facts, if former observations had well described the special disease now extended over so large a surface.

It is particularly that we may not have to regret, at a future time, the absence of exact observation, that I consider it useful to approach all the questions that interest science and practice, while the cause is still in action.

I have a new observation to make on one of these questions, doubly worthy of attention on this account.

Can the peculiar disease be introduced into the tubes without the intervention of their stems or roots? Is it communicated by the affected tubes to those that are healthy? Some observers have answered in the negative, many in the affirmative; but have these last confounded the transmission of the effects of the succeeding putrefaction with the disease, or even with the peculiar influence, the lowering of the temperature produced upon the stems and tubers in the month of August.

In this state of doubt, and in the absence of all precise and detailed observation, I made the following experiment:—Ten diseased tubers of the *patraque jaune* kind were placed on a flat board, round two healthy tubers of another variety, the *vilelotte jaune*, and which had been divided in a plane passing through their axes.

The board was placed under a bell glass in an atmosphere saturated with moisture a a temperature of from 68° to 82°.

At the end of eight days no sign of the transmission of the disease was perceptible; four days later, a change was manifest on the cut surface of one of the sections of the tuber; this surface appeared dry and white like powdered starch.

I made my observations under very favorable circumstances for establishing the nature of the change, for I took advantage, for that purpose, of the presence of MM. Decaisne and Melsens, who were kind enough to review, along with me, the principal facts mentioned in my preceding communications, and to compare the observations by means of three excellent microscopes, made by MM. Brunner, Chevalier, and Oberhausser.

I will add, while on this subject, that M. Melsens having an idea of submitting the orange yellow organism extracted by my process to the action of concentrated and boiling hydrochloric acid, the details of the structure of this organism became more clear and perceptible, by means of a magnifying power of 1,000 diameters. M. Decaisne made a drawing of this, under the microscope, which I have added to my sketches.

Returning to the examination of the cut tuber, which had been subjected for twelve days to the presumed influence of the sporules, I must observe that the parts which presented the powdery appearance, were composed, in fact, of fecula freed from their cellular envelopes.

The broken portions of the cells were found among this white and inert mass.

Beyond that, on the margin of the white mass, were found organised bodies of an orange-yellow color, like those that appeared to represent the head of the fungus.

In this case the attack of the parasite took place without direct contact, and simply by the agitation I gave to the air by raising and replacing the bell several times every day; the penetration was inverse to that I had observed in the growing tubers, for it was from the centre towards the periphery.

It is a fact worthy of notice, that the energy with which cryptogamic vegetation, like that which attacks the tubers in our fields, can reproduce itself on a divided potato, exhaust its juices locally, disintegrate its cells, lay bare the fecula, without, as yet,

* *Comptes Rendus.*

attacking it, and reduces the mass to powder to such an extent, that if similar phenomena of parasitic vegetation could be regulated at will, and arrested in time, it would form the elements of a profitable occupation.

A single instance of transmission between tubers is certainly insufficient; it tends, however, to justify one of the precautions recommended in my first communication, and will explain the direct invasion of the disease, without the intervention of the stem, noticed in certain cases, but which I have not myself met with.

For the purpose of discovering, comparatively, whether the transmission would take place without the influence of much moisture, I surrounded three healthy tubers of the same variety, one of which was cut in two, with twelve tubers greatly diseased. I placed these last nearly in contact with the first, covered them with dry hay, and put them in the same spot, the temperature of which varied from 68° to 83°, but without the addition of water. I had, on the contrary, contrived an easy passage for the vapor by means of a gentle current of air; at the end of twelve, or even fifteen days, no appearance of cryptogamic vegetation, nor any change whatever, appeared on the healthy tubers.

This fact may be added to those disclosed by other experiments, which point out, with more certainty, the influences to be guarded against, as well as the nature of the precautions to be taken to avoid or ameliorate them.

USES TO BE MADE OF THE POTATOES AND THE PULP.

A great number of the facts that have come to our knowledge, confirm the opinion which attributes no unhealthy action to the diseased tubers, so long as they have undergone no other change, if the substance has remained firm, and free from putrid fermentation. You can, however, in this case, recognise in this alimentary substance, a bitter taste afterwards, which it will be useful, perhaps, to explain.

The herbaceous tissue, beneath the epidermis of the potato, contains several substances of an acrid smell, and somewhat bitter; these characters are more strongly marked in certain varieties, whose substance beneath the epidermis is red; it is increased by the exposure of the tubers to the action of light: frequently it is not perceptible, as long as the tissue remains uninjured; but when it has been rendered permeable, as it has been remarked to be, when thawed after having been frozen, and as it must necessarily be, by the introduction and developement of liquids, sporules, and cryptogamic filaments, then the juices are poured out from the herbaceous tissue into the tissues that lie underneath, and occasion the disagreeable taste we have noticed.

With this exception, the use of the potatoes as food does not appear, as yet, to have offered any appreciable inconvenience. We may mention among those persons who have made the experiment with great care, and have communicated their observations to us, Dr. Meval and M. Decaine.

The observations on feeding animals have been numerous and conclusive, not only in what relates to the tubers, but also the pulp: the latter contains a much larger proportion of cryptogamic vegetation, which remains entangled in the conglomerations of the cells, while the greatest portion of the fecula is extracted from the tissues.

Milch cows, sheep, and pigs, have been fed in this manner, without any observable inconvenience.

PRESERVATION OF THE CROP.

Every observation agrees in proving, as we imagined would be the case, that the prompt extraction of the fecula is the best means of avoiding spontaneous loss: unfortunately we cannot, in most localities, have all the injured or doubtful tubers rasped immediately; besides, it would be desirable that a portion should be preserved as food for animals.

It is very plain that very simple and inexpressive processes, alone, can be employed in most of the rural labors of this description.

Ordinary trenching would be one of the worst plans, for the putrid fermentation spreads with great rapidity, by the contact of the tubers, even in the case of those that are sound; they would, therefore, affect the whole mass if enclosed in a trench.

If we are obliged to place the potatoes in heaps, they ought to be as small as possible, and separated from each other.

It would be better to spread them out in a single layer, if there is sufficient space.

As long as the surface of the tubers is dry, the fungi that reach them are less injurious: two or three days of exposure to the sun in the dry air, would, therefore, have a very favorable influence on their future preservation. I have remarked that washing them first, and then immersion in lime and water (a 0·05) assists their drying.

If the disposable hands will admit of it, without too much expense, the useful conditions of isolation and equable heat, without augmenting the heaps beyond measure, may be obtained, by placing alternately isolated

beds of tubers, and of sandy earth, filling up the intervals, and forming a thickness above all of 2 centimetres; these dispositions would allow the prepared trenches to be employed, or to make use of the trenches dug in the earth.

Many other means have been proposed, and from the object of examination before a commission named by the Academy, others, also, have been the object of experiments made by M. Dumas, which, no doubt, will be repeated on a large scale.

Our fellow academician, starting from the preconceived ideas of the causes of the evil, and which were found to be in accordance with the results of the investigations to which, on my side, I had devoted myself, was led to employ agents which render azotised organic matters, or albumenoïds and ferments, imputrescible and inert.

Among the least expensive, tan has appeared to succeed; stratified in layers, along with the potatoes, it absorbs the the oxygen of the air, and prevents its coming to the aid of the fermentation.

Sulphurous acid, which prevents, or suspends, all kinds of fermentations, bleached and kept in good condition injured tubers, exposed for an instant to its action in the combustion of roll brimstone: we have the means of applying this acid in a large way at a small cost.

But an unexpected result, observed during this series of essays, deserves immediate notice, for the purpose of preventing agriculturalists falling into error.

Among the antisceptic agents, sea salt, so favourable, in other respects, to nutrition, was recommended by many persons, who certainly had not tried it. M. Dumas thought that, in the first instance, it certainly would be proper to notice its effects on the diseased tubers, and he observed that this substance, in small quantities, hastened, in a most extraordinary manner, the putrefaction of the tubers which had been attacked.

Among the accessory causes of the destruction of the potatoes, must be reckoned the attacks of many insects: all, however, do not perform the same part. Our fellow academician, M. Rayer, having presented to the Central Society of Agriculture those which he had most commonly found at work in the vicinity of the tubers, it resulted from these observations, as well as from those of M. Gnein-Meneville, that the most common of these insects was the *julus guttulatus*, a myriopod, which also attacks every kind of fruit, and divers vegetable products; another belonged to a considerable group, the numerous species of which busy themselves in destroying fungi, and cryptogamous plants in general; and lastly, a third appears to represent the larvæ of one of the brachylytrous colcoptera, which are all carnivorous.

In addition we find, on fungi, and in obscure places, where cryptogamous plants vegetate, a great number of species of brachelytri assembled to feed on the insects that are born and live among fungi.

PRESERVATIVES AGAINST THE DISEASE BY WHICH THE POTATO CROPS ARE AT PRESENT ATTACKED.*

BY M. V. PAQUET.

M. Victor Paquet believes, according to the observations he has made, that the disease of the potato is the result of the development of a little microscopic fungus, of the order Puccinia. In 1831, the same parasitic plant produced the same effect on the anemone, as it has done this year on the potato.

To stop the growth of the fungus, M. Paquet tried the following plan: about 25 decilitres of potatoes, in which the disease had commenced, were separated into two equal lots; one of these was sprinkled with quicklime, to which about one-fourth the quantity of soot and powdered charcoal were added. The lot thus prepared with lime was placed in a chest. The other lot, which underwent no preparation, was placed in another chest, and the two boxes put into the cellar. By the twelfth day, the tubers without lime were completely gangrened, and already in a state of fermentation. The others, on the contrary, were as sound and dry as in the preceding year.

EXPERIMENTS ON THE EMPLOYMENT OF AMMONIO-MAGNESIAN PHOSPHATE AS A MANURE.†

BY M. BOUSSINGAULT.

The numerous analysis which I made during my experiments on soils, pointed out to me a remarkable connexion in the association of many of the elements that enter into the constitution of plants. Thus magnesia, the earth, which is believed to be so injurious to vegetation, is constantly met with in ashes, and its proportion always bears a certain ratio to that of phosphoric acid; and thus we are led to perceive that the mineral parts of wheat, maize, and leguminous plants, often contain phosphate of magnesia.

* *Comptes Rendus.*
† *Ibid.*

On the other hand, when we examine attentively the whole of the component parts of an alimentary vegetable substance, we perceive an evident connexion between the azote and the phosphoric acid, which seems to point out that in vegetable organization the phosphates belong more particularly to the nutritive azotized principle, and that they follow it even in the organism of animals which assimilate it.

These considerations, on which I have already insisted in my "Rural Economy," have induced me to make a trial of ammonio-magnesian phosphate as a manure.

This salt, in fact, contains the elements which appear to be the most necessary for the development of plants—phosphoric acid, which, in the state of a phosphate, forms a portion of all seeds; magnesia, which is always met with in ashes, and ammonia, which assists, in connection with the azote of the atmosphere, in the production of azotised substances. In addition to this, ammonio-magnesian phosphate possesses one of the properties that belong to gypsum; and that which is most satisfactory in its employment, is its feeble solubility. A salt that is very little soluble, if it be in its nature useful, cannot become injurious in its amount, because the water which must be absorbed by the roots takes up but a very small quantity.

The 1st of May I placed a quantity of arable earth in several clay pots, each of which held 15 cubic decimetres. These vessels were divided into two series: in the first, I added to the earth of each 16 grammes of ammonio-magnesian phosphate, and a seed of early maize, which had already germinated (quartantain maize). All the plants were placed in the open air, and when the dryness of the weather required it, they were watered with the same quantity of water.

During the fifteen days that followed their appearance above ground, the plants all appeared alike, and were equally vigorous; it was only after the twenty-fifth day I began to perceive a difference, which was always maintained after that date.

The 25th July, the plants of the first series, those which had received the phosphate, were double the height and thickness of stem; was triple that of the height and diameter of the maize, that rose from the earth in a normal state.

The 25th August these proportions were not exactly the same: the maize of the first series had once and a half the height, and twice the diameter of the maize in the second series.

All the plants blossomed and formed their ear at the same time. Those which had been developed under the influence of the double salt, had two perfect ears, and one abortive ear; the others had one perfect ear and one abortive. I may add that this last was the usual state of the maize cultivated in the open ground this year. The grain of the first plants weighed 2½, and that of the second 1.

I generally attach but little importance to experiments in agriculture made on a small scale; nevertheless, in the case of a new manure, I am in the habit of trying it, in the first instance, as I did in the present case, before I make the experiment in the large way. On this account I should not have communicated the results I have now related, if they had not been so particularly decisive. I had, before this time, made experiments, both in the large and small way upon many kinds of manure; but I never yet obtained such striking differential results.

Next year I intend to make experiments on a large cultivation, the only kind, in fact, that would be fit to furnish facts in rural economy. For this purpose I have entered into arrangements with M. Shattenmann, who so skilfully superintends the manufactory of chemical products at Bouxwiller. As a result of the manufacture of glue from bones, M. Shattenmann has at his disposal a considerable quantity of phosphate of lime, ready dissolved in chlorohydric acid, and it would be, consequently, easy to separate the phosphoric acid. This acid, once extracted, the preparation of the double phosphate can no longer present any serious difficulties in an establishment where ammoniacal salts are produced, and which is, also, in the neighbourhood of a large deposit of *dolomic*. If, in other respects, as I am much inclined to believe, the double phosphate offers a real advantage over the employment of other ammoniacal salts, as a manure, it is evident that it will be an easy task to convert guano into ammonio-magnesian phosphate, by the intervention of magnesia, one of those substances which are most generally distributed over the surface of the globe.

DESCRIPTION OF A NEW METHOD OF PRESERVING SUBJECTS IN NATURAL HISTORY.*

BY M. GANNAL.

Let a box be made of extremely thin deal, about 4 feet 6 inches in length, 3 feet wide, and 1 foot in height; this box, which is extremely light for its size, is to be entirely covered with paper, pasted over the outside.

* *Comptes Rendus.*

This box has a double bottom, made of two thin iron plates turned up round the sides to the height of about an inch. On the lowest plate I spread a layer of fine wet sand, not quite an inch in thickness; I then fix my box, and fill up the double bottom with sand.

The whole apparatus rests upon two little trossels, and underneath, below the centre of the iron bottom, I place the brazier that is to heat the apparatus.

In the inside of the box I place a plate of the size of the bottom, resting on brackets; on this plate the objects to be submitted to chemical action are arranged.

When everything is thus prepared, I place in the box the quadrupeds, birds, and insects, I wish to purify; then I put on the cover, and fix it on by means of strips of pasted paper.

The box is pierced with three holes, the first on the side, and near the bottom, in which I fit a retort holding 2 quarts, containing about 2 pounds weight of glass coarsely broken; a straight tube is adapted to the upper opening of the retort, and plunged in to within an inch of the bottom; the retort itself rests upon a sand bath, placed over a brazier.

In the cover I make two holes, one to receive a thermometer, half the length of which is plunged into the box; the other hole remains open, to allow the air in the box to escape.

Everything being thus prepared, I light the fire beneath the double iron bottom, and, when the thermometer begins to rise, I light the fire beneath the retort.

When the thermometer has risen to 84°, I increase the fire under the retort, and introduce spirits of turpentine by means of the straight tube, by small quantities at a time, until, in the space of an hour-and-a-half or two hours, I have distilled about one quart or three pints, according to the number and size of the objects in the box. I take great care to conduct the operation in such a manner that the thermometer shall not rise above 150°.

When I consider the operation is concluded, which I ascertain by the smell of the essence rising through the hole that is left open, I close the holes, withdrawing the retort and the braziers, and leave the box for eight and forty hours. At the end of that time I remove the cover, take out the objects, and replace them in their cases.

It is readily seen that a temperature from 140° to 158° must destroy the larvæ, the animalcules, and the eggs in the preparations. On the other hand, at this temperature, the pores of the wool, the feathers, and the skin, will open, and become impregnated with the vapour of the essence, which remains fixed after it has become cold, and sufficient to preserve them from a fresh attack. Besides, the quantity of the essence is so trifling, that it is impossible to discover whether the most delicate bird or butterfly has been subjected to the operation.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

POISONING BY AMERICAN FLOUR.*

BY WILLIAM TAIT, M.D.

About one o'clock on the morning of the 28th September, 1844, I was called to the house of a respectable victual-dealer in town, where several members of the family were reported to have become seriously unwell. On my arrival, my attention was first directed to the mistress of the house, a corpulent-looking female, about forty-seven years of age. At this time, she was lying in bed, supported by one of her daughters, and presented a countenance which was pale and collapsed, cold extremities, and a quick feeble pulse. She seemed to comprehend every thing which was said to her, but wa so oppressed as to be unable to return any answer to the questions put, otherwise than by a slight inclination of the head. The pupils were rather contracted, and very sensible to light. The tongue was covered with a thick frothy mucus. Thirst was incessant, and fluid was only swallowed a very few minutes before it was rejected. Her agony always appeared much increased before vomiting. This was evinced by a constant rolling of the head and eyes, a twisting of the body, and a convulsive pressing with both hands over the umbilical region. The matter vomited in my presence appeared to be simply the fluid which was swallowed, mixed with mucus, and a few streaks of

* *Northern Journal of Medicine*, September, 1845.

blood. She could not tolerate the slightest pressure over the epigastrium or other part of the abdomen. The bowels had been repeatedly opened, but unfortunately the stools had not been preserved, and I had no opportunity of obtaining any exact information regarding them.

Besides Mrs. J., Mr. J., two daughters, and three sons, the youngest of whom was fifteen years of age, were affected in a similar manner. The symptoms in all, however, were not exhibited in the same degree of intensity; and whilst they were nearly similar in their origin and progress, some of them were altogether absent. Mr. J., for example, had no vomiting, although he suffered more from sickness and tormina than any of the others.

So many individuals exhibiting similar symptoms in the same family, led me at once to suspect that all must be attributable to the same cause; and this, I had no doubt, was of a poisonous nature. On inquiry into the history of the cases, I learned that, besides broth and meat, the family had, on the preceding day, partaken of an apple-dumpling, the paste of which had been made of American flour. This, from its fine appearance, was considered to be of a very superior quality. Soon after dinner, a heavy uneasy sensation was felt in the stomach. This was accompanied by great sickness and inability to sit in an erect posture, which in some members of the family was soon followed by vomiting. Two of them, who got speedily rid of the contents of the stomach, recovered quickly. In four, vomiting did not take place before five or six hours had elapsed. In these the sickness was accompanied with lancinating pains in the stomach and bowels, and a burning heat in the epigastrium, a dry sensation in the mouth, thirst, pain in the forehead, and restlessness. When vomiting took place, relief was obtained for a short time, until it returned. The vomiting was succeeded by purging, general depression, paleness, and collapse of the countenance. In all but one young man, the pulse was quick and feeble, and in him it was full and bounding. He complained much of headache, and his eyes were prominent and congested.

The treatment pursued was varied according to circumstances. Vomiting was encouraged by draughts of tepid water in some; sedative draughts were administered in others; and warm water enemata to those who had still a desire to go to stool. A vein was opened with complete relief in the young man with the full pulse; and the mistress of the house, besides receiving sedative draughts to allay the irritation in the stomach and bowels, had bottles of warm water applied to different parts of the body to preserve the heat, and a large sinapism over the epigastrium. In two hours from my arrival, all had experienced almost complete relief, and some of them had fallen asleep. Next day all had headache and thirst, but these symptoms in twenty-four hours after had entirely disappeared. The mistress of the house was the only one who appeared to have permanently suffered. For several weeks she continued pale, and suffered from irritability of the stomach and bowels, and is still in an infirm state of health; but her period of life may also have had its influence in producing some of the ailments of which she complains.

The next question to be considered is, what was the cause of these symptoms? That it was something in the flour will appear probable from the facts, that some members of the family dined solely on the broth and meat which was at the table that day, and were in no way affected; and those of them who partook of the apples, and did not taste the paste, also escaped with impunity. Mrs. J., who was much worse than any of the others, took none of the apples, but dined almost entirely upon the paste. In, short, after the fullest enquiry, I found that the severity of the symptoms was in direct proportion to the amount of the paste which had been taken.

Previous to making use of any of the flour referred to, Mr. J. had sold two stones of it to two different parties; but as these were both strangers, he had no opportunity of finding them out, or ascertaining whether its use was followed with any bad consequences.

A careful analysis by myself, and my highly accomplished friends, Messrs. T. and H. Smith, chemists, Duke Street, made separately, showed no trace of mineral poison; and the only other conclusion left for me to adopt is, that some acrid vegetable poison was mixed with the flour. The symptoms did not exactly resemble those which are said to follow the taking of the Darnel grass, but approach them more nearly than those of any other poison with which I am acquainted. In two members of the family, along with their irritant effects, they seemed to act also on the nervous system. Mrs. J. was reported to have fainted twice before I saw her. She had a good deal of rolling of the eyes and twitching of the muscles and headache, which continued for a considerable time. All had headache next day; and one had very decided symptoms of congestion of the brain at the time, which was relieved by venesection. These con-

jectures, however, are merely thrown out for the purpose of directing attention to the subject, in the hope that some one with more time at his command, than I have, and better qualified for the inquiry, will endeavour to give a more satisfactory explanation than that which I have so imperfectly attempted to submit.

OBSERVATIONS ON HYPOSULPHITE OF SODA, OR SULPHITE-SULPHURET OF SODA.*

BY E. MOUCHON.

THE *Abeille Médicale* for the month of June, contains an article on the hyposulphite of soda. This work deplores, as we do, the complete neglect to which this powerful agent has been subjected, and, at the same time, applauds the praiseworthy efforts that have been made with complete success by the most distinguished dermatologist of the present day, to revive the credit of an article which the praises of Chaussier and Biett ought to have protected from any unhappy influence.

While we congratulate Dr. Quesneville for the happy idea, which first occurred to him, to restore to notice anti-herpetic, so deserving of recommendation, let us blame him for having made the plan he adopts in the preparation of his syrup of hyposulphite of soda his own exclusive property, and let us beg of him to allow us to establish a formula which shall become the property of all.

The hyposulphite of soda is, by its nature, easily subjected to this species of transformation. Its great solubility in water permits us, in fact, to have recourse to that method without the least fear of failure, and every pharmacopolist, who will give himself the trouble to tax his intellect for an instant, will be able to establish a rational formula. The following is one which I think as good as that of M. Quesneville. It is extremely simple, so that it requires but an instant to reflect upon it and put it in execution,

℞.—Hyposulphite of soda 45 parts.
Distilled water .... 445 ,,
Sugar in coarse powder ..... 1,000 ,,

Let the hyposulphite of soda be dissolved without heat in the distilled water, add the sugar to this solution, and dissolve it at the ordinary temperature of the air, or by means of a sand-bath, then filter the syrup through filtering paper.

Thirty grammes of this syrup will contain one gramme of hyposulphite, and this will enable you to dispense from thirty to one hundred and twenty-five grammes in the course of four-and-twenty hours.

No inconvenience appears to be attached to the use of the hyposulphite of soda, and this consideration, in addition to the incontestible advantages that result from its efficacy in cases of exanthemetic affections, painful irritations of the skin, slight congestions of the viscera, scrofula, lymphatic diseases, &c. This consideration, we say, ought to be an additional element of success for this preparation of sulphur, which more strongly fortifies us in our good opinion of its future favor. As to syrup in particular, it appears to us very right to favor the employment of this agent, which we also recommend with greater earnestness because it is at present extremely easy to have it always of a well defined strength, and perfectly pure, thanks to its application to the purposes of daguerreotype processes; for it must be well understood that the state of oblivion into which it had fallen, was chiefly caused by the impure manner in which it used to be prepared.

* *Journal de Chimie*, Sept., 1845.

NOTE ON THE ABSORPTION OF EMETIC TARTAR AND THE ELIMINATION OF ANTIMONY BY THE URINE.*

BY MM. MILLON AND LAVERAN.

AT the sitting on the 12th August, 1844, we had the honor of drawing the attention of the Academy to several experiments on the absorption of medicaments, and their elimination by the urine. Since our first labors, we have continued our observations in the hopes of discovering some useful indications, when we should have connected by degrees, as far as medical observation and chemical analysis would permit, the physiological circumstances of their absorption, and the time and duration of their elimination by the urine.

We shall shortly communicate the results we have obtained, by applying this method to the exhibition of emetic tartar. On the present occasion we shall separate from our complete work a particular fact, which has been exhibited in connection between the animal organism, and this medicament. This fact has particular interest in regard to every important point in toxicological researches.

In a series of numerous observations,

* *Comptes Rendus.*

when our patients had taken emetic tartar once, or twice at the furthest, in the usual dose of one decigramme, which sometimes was raised in particular cases to three decigrammes, we noticed, first, that the antimony was constantly found in the urine. The elimination of the metal was, in many cases slow, and we had, therefore, to follow its passage in the urine, not only for some days after it had been taken into the stomach, but some days after it had ceased to show itself in the urine. We have thus seen the antimony reappear, following a true intermittent state in its elimination, and remaining in the animal economy for a time beyond all previous calculation.

The cure of the patients and their leaving the hospital were opposed to our giving the absolute time during which antimony instals and fixes itself in our organs. Nevertheless, in the case of two patients, we were enabled to find it twenty-four days after it had been administered. One of these patients who died from a phthisis of long standing, retained metallic antimony in his body, which was proved to exist in his liver, when submitted to chemical analysis. We have also extracted from our book of experiments a case in which antimony has been recognised in the urine at the expiration of twenty days; two other cases after nineteen days, three after sixteen, and seventeen after eighteen days.

Desirous to facilitate the researches these first facts may give rise to, we decided on pointing out, in continuation, our mode of proceeding to prove the presence of antimony. We added ten cubic centimetres of pure and smoking hydrochloric acid to one decilitre of urine; we mixed the two liquids together with a little piece of well-cleaned tin, which remained in the acid urine. The tin becomes black at the end of a few hours, if the antimony abounds; but it is necessary to wait three or four days when the quantity of antimony is small. An ordinary temperature is sufficient, but the deposit is sensibly quicker on warm days. You must plunge a fresh piece of tin into each new quantity of urine, for fear of seeing the antimony arising from a previous precipitation reappear, notwithstanding the cleaning and scraping of the tin.

Before we leave the fact of permanence, we must add that the periods of intermission which are observable in the elimination of antimony, are longer in proportion to the length of time that has elapsed since it was administered. The interval, which in the beginning never exceeds two or three days, lasts from six to seven, when the time of taking the antimony into the stomach dates eight or ten days back, and the time for the antimony's remaining is sensibly prolonged when the dose has been repeated twice. It was under the latter circumstance we were able to detect the presence of the metal after four and twenty days. What would be the limits, if the organs had received arsenic, which, as all toxicologists acknowledge, is eliminated more slowly than antimony? When instead of a weak dose repeated twice at the most, the quantity of the medicament, or poison, shall have been great, and the exhibition of it reiterated.

The fact of intermittence attracted all our attention, and we are not without some hopes of being able to establish tolerably extended relations between this peculiar progress of the elimination of a metal, proved by chemical analysis, and the completely parallel intermittent progress, of many phenomena, that frequently occur in pathology, and still are very obscure. But it will be by producing and revising all the facts which attach themselves to the passage and elimination of antimony in the animal economy, that we shall have the right to raise this discussion.

THE DANGER OF ARSENICAL INJECTIONS IN BODIES INTENDED FOR DISSECTION.

The competition that has recently taken place for the situation of Superintendent of the anatomical labors of the Faculty of Medicine at Montpellier, has shown the danger arising from the employment of arsenic in the preservation of bodies intended for dissection.

Five of the candidates engaged in the dissection of bodies which had been injected with an arsenical solution, were not long before they became a prey to symptoms of a more or less dangerous character. Some were affected in the head with giddiness and dimness of sight, and the intellectual faculties became impaired and painful; others suffered praticularly in the gastro-intestinal organs: violent pains, colic, diarrhœa, nausea, and vomitings affected them on the second day of their labour, and were followed by a feverish wakefulness.

In the case of them all one symptom was exhibited which we may call pathognomonic, so much was the characteristic it exhibited uniform and special; all the candidates experienced a most violent shooting and continued pain at the extremity of the fingers. The principal seat of this pain was in the fleshy part of the fingers, and even within the circumference of the nails it seemed impossible for

any of the candidates to use an instrument of any description. An examination of the fingers exhibited a swelling at their extremity, and an injection, visible through the substance of the nail, resembling an ecchymosis, and in addition to this a violent pulsation in the collateral arteries; the nail, after a time, completely separated itself from the surrounding tissues, wherever the ecchymosis existed.

CONTRIBUTIONS TOWARDS THE CHEMICAL HISTORY OF THE GRATIOLA.*

BY M. EUGENE, MARCHAND, OF FECAMP.

THE gratiola (gratiola officinalis, L), *hedge hyssops*, is a plant endowed with purgative powers of the most energetic nature, and on this account it is sometimes used as a popular medicine, under the title of "the poor man's herb." Looking at it as one of the most active and most dangerous plants that grow in France, we ought to be astonished it has not, long since, attracted the attention of pharmacopolists and chemists; for one only, the learned Vauquelin, published, in 1809, the result of his inquiries into its constitution; but the means science, at that time, had placed at the disposal of the chemist were far from reaching the perfection and precision organic analysis has attained since that epoch. We must not, therefore, be astonished if plants, analysed at the beginning of this century, exhibit to the chemist of our day, who analyses them anew, principles of which his predecessors were far from even suspecting the existence. Having mentioned this to justify myself for having ventured to undertake, afresh, the analysis of a plant that merited the honor of being examined by Vauquelin, I shall endeavour at present to demonstrate, that the extremely bitter *resinoïd* matter, known to exist in the gratiola, and to which its properties are attributed, is not the direct principle, but a complex substance, a part of which, only communicates to it its medicinal energy.

But, before we proceed further, let us examine the list of the principles Vauquelin detected in this plant. These were an extremely bitter resinoïd matter, an animal matter, a brown-colored gum, a vegetable acid, which Vauquelin believed to be either malic or acetic, and which was in combination with soda and lime, phosphates of lime and iron, oxalite of lime, chloride of sodium, silica, and woody fibre (*Vauquelin Bulletin de Pharmacie*, t. i. p. 481).

* *Journal de Chimie.*

It may be observed that Vauquelin has not mentioned tannic acid among these principles, and yet it exists in the gratiola. This was because at the time Vauquelin undertook his task, men had extremely vague ideas of the constitution of organic substances, and on the characters they could offer in their reciprocal combinations. Although science has not as yet had the last word on the subject, it is at least permitted to us, in a host of instances, to dissect nature in a better manner, and thus be able to discover laws and substances she so obstinately conceals from us.

The whole of the tannic acid that exists in the gratiola is found in the resinoïd substance obtained by Vauquelin, and it exists, in combination with other principles, among which is found one at least which ought to take its place near digitaline. In fact, if we dissolve in alcohol the resinoïd matter of Vauquelin; if we add to the liquid sulphate of iron, until it becomes tinged with blue; if we add to this mixture as much hydrate of lime as is necessary to saturate the free sulphuric acid, and decompose the excess of sulphate of iron that was used; if we mix the alcholic liquor with the water, for the purpose of afterwards filtering it (after having bleached it by means of animal charcoal, if necessary); then, if the liquor is evaporated in *vacuo*, or rather in a situation where the aqueous vapor shall be constantly carried off by concentrated sulphuric acid, we shall obtain as a residuum a white matter; but still a compound substance.

This substance, by the addition of water, is partially dissolved, and gives an excessively bitter taste to the liquid. The intention in adding the water is to remove the mineral saline particles that are often, if not always, mixed up with the bitter principle of the gratiola, in this first part of the operation.

What remains after being washed in water is a substance easily reduced to powder. When hydrated ether is added to this, it softens, and is partly dissolved; while the portion that remains undissolved collects at the bottom of the vessel in the form of a syrup; this liquid separated from the layer of ether that floats over it, and dissolved in alcohol remains unclouded, and, if after this having been mixed with the alcohol, it is left in the open air, we obtain, when the liquid has evaporated, a white mass, in which you may perceive a number of minute globules, resembling papillæ. This substance, in my opinion, bears the same relation to the gratiola, as the digitaline does to the digitalis, and I think I am authorized in possessing for it the name gratioline, or, better still, gratiolin, to show that the substance is not an alkaloïd, for the termination *ine* ought, in

my opinion, to be reserved exclusively for that class of bodies.

Whatever may be the fate of this proposition, I shall no longer dwell upon it, but continue the enumeration of the characters this new principle presents.

It appears under the form of a white mass, made up of minute grains; it is to a very small extent soluble in water, to which it, nevertheless, imparts a very bitter taste, which, as in the case of digitaline, is particularly sensible at the back of the mouth; a small quantity of alcohol renders it rather more soluble in this liquid; alcohol dissolves it easily, and the liquid becomes milky when mixed with water; it is, to a certain extent, soluble in ether.

Placed in water, and submitted to the action of heat, until the water boils, it softens and collects on the surface under the form of a small olly drop.

Placed in a capsule of platinum, and heated in an open fire, it softens, swells, melts, becomes colored, is inflamed, burns with smoke, and at last leaves a residuum of black charcoal, which disappears by continuing the calcination. But I must still observe that, after this calcination, a small quantity of white ashes remain.

Sulphuric acid turns gratiolin yellow, and, while dissolving, it acquires a purple color; the solution does not turn green when mixed with water, but it makes it rather cloudy and becomes discolored.

Allow me, in connexion with this reaction, to offer a few reflections on the red color many bodies assume under the influence of concentrated sulphuric acid. It is sometime since M. Raspail pointed out, as a property characteristic of sugar, the power that substance possessed of developing a beautiful red color, when, after being mixed with albumen or oil, it is submitted to the action of concentrated sulphuric acid. More recently still, M. Pettenkofer observed, that when, in this experiment, you substitute biline for the albumen or oil, the red color still appears. As early as in 1838, the first of these two chemists, speaking of the red color salicine always assumes when in contact with sulphuric acid, gave this opinion, that salicine might perhaps be merely a combination of sugar, and a principle which he denominated *resineux*, and which in this case might answer the purpose of exciting the characteristic reaction of sugar. Since that time, distinguished chemists have added their authority to the opinion of M. Rospail; for, in 1839, M. Liebig was led to consider salicine as a combination of grape, sugar, and saliretine. More recently, M. Piria was induced, in his turn, after a series of very remarkable experiments, to consider the same body (salicine), as a combination of sugar with another substance, discovered by himself, and to which he gave the name of saligenine. As we see, then, let the constitution of salicine be what it may, chemists always agree that sugar is one of its elements, and the opinion of M. Respail is entirely confirmed.

Passing, then, to digitaline and gratiolin, which also assume a beautiful red color when in contact with concentrated sulphuric acid. Recollecting that these bodies, like salicine, are neutral substances, would it not be right to presume that sugar enters also into their constitution? I know that this question can only be resolved when we well understand the elementary composition of these substances; but it is a subject of study which, it appears, must lead to interesting results, and it would be desirable that chemists placed in favorable situations to undertake such a task would busy themselves with it. I am persuaded they would be amply repaid for their trouble.

After this digression, I again take up the description of the chemical characters of gratiolin.

Nitric acid dissolves it without discoloring it.

Chlorohydric acid dissolves it, and it assumes a yellow color.

Caustic potash communicates a dirty green color to it, which afterwards passes into a greenish yellow and finally into white.

Caustic ammonia communicates to it a blue color, well marked, but without being beautiful. The reaction quickly takes place, and in a short time the mass again becomes white. It is useful to observe that, under the influence of these two agents, gratiolin does not dissolve in the alkaline liquor.

The extract of oak galls precipitates gratiolin from its watery solution, if the latter is perfectly neuter or slightly alkaline. If it is alkaline the precipitation does not take place, for the tannate of gratiolin is soluble in liquor ammonia.

The etherous liquid of which I have spoken above, having been evaporated, has left, as a residuum, a white uncrystallised substance, slightly bitter, insoluble in water, soluble in alcohol at 104°, capable of being precipitated from this solution by water, soluble also in ether. This substance is acted on by fire in the same manner as gratiolin, from which it differs, however, inasmuch as concentrated sulphuric acid slowly dissolves it, acquiring a yellowish-brown color, slightly tinged with red. But everything induces me to believe that the color developed under these circumstances would be simply yellow, if the substance on which we are engaged were not ac-

companied by a small quantity of gratiolin which, as we have already seen, is slightly soluble in ether.

In a forthcoming paper, I shall return to the chemical and therapeutical properties of gratiolin, and give the result of my inquiries into the chemical constitution of the gratiola.

PECULIAR ODOR OF THE BREATH, PREVIOUS TO DEATH, IN CERTAIN DISEASES.*

BY E. J. QUEKETT.

It must be evident to most persons that for a short period previous to death taking place, a peculiar odor, somewhat resembling phosphorus, is occasionally manifest in the breath of certain patients, which is not to be detected in all instances.

My attention was first directed to the fact about sixteen years since, whilst being present at the death of a man, who, being kicked in the abdomen by a horse, died of peritonitis, on which occasion this phosphoretted (?) odor of the breath was so remarkable, that it made some impression on my mind as to its singularity.

Not a long period after the first case another instance occurred, in which it was likewise apparent; these circumstances led me to smell the breath of several persons, after, who were approaching dissolution from various diseases, but in these I was unable to detect the same odor as in the former.

In the next case that occurred, it was again remarkably evident; this was an instance of a young woman dying of disease of the uterus and peritoneum, about ten days after delivery.

On reviewing the several cases in which the fact had been manifested, it was evident that disease of the abdominal organs was present on each occasion, but in those cases in which the odor was not apparent, the abdominal organs were not diseased. This view of the matter induced me to carry on the observations on all subsequent cases of disease, and I have found that this peculiar character of the breath is always present in death, when the disease attacks the peritoneal or mucous coats of the abdominal viscera.

From the observations made, it appears that age has nothing to do with its occurrence, as cases are to be met with from infancy to old age; neither has the sex, it being equally frequent in one as in the other. It is particularly evident in fatal acute inflammation of the peritoneum, and becomes apparent when collapse is indicated; but in chronic inflammation of the mucous membrane of the intestines, it is often discernable three or four days previous to death, and whenever it is to be detected, I have always noticed that it is as certain a sign of the doom of the patient, as the stopping of the pulse tells us that life has become extinct. It is occasionally present in phthisis, apparently only in those instances where the intestinal tube is implicated, and diarrhœa has existed, together with disease of the lungs; and it is not improbable that in phthisical patients, when the odor is very marked, that the intestinal disease is the most prominent in the cause of death.

On certain occasions I have remarked the breath to be so strongly smelling like phosphorus, that it is not improbable that it would have appeared luminous, if proper means had been taken to exclude the light from the apartment.

The opportunities that I have had of observing this fact have been tolerably numerous, but still further observations are required to throw more light on the cause of disease of the abdominal viscera, having more than others the effect of producing certain abnormal changes in the respiratory functions, which other fatal diseases appear not to do.

With the view to obtain more information, I give this outline of my observations in order to induce other practitioners to add further facts, so that the phenomenon may be better understood, and probably be made of some practical importance in the treatment of those diseases which contribute towards its production.

* *Lancet.*

MODE OF DETECTING MORBID BILE.*

In some diseases, however, as in cholera, &c., Dr. Heller has found that the coloring matter of bile may undergo a very considerable morbid change, in consequence of which, when treated with nitric acid, it assumes at once a red instead of a green color. He has found that for bile which has undergone this change ammonia is a better and a more certain test than nitric acid; for although this latter reagent will detect it when in any abundance, yet it is apt to prove deceptive in cases where urine is the fluid undergoing examination, especially is much hæmatosine be present, which substance becomes more or less red by nitric acid. In using the ammonia test, a small quantity only should be at first dropped in, immediately upon

* *Medical Gazette.*

doing which a bright red color is struck; more of the ammonia may then be added until a reddish-brown fluid is obtained. In this way it is possible to detect the presence of a very minute quantity of altered bile pigment, even when nitric acid fails to afford the smallest evidence of its existence.

ON THE FREQUENCY OF ALKALINE URINE IN HEALTHY PERSONS.*

It was formerly assumed that recent urine displays an acid reaction, unless it has lost its acidity by the previous internal use of alkalies. On this account alkaline urine was unhealthy. But Wöhler showed, that not only after the use of neutral salts with vegetable acids, but even after (vegetable) acid salts, the urine was frequently alkalinescent. He assumes that these latter salts are transformed into carbonates within the body, and are brought into the urine as such, rendering it alkaline, and causing also the precipitation of the phosphates, which are only soluble in an excess of acid. Besides, he observed, that after taking abundance of fruit, particularly sweet fruits, such as cherries or strawberries, the urine became often alkaline. Wöhler concluded from the above, that salts with vegetable acids and fruit are particularly to be recommended in the uric acid diathesis. Practice has confirmed the truth of this recommendation.

The author found that, even after taking from one to two tablespoonfuls of apple-juice, the urine became alkaline within from half an hour to an hour and a half.

The effervescence of alkaline urine, observed by Wöhler, after taking great quantities of fruit, has been remarked by the author after the above small quantity of apple-juice, but more decidedly after about twelve baked plums.

As precautions for the experiments, the bladder ought to be emptied before eating the fruit, and only little water should be taken, in order that the alkaline reaction may appear prominently. Fruit exerts, of course, the same influence on the urine during illness as in a natural state, so that turbid urine must sometimes be rather ascribed to diet than to abnormal causes.

The above facts must influence considerably the diagnosis of several diseases, as,—

1. *Simple chronic nephritis*, its chief and sometimes only symptom is maintained to be: less acid, or alkaline urine, with a sediment of phosphates (particularly phosphate of lime and triple phosphate).

2. In *spinal diseases* the alkaline urine might also be occasioned by fruit, at least the author found no alkalinity in caries of the vertebræ, and whenever alkaline urine is met with in spinal diseases, it may be occasioned by a secondary disorder of the urinary mucous membrane, or by decomposition of the urine during the period it remains in the paralysed bladder. If vesical disease combines with the spinal, the urine is found alkaline.

3. *Morbus Brightii.*—The property of alkaline urine to become turbid, when heated, may easily give it the appearance of containing albumen, particularly since the precipitate of the phosphates floats at first in the form of little clouds in the vessel, like coagula of albumen. They can easily be distinguished from albumen by their solubility in acids (particularly nitric acid), by their being deposited long before boiling takes place, and by their much more rapid precipitation than coagulated albumen, consequent on their greater density.

* *The Lancet*, and *Northern Journal*.

TO COVER PILLS OR EXTRACT OF COPAIBA WITH GELATINE.*

This process, invented by M. Garot, is exceedingly easy and practicable, and it is surprising that it is not employed more generally in this country, as it much more effectually disguises the taste and odor, and interferes less with the solution, of the medicine, than the method of gilding or silvering usually practised.

It is applicable to every substance capable of a pilular consistence, such as balsam, camphor, musk, assafœtida, mercurial and ferruginous preparations, &c. Two hundred pills can be coated with gelatine in an hour, and will be ready for use after the lapse of two hours. The pilular mass so coated remains soft a much longer time than according to any other plan. We shall now proceed to describe the process:—

Fix the pills on long, fine pins; plunge them into thick, purified glue, placed in a hot-water bath; then remove them by a rotatory motion, and stick the heads of the pins in paste spread out on a slab, so that the pins may remain elevated in the air; as soon as fifty are thus treated, rotate them individually in the heat of a taper, to harden the external pellicle; pull out the point of the pin, and the process is complete.

* *Lancet*, and *Dublin Hospital Gazette*.

PROCESS FOR PREPARING VALERIANIC ACID.*

BY MESSRS. T. AND H. SMITH.

HAVING of late had considerable demand for valerianate of zinc, in consequence, probably, of a desire among medical men to test its value as a therapeutic agent, we were led to inquire whether a method of obtaining the acid of valerian more quickly and abundantly than by the usual process, so very unsatisfactory in these respects, could not be devised. The method we adopted completely answered our expectations, and as it may be of some use as a guide to other chemists, who may have occasion to prepare valerianic acid, the process is subjoined.

Let the valerian be boiled for three or four hours in rather more than its bulk of water, holding in solution an ounce of carbonate of soda for every pound of the root, and replacing the water as it evaporates. Let the liquid be strongly pressed out, and the residuum again boiled for a little, with a like quantity of water. After another pressing, let the process be repeated in the same way, a third and last time. After mixing the liquids, and adding two fluid drachms of strong sulphuric acid for each pound of the root used, let them be distilled. When about three-fourths of the liquid have passed into the receiver, let the distillate be fully neutralised with carbonate of soda, for which purpose a quarter of an ounce for every pound of the root will be quite enough. The valerianic acid can then be obtained in the usual way—viz., by concentrating the solution of valerinate of soda, decomposing it with sulphuric acid, and separating the valerianic acid, now set free, either by means of a separator or distillation. When the valerianic acid has been once obtained, no difficulty can occur in preparing any of the valerianates.

The advantage of this method over that in which the root itself is simply distilled with water, will be seen on contrasting the results of both. By the latter method, the time required was six times greater than when the valerianate of soda in solution was distilled after adding a strong acid; and although the sacrifice of time was so much greater in the first case, the contents of the still continued to smell strongly of valerian, while in the latter, not a trace of the peculiar odor of valerianic acid could be detected, showing that the principle had been more effectually exhausted. And further, the amount of acid obtained, four scruples from the pound of root, was nearly thrice as great as could be got without the use of soda.

Since the above process was adopted by us, M. S. Rabourdin, Pharmacien à Orléans, has published in the *Journal de Pharmacie*, a method suggested apparently by views of the subject similar to our own. He conceived that the valerianic acid might be partly in combination with a base, and that, by adding a strong acid, the volatile acid would be completely set free, so that, on distillation, a much larger product would be obtained. The result wholly justified his preconceptions. Five kilogrammes of the root gave from forty-five to fifty grammes of the acid, a result closely agreeing with the quantity produced by our own process.

The principle of the process suggested is very simple. In the one case an acid is presented to a valerianate, or supposed valerinate, to free it from combination with its base, by the exertion of a stronger affinity; while, in the other case, a base (soda) is presented to the valerianic acid, powerful enough to cause its effectual separation from that with which it was previously combined.

The two processes serve the same end; but it seems to us that the latter is, on the whole, to be preferred, principally because it has the advantage of presenting the acid in a state of complete solution, from which the operator can get it more easily and effectually detached, than when contained in the hard unyielding cells of the woody fibre.

* *Lancet.*

ON THE EFFECTS OF FOOD ON THE BLOOD.*

BY DR. BUCHANAN.

DR. BUCHANAN has drawn the following conclusions on the state of the blood after taking food:—

1. The serum of the blood of a healthy man fasting, is perfectly transparent, and of a yellowish or slightly greenish tint.

2. A heterogenous meal, such as that usually set on tables of the rich, renders the serum white.

3. The whiteness may commence as early as half-an-hour after eating, and may continue ten or twelve, and sometimes as long as eighteen hours, according to the kind and quality of the food, and the state of the functions of primary and secondary digestion.

4. *Starch* and *sugar*, probably all vegetable substances destitute of oil, give no whiteness to the serum of the blood.

5. *Fibrin*, *albumen*, and *casein*, and pro-

* *Medical Gazette*, Oct. 10, 1845.

bably *protein-compounds* in all their forms, if destitute of oil, give no whiteness.

6. Oils combined, whether naturally or artificially, with protein compounds or with starch, render the serum of the blood white; probably, therefore, oils produce that effect in whatever way taken.

7. Gelatin seems to render the serum of the blood white; this, however, cannot be considered as certainly established, as there may have been some fat in the beef-tea which was taken along with the calf-foot jelly in both experiments on which the above conclusion rests.

8. The coagulum of the blood very frequently exhibits, after taking food, a crust of pellucid fibrin, or of pellucid fibrin dotted with more opaque particles, and with little of the contraction technically named "cupping."

9. The appearances of the coagulum just mentioned are much more common after azotized than after non-azotized food.

These conclusions relating to the visible characters of the blood may be considered, with the single exception above mentioned, as well established. The conclusions which follow relate chiefly to the chemical properties of the blood, and are not worthy of the same reliance; but the evidence on which they rest has been laid before the reader, and he must judge of them for himself.

1. The substance defined above under the name of Pabulin,* is most abundant in the blood a few hours after taking food, sooner or later according to the rapidity of digestion.

2. It is less abundant as the time when the food has been taken is more remote, and is small in quantity after a fast of twenty-four hours.

3. It is much more abundant after azotized, than after non-azotized food.

4. It varies in quality, floating or subsiding, according to the kind of food taken.

5. It is probably analogous in nature to the white substance which gives color to the serum of the blood.

6. The difference between these two forms of this substance probably is, that it is sometimes combined with an alkaline, or earthy salt (chloride of sodium, sulphate of soda, &c.), and sometimes with an oily body (stearate of glycerine, &c). In the former case it seems to dissolve completely in the blood, while in the latter it is only partially dissolved, and renders the serum opaque.

7. The azotized principles of the food are probably made to combine, in the digestive tube, with the alkaline, earthy, and oily salts mentioned above; and thus become capable of being absorbed into the blood.

8. The alkaline and earthy compounds are probably absorbed directly by the blood-vessels, while it seems to be well ascertained that the oily compounds are absorbed through the lacteals.

* A white substance obtained by precipitation either from limpid or opaque serum, or dissolving in it common salt to saturation. Neither phosphate of soda nor bicarbonate of soda threw down this body from serum.

MODE OF PROTECTING WORKMEN FROM THE EFFECTS OF LEAD.

BY M. MEILLET.

A TINNED iron mask is fitted to the face, and a cylindrical tube is attached to it opposite to the nose and mouth; in the interior of the tube there is a metallic net-work, above which is a fine sponge saturated with a very dilute solution of hydrosulphate of soda. The sponge is so placed as entirely to fill the cylinder; it is fixed by means of a cover, also furnished with a metallic net-work; the air being thus compelled to traverse a body saturated with a liquid, which decomposes the salts of lead. All the lead vapors remain behind in the state of sulphuret. This salt is slowly and uniformly decomposed by the carbonic acid, which is always abundantly present in these manufactories. The workmen thus respires throughout the day an extremely small quantity of sulphuretted hydrogen, which of all substances neutralises the effects of lead most effectually. M. Meillet also recommends that the hands should be well rubbed with an ointment containing thirty grammes of hydrosulphate of soda to a pound of lead; the hands then wiped. These means have been tried by the advice of M. Meillet in many shot manufactories, and the results have been most satisfactory.

TO CORRESPONDENTS.

"H. O." (Southampton).—The MS. has been mislaid; but if another copy be sent it shall be inserted. Send to the office, 310, Strand.

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THE CHEMIST.

I. CHEMISTRY.

NOTE ON THE PRODUCTION OF IODOFORM.*

BY M. MILLON.

Iodoform is obtained by the combined action of iodine and the alkalies on alcohol; the analogy that exists between this combination and chloroform, allows us to imagine that it can also be prepared by replacing the alcohol by acetone, spirit of wood, or some product of the methylic series. I have not made any attempt to produce it in this manner; but I have just discovered, while in search of an expeditious method of preparing iodide of potassium that many substances can produce iodoform, when we allow iodine and carbonate of potassium to act upon them simultaneously—or, which is better still, the bicarbonate of the same base.

Cane sugar, grape sugar, the sugar of milk, gum, dextrine, and many albuminoïd substances are in the same situation.

Iodoform is produced in tolerable abundance with all these substances, although, nevertheless, they cannot, in the conditions under which I operated, succeed in advantageously replacing alcohol.

Equal quantities of iodine and bicarbonate of potash are employed, and a very small quantity of the organic substance; the iodine is added last, and by small quantities at a time; heat is applied, and the iodoform is almost immediately perceptible.

The quantity may be more or less: you will succeed very well with sugar, by agitating 4 or 5 parts of water with 1 of iodine.

When an albuminoïd substance is thus treated (as albumen, fibrin, caseïn, glutin or legumin) it is proper in the first instance to employ a little caustic potash; the nitrogenised matter is in this manner readily dissolved, and the froth that is afterwards formed by the disengagement of the carbonic acid, becomes less persistent.

The production of iodoform is very limited, notwithstanding the instances that have been adduced; I have not been able to produce it, either with resins, fatty substances, or essential oils, in the number of which I place amylic alcohol, as having furnished me with a negative result.

If we bring together the organic products, excepting the products of ultimate decomposition, which are to a certain extent common to the decomposition of sugars, gum, and alcohol, the only acids we discover are the acetic and formic, but to these we must add iodoform.

As to the products of the decomposition which are derived at the same time from hydrocarburetted substances, such as sugars, gums, &c., and from albuminoïd substances, they are already sufficiently numerous. Acetic acid exists in the products of the distillation of all of them; and an energetic oxidation of oxalic acid is engendered, on both sides; formic acid had been noticed by M. Mulder among the principles which potash disengages from albuminoïd substances. We are also indebted to M. Mulder for the very remarkable discovery of humic products, by the action of hydrochloric acid on albuminoïd substances; hydrochloric acid also engenders humic combinations at the expense of hydrocarbonised substances; iodoform thus offers a new point of resemblance to these two classes of substances, which every day take a more important part among organic phenomena. Do there exist in these two grand orders of organic principles some points derived from the same primitive grouping? Will the intimate combination of organic principles exhibit, in this instance, one of its most remarkable effects? Finally, will albuminoïd substances constitute the last degree of organic complexity, including in themselves every essential organic arrangement?

It would be easy to enter into a long discussion on this subject; but the facts which already establish an incontestible resemblance will also, perhaps, at some early day produce a satisfactory solution.

*Comptes Rendus, Oct. 1845.

A NEW PROCESS IN EUDIOMETRY FOR CALCULATING THE VOLUME OF THE ELEMENTS OF ATMOSPHERIC AIR IN RELATION TO EACH OTHER.*

BY M. LASSAIGNE.

SINCE the discovery of the component parts of the air, many methods have been employed by chemists to discover the relative volume of the constituent parts of this elastic fluid. The processes generally resorted to in laboratories, are all founded on the absorption of oxygen by various simple and compound bodies, whether at the ordinary temperature of the air, or by exciting the action, by caloric or electricity. In this manner the solution of sulphuret of potassium was formerly used, and afterwards phosphorus, hydrogen, and the binoxide of nitrogen have been employed, and of late years the proto-sulphate of iron, decomposed by potassium, has been recommended.

Dumas and Boussingault, by submitting air to the action of copper in a state of division, and heated to a dull red heat, has latterly produced an important modification which enables us to estimate, by weight, the oxygen and nitrogen which exist in the air, instead of calculating the volume of each of these gases, as was the case in old eudiometrical experiments.

In performing experiments, latterly, with protosulphate of iron, according to the directions of M. Dupasquier, and repeating the process he pointed out and published, we were led to adopt a test well known to chemists, but which, so far as we know, has never been applied to the analysis of air.

This method is founded on the readiness with which copper divided into thin tables becomes oxidised in contact with air, in the presence of liquid ammonia, and on the formation of a blue ammoniuret of deutoxide of copper.

Many successive experiments having shown us that this test, acting in a limited volume of air, in a very short time, completes the total absorption of the oxygen contained in it, leaving the nitrogen free, we were induced to think of the application of this property to the analysis of the air; and the result has been what we expected.

The application of this new method is extremely simple, and it requires the employment of no peculiar apparatus. A common graduated tube 14 to 15 centimetres in length, and 12 millimetres in diameter, and a small phial with a ground glass stopper, containing 30 to 35 cubic centimetres, are the only vessels necessary.

The process consists in introducing into the phial from 3 to 4 grammes of copper turnings, then pouring in distilled water until the phial is half full, and afterwards filling it with a concentrated solution of ammonia. The bottle, thus completely filled, is closed with its glass stopper, and inverted in the water trough, taking care that the copper turnings do not rest on the orifice of the bottle. This first disposition being arranged, you measure a volume of air in the graduated tube filled with water, and by means of a small glass funnel it is passed into the bottle, which has been uncorked under water. This being done, the mouth of the bottle is immediately closed, and it is taken out of the pneumatic trough, and shaken incessantly for eight or ten minutes. In less than a minute or two the ammonia is seen to assume a bluish tinge, which becomes gradually darker, as the ammoniuret of deutoxide of copper is formed. This blue tint assumes its maximum of intensity when you operate on from 15 to 20 cubic centimetres of air, it then becomes gradually fainter, when all the oxygen of the volume of air on which you operate has been absorbed; this gradual loss of color, which points out the close of the operation, is due to the action of the copper, in excess, on the ammoniuret of the deutoxide, which is converted into colorless ammoniuret of protoxide.

When we have arrived at this point of the experiment, the gaseous residue is passed into the graduated tube for the purpose of measuring it, taking the precautions requisite in these kinds of operations. In the various experiments we have made, by employing the method we submit to the notice of chemists, the gaseous residue, after having been subjected to the action of the copper and ammonia, contains no trace of oxygen; for if phosphorus be introduced to detect it, it gives out no phosphorescent light in darkness, nor does it produce any diminution of its volume.

The volume of the nitric gas determined by this process, has always been two or three tenths of a degree greater than that obtained by the action of phosphorus upon the air. The proportion has been 79 : 792·2. The latter number deduced from our experiment, comes very near 79·17 which MM. Dumas and Boussingault have deduced from the most laborious analysis of the air.

When analysing an artificial mixture, composed of 41·5 air, and 57·5 nitrogen, in which the proportion of nitrogen is necessarily increased to 90·2, the new method indicates 90 of nitric gas.

* *Comptes Rendus*, Oct., 1845.

The simplicity of this operation, and the short time in which it can be performed, will enable us, without doubt, to employ it in various cases.

ON THE ACTION OF ALKALINE BICARBONATES ON VEGETABLE BASES IN THE PRESENCE OF TARTARIC ACID.*

BY C. OPPERMANN.

During his experiments on the organic bases or alkaloïds, the chemist is necessarily obliged to make use of the generic and specific characters these bases possess, and which consist, among others, of the property of being separated by powerful inorganic bases and alkaline carbonates. In the latter instance, as we know, they often occasion, incidentally, combinations between the displaced and the displacing bases of such a nature that these reactions offer the means of distinguishing one from the other, on account of some being dissolved in an excess of the precipitating base, while others remain insoluble.

The phenomenon of the precipitation of bases by one another, when they are in the state of a salt, is not, however, a constant phenomenon. We know, by the experiments, of Lasonne and Rose, that many organic substances, such as tartaric acid, sugar and albumen oppose themselves to the displacement and precipitation of an oxide, to such an extent as to resist the agency of a number of tests. M. Persoz having remarked, that, like alumina, certain organic bases possessed the property of being concealed by tartaric acid, I have endeavoured to ascertain to what extent this phenomenon was peculiar to this particular substance, for the influence such a fact might have upon inquiries into vegetable alkalies is easily conceived. I have, therefore, made experiments to ascertain whether, tartaric acid, albumen, and other fixed organic bodies possessed or not, the property of preventing the separation and precipitation of a vegetable alkali. I shall in this place, merely give the result of my experiments on the action of the salts of morphia, narcotia, strychnia, brucia, quinia, cinchonia and veratria, in the presence of tartaric acid, and fixed alkaline bicarbonates. The solutions of the salts I have mentioned were made in the proportion of 1 of salt, to from 200 to 500 of water (the action is precisely the same, whether we operate upon these solutions, or on decoctions of the extracts of the plants that contain these vegetable bases, provided they are properly prepared); tartaric acid being added until the action of the acid becomes well marked, and afterwards supersaturated with one or other of the fixed bicarbonates; it appears useless to add that the acid tartrates and alkaloïds show the same results.

The salts of morphia are not precipitated by the alkaline bicarbonates, but the salts of narcotia immediately produce a considerable white easily-pulverised precipitate. If we wish to be satisfied of the complete absence of narcotine in a neutral solution, we employ sulphocyanuret of potasium; this test, which produces no cloudiness in neutral solutions containing morphia, immediately causes a precipitate of a deep rose color, in those which contain narcotine, even in a quantity too small to be detected by weight. But we must always remember that a slight excess of sulphocyanuret redissolves the precipitate formed.

The salts of strychnia, under the same circumstances, either produce easily pulverised precipitates, or furnish crystals of considerable size, according to the quantities of tartaric acid, bicarbonate of soda, or potassium employed, and according to the degree of dilution; if the solution be very weak, there is no precipitate, if it be rather less so, and if it contains much tartaric acid, it still does not form an immediate precipitate by means of the two bicarbonates, but at the end of a quarter of an hour, we see very long and thin crystals appear, whose number continues to increase until all the strychnia contained in the solution is crystallised, we may perfectly satisfy ourselves that the solution contains no more strychnia or salt of strychnia, by adding as M. Marchand has indicated, peroxide of lead, and a mixture composed of sulphuric and nitric acids; the blue color to which a quantity of strychnia too small to be weighed gives rise, in the presence of these matters, is, as we know, a most certain sign of the presence or absence of this base; for chlorine, which M. Pelletier has recommended as a specific test for strychnia, acts in precisely the same manner on veratrine, as direct experiment has proved to me.

If the solution of strychnia is concentrated and contains but little tartaric acid, the bicarbonates immediately produce a white precipitate of a crystalline texture and the liquid no longer exhibits a trace of strychnia.

Brucia and the solutions of the salts of that base, differ completely in this respect, from the salts of strychnia; the alkaline bicarbonates produce not the slightest cloudiness.

* *Comptes Rendus*, Oct. 6th 1845.

These salts of quinine and cinchonine can also be compared and separated from one another in the same manner; the quinine cannot be precipitated when tartaric acid is present, while cinchonine is precipitated by the two alkaline bicarbonates.

In the solutions of the salts of veratria, acidified by means of tartaric acid, the bicarbonate of soda alone produces a precipitate, the bicarbonate of potash producing none.

We see, therefore, that in making use of the property possessed by tartaric acid of protecting certain bases from the action of alkaline bicarbonates, we succeed in establishing two alkaloïd groups, very distinct from each other, the first of which includes those that can be precipitated by bicarbonate of soda, and these are cinchonine narcotine, strychnine and veratrine.

The second groups contains those that are protected from their action, namely, quinine, morphia, and brucia.

Tartaric acid also protects the infusion of gall nuts from the action of all these bases, with the exception of cinchonine and strychnia; but it abundantly precipitates the five others as soon as the acid has been neutralised by ammonia; it must be noticed, however, that an excess of this last base redissolves the tannate of brucia.

Besides this, we find, and this appears to me to be worthy of attention, that of the two bases which are met with in the same plant, one is constantly protected by tartaric acid, while the other is not: the employment of this method is valuable, because it allows us to separate the two bases with the greatest precision.

MEMOIR ON THE EQUIVALENTS OF SEVERAL SIMPLE BODIES.*

BY M. J. PELOUZE.

Most of the numbers which express the proportions in which simple bodies combined were established by Berzelius. For more than thirty years, the works which served as the basis of these important determinations remained unshaken; they sufficed for all the requirements of science, whose progress they assisted to an extraordinary degree. However, the state of chemical analysis at that remote period did not always enable Berzelius to attain the degree of accuracy which the perfection of the methods must necessarily bring about at a later period, and he himself foresaw that a revision of the atomic weights would one day become useful, and even necessary; this revision has been commenced. In a remarkable memoir on the composition of carbonic acid, M. Dumas demonstrated that the atomic weight of carbon deduced, by Berzelius, from the densities of oxygen and carbonic acid, was much too high, and that from 76·44 it must be reduced to 75·00.*

In another memoir, M. Dumas found, for hydrogen and calcium, numbers corresponding to 12·5 and 250; and it appeared to him that Prout's hypothesis, which regards equivalents as multiples of a body taken as unity (hydrogen), might be well founded.

This hypothesis appeared to be supported by a great number of works which followed one another in almost uninterrupted succession for two years. To carbon, oxygen, and calcium were soon added, as new multiples of hydrogen, nitrogen, chlorine, sulphur, zinc, bromine, mercury, barium, strontium, &c., &c.

The hypothesis of the English chemist seemed to be in the way of being very soon ranked among the best proved truths; but at length the difficulty of the experiments, whose object it is to fix the proportional numbers of chemistry, and the seriousness of changes so rapidly introduced into so important a question, did not escape some of the authors of which I speak, and an hypothesis, whose correctness has not hitherto been demonstrated, will have brought to science works of great precision.

Among these works, I will name, in particular, those of M. Marignac, which had for their object the determination of the proportional numbers of silver, chlorine, and potassium.

I do not think that, besides carbon, whose atomic weight was determined by a method which ensured great accuracy, there is in all the lists of simple bodies a single element, whose equivalent has been fixed with so much precision as chlorine and silver. M. Marignac seems to have left nothing to be desired in this respect; and the chemists who have read his work will, doubtless, acquiesce in the tribute paid him by Berzelius himself.

In the course of his investigations, M. Marignac particularly devoted himself to determining the equivalent of chloride of potassium by a method already followed by Berzelius. This equivalent was not a multiple of hydrogen; I made the remark in a note which I had the honor of communicating to the Academy.

I proved also, relying on the perfectly con-

* *Comptes Rendus.*

* Throughout this memoir the equivalents are reckoned O = 100—C = 75—H = 12·5.

cordant experiments of M. Berzelius and M. Marignac, and on some other analyses of my own, that, since the equivalent of chloride of potassium was not a multiple of 12·5, chlorine and potassium, or at least one of these elements, must be at variance with Prout's hypothesis, which would, therefore, appear unfounded.

About the same period, I communicated to M. Marignac my intention of applying a method, capable of great accuracy, to the verification and determination of a great number of atomic weights, inviting him, if he would be kind enough to adopt it himself, to control his own experiments on silver, chlorine, and potassium.

Chemists will have great confidence in this method, for it is in all respects the same as that which M. Gay-Lussac first proposed for the analysis of the alloys of silver by the humid way, of which fifteen years of daily practice have proved both the utility and the extreme precision.

I operate as follows:—

I take perfectly pure silver, and weigh it in a good assay balance, rapidly and easily sensible to a quarter of a milligramme; I introduce from 2 to 6 grammes of it into a flask of the capacity of about 200 cubic centimetres; I dissolve it in nitric acid; I dilute the solution with 100 or 150 grammes of water, and I introduce into the latter the metallic or metalloïdic chloride. One or two preparatory experiments indicated to me, very nearly, the respective quantities of silver and chloride requisite to be put in contact. If the chloride is solid, I carry it directly from the pan of the balance to the flask; if it is liquid, I weigh it in a small bulb of glass, which I make with a blow-pipe, and which I afterwards introduce into the solution of silver. I cork the flask and shake it, this breaks the bulb, and thus brings the liquid it contains into contact with the nitrate of silver. I clear the liquid by agitation, and I finish the precipitation with the *decimal liquor* of silver (1 gramme per litre, or 1 milligramme per cubic centimetre).

I conform myself, moreover, to the instructions given by M. Gay-Lussac in his beautiful work on the humid way. With a little skill in investigations of this kind, the error committed should not exceed half or even a quarter of a thousandth part of the weight of silver.

Indeed, in this case, there is no precipitate on a filter, to be washed, dried, and weighed; there are only two weighings, namely, those of the bodies put in contact. Their contact and reaction take place in a well-closed vessel. The term of analysis is indicated by a very simple sign, by the transparency of a perfectly colorless liquor, which a minute fraction of a milligramme of chloride of silver suffices to render turbid in the most visible manner.

The principal difficulty to be conquered is not inherent to this process; it is presented in almost all analyses; it is that always experienced in obtaining bodies perfectly pure. It is clear that the chloride employed in the precipitation of silver should be perfectly free from all extraneous matter. I have always done my utmost to attain this object, and I think I have succeeded, if I may judge from the almost complete identity of the results presented by the same chlorides obtained and purified by different processes.

Be it as it may, it is with great reserve that I present this work to the Academy, and I admit that I should not have dared to undertake so delicate a task as that of the determination of chemical equivalents, had not a two-fold reason encouraged me to make known, at this time, some results which I propose to pursue and complete. In the first place, the method which I used is not my own; I have already said that it is that of M. Gay-Lussac applied to the question of proportional numbers; and, on the other hand, M. Berzelius, in giving an account, in his last *Jharbuch*, of M. Marignac's work, mentioned this method as appearing to him susceptible of great precision.

The equivalents of chlorine and silver, deduced from the experiments of M. Marignac, and otherwise so like those found in Berzelius' tables, have served me as a point of departure for calculating the very equivalents whose determination forms the principal object of this memoir. These equivalents are 443·20 for chlorine, and 1349, for silver.

EQUIVALENT OF SODIUM.

100·000 of silver were precipitated by

I. . .	54185	of chloride of sodium.
II. . .	54125	
III. . .	54135	

The mean of these three experiments gives, for the equivalent of sodium, 287·17, and for that of the chloride 730·37.

The chloride of sodium was obtained, either by decomposing the chloride of barium by sulphate of soda, or by decomposing carbonate of soda by hydrochloric acid, the salt crystallised several times, dried at 392° F., or fused, gave identical results. I also operated directly on fine samples of rock salt, of perfect transparence and perfect purity, found in the galleries of the mines of Dilaze.

EQUIVALENT OF POTASSIUM.

The chloride of potassium used resulted from the calcination of very pure chlorate of potassa; it was crystallised: it was dried like that of sodium. The equivalent of this salt was deduced from three experiments, whose mean gave 932·50, a number closely resembling that of 93·234, formerly obtained by M. Marignac.

M. Livel, whose accuracy is so well known among chemists, found 932·59,

932·50—443·20=489·30

is, therefore, the equivalent of potassium, and this number is almost identical with

932·34—443·20=489·14 (Marignac.)

NITROGEN.

I operated once on crystals of sal-ammoniac obtained by solution in water, and a second time on a sample of the same salt sublimed.

100 parts of silver were precipitated, first, by 49·556, and secondly by 49·517 of sal-ammoniac.

The first of these numbers gives for sal-ammoniac the equivalent 668·38, and for that of nitrogen, 175·58; the second gives 667·98 and 174·78.

Mean of the equivalent of nitrogen, 175·08.

BARIUM.

The chloride of barium was purified by several successive crystallisations; after the third crystallisation, it began to give sensibly identical numbers. It was dried at 392°F. in an oil-bath, or in a tube with a lamp, at a temperature below dull red.

4gr.·002 of silver were precipitated by 3·860 of chloride of barium.

6gr.·003 of silver were precipitated by 5·790 of chloride of barium.

3gr.·001 of silver were precipitated by 2·895 of chloride of barium.

Whence we deduce for the equivalent of chloride of barium:

I.	1301·14
II.	1301·14
III.	1301·36

and for that of barium—

I.	857·94
II.	857·94
III.	858·16

Mean of three experiments 858·01

The clearing of the liquids is infinitely more rapid with the chloride of barium than with that of sodium. The chlorides of barium and strontium, in losing their water of crystallisation, do not alter, and, in this respect, they differ from chloride of calcium. Their solution, after desiccation, is without action on reddened litmus paper. Carbonic acid produces no precipitate in them. Calcined in a long tube filled with asbestos, these two salts disengage water which does not give the slightest trouble in a solution of nitrate of silver.

STRONTIUM.

The same observations as for the foregoing salt. By crystallisation, we easily obtain needles several centimetres in length, which, dried at 392°F., or over a lamp in a tube below dull red, very soon give constant results.

However, a first crystallisation gives, in general, too high an equivalent and, inversely for barium, too low a number. This appears to be owing to a mixture of these two salts.

2gr.·014 of silver were precipitated by 1·480 of chloride of strontium.

3gr.·008 of silver were precipitated by 2·210 of chloride of strontium.

From which we deduce, for the equivalent of chloride of strontium—

I.	991·32
II.	991·12

and for that of strontium—

I.	548·12
II.	547·92
Mean	548·02

SILICIUM.

The chloride of silicium used in my experiments was given to me by M. Ebelmen. It was perfectly transparent and completely colorless; it left no trace of residue after evaporation in a small glass capsule. It had remained for a long time over mercury.

After the precipitation of silver which was very easily effected by the chloride, the liquid filtered and evaporated, left colorless and transparent gelatinous silica. It appeared to me that the whole of the silica was not dissolved in the nitric acid; but the feeble proportion, which remained in suspension in the liquor, was by no means opposed to the clearing of the liquor by agitation, and I do not think that its presence gave rise to any error in the results.

2gr.·9595 of silver were precipated by 1.167 of chloride of silicium.

3gr.·685 of silver were precipitated by 1·454 of chloride of silicium.

For 1 gramme of silver, we have

I.... 0·394325 } chloride of silicium.
II.... 0·394570 }

Whence, for the equivalent of chloride of silicium—

I.	531·95
II.	532·28

and for silicium itself—

I. . . . . 88·75
II. . . . . 89·13

Mean . . . . . 88·94

PHOSPHORUS.

I operated with the protochloride $PhCl^2$; but I propose also to use the perchloride.

Well-dried chlorine was received over phosphorus. When the latter was dissolved, the disengagement of gas was stopped, and I put in contact with a great excess of finely divided phosphorus, the mixture represented by protochloride, and a small quantity of perchloride. After several days' contact, the decanted mixture was agitated with an amalgam of tin, distilled over this amalgam, and submitted to successive rectifications; at the end of a short time, the distilled liquid always precipitated nearly exactly similar quantities of silver; it was colorless and did not render distilled water turbid.

At the moment when the chloride of phosphorus was put in contact with an acid solution of nitrate of silver, it produced a brown precipitate which became gradually decolored and very soon affected the color of chloride of silver, slightly altered by light. Phosphorus acid, produced by the action of water on the chloride, is doubtless very rapidly changed into phosphoric acid; but the presence of this acid should not cause any fatal interference, for it is without action, not only on the acid nitrate, but also on the salt itself in the free state.

It requires on the average of 42·74 parts of protochloride of phosphorus to precipitate 100 parts of silver. These results have led to the number of 400·3 for the equivalent of phosphorus.

ARSENIC.

The chloride was distilled several times to free it from an excess of chlorine which it contained. It was colorless, and disappeared entirely in a great quantity of water. Its boiling point, during its rectification, did not seem to change perceptibly. The thermometer marked from 273 to 275°F.

These experiments gave:—

		Arsenic.
I.	2267·5—1329·6	=937·9
II.	2266·7—1329·6	=937·1
III.	2267·0—1329·6	=937·4

Mean of these experiments.

937·9
937·2
937·4
2812·4

$$\frac{2812{\cdot}4}{3} = 937{\cdot}5$$

Equivalent of arsenic = 937·50

The following table indicates the value of the different equivalents of which I have spoken. I have put them in comparison with those found in Berzelius' tables.

		According to Berzelius.
Sodium	287·17	290·90
Potassium	489·30	489·92
Nitrogen	175·00	177·03
Barium	858·03	856·88
Strontium	548·02	547·29
Silicium	88·94	92·43
Phosphorus	400·30	392·29
Arsenic	937·50	940·08

The most considerable changes in the numerical value of the above equivalents principally affect phosphorus and silicium. All the others, except nitrogen, differ very little from those which are current in science.

If we divide these numbers by 12·5, the equivalent of hydrogen, we arrive results, some of which are very much at variance with Prout's hypothesis. For others, on the contrary, such as nitrogen, phosphorus and arsenic, the quotients are entire numbers. It is especially the elements of organic matters which appear to be in the latter case. Is the hypothesis in question true as regards some bodies, and false as regards others? If it is difficult to decide in the case in which the numbers of analysis are confounded with those of theory, it must be owned that, by confining ourselves entirely to experiment, it seems that the question of equivalents being multiples of that of hydrogen (12·5) is answered in the negative.

NOTE ON THE ACID OF THE FIR.*

BY M. AUG. LAURENT.

FRESH experiments have proved to me that the natural acid of the fir is pymaric acid. This compound substance, either by age, or by the influence of certain agents, is transformed into two isomerical compound substances, the pinic and sylvic acids. It is wrong to assert, as it is laid down in treatises on chemistry, that sylvic acid crystallises in quadrilateral tables. Its form is extremely remarkable, and would of itself be sufficient to enable you to recognise this acid at the first glance: the crystals have the form of triangular tables. This error prevented my recognizing the identity of the sylvic and pyromaric acids. As to the pinic and amorphous pyromaric acids, they are probably identical. The pinic and sylvic

* *Comptes Rendus*, Oct. 1845.

acids that we meet with in the resins of commerce, have their origin in the modifications the natural or pymaric acid has undergone, through the effects of time, or by the influence of heat or light.

ON A DOUBLE CARBONATE OF POTASSA AND SODA.*

BY M. MARGUERITTE.

The salt which forms the subject of this paper was sent to M. Pelonze, by M. d'Heur, one of his former pupils; it was the result of several successive concentrations and crystallisations of prussiate of potash, prepared by the ordinary process, by decomposing animal matters by the potassa of commerce.

The principal characters of this combination might cause it, at first sight, to be regarded as a bicarbonate of potassa. Indeed, the reagents indicated the presence of potassa, the acid caused affervescence, and the crystals were well defined, and were not deliquescent.

Having, then, employed this salt in a preparation, after having submitted it to calcination, to convert it into neutral carbonate, I saw it fused in its waters of crystallisation on the first operation of heat; this single property, which bicarbonate of potassa does not possess, induced me to examine it further.

The solution of this salt gave an abundant precipitate, with the salts of magnesia and lime; but, in the presence of the latter, it did not disengage, either with or without heat, the smallest quantity of carbonic acid; it was not, therefore, a bicarbonate. Although I obtained, by chloride of platinum, and tartaric acid, the characteristic precipitates of potassa, it could not be admitted that this was a neutral carbonate of this base, since the salt was not deliquescent.

Some slight traces of efflorescence, which the best defined crystals presented, led me to suspect the existence of soda. Exposed to the flame of the blow-pipe, a very intense yellow coloration was produced, a distinctive character, and, although this test, applied to commercial salts, is too delicate, I thought I should ascertain whether soda formed part of the combination, or whether it was only a minute quantity—an impurity.

Antimoniate of potassa, the reagent indicated by M. Frémy, gave an abundant precipitate; sulphuric acid produced a perceptible crystallisation of sulphate of soda; it therefore became evident that the soda was a constituent part of the combination.

After having proved the absence of sulphate and chloride, and other acids than carbonic acid, it was necessary to determine in what proportions the carbonates of potassa and soda were united. For this purpose, I followed the two processes of M. Gay-Lussac, the analysis by abatement of temperature, and alkalimetry.

By strictly observing the indicated precautions in the mode of operating, 50 grammes of the salt previously converted into chloride, and dried, produced, on being dissolved in 200 grammes of water, an abatement of temperature of—

I. 5·75 which corresponds to 40·5 of Po Cl and 59·5 of So Cl.

II. 5·60 which corresponds to 38·9 of Po Cl, and 61·1 of So Cl.

A mixture of 2 equivalents of chloride of sodium, and 1 of chloride of potassium would have given, in the same circumstances, 38·8 per cent. of chloride of potassium.

Alkalimetry confirmed this composition: for 4gr.·807 of this salt always required, for neutralisation, from 80 to 81½ cubic centimetres of the normal liquor of sulphuric acid, and the mean of several experiments was about 80·5.

4·807 of dried carbonate, supposing them to be formed of 2 SoO CO^2 + PoO CO^2, contain—

1·8945 of PoO CO^2 which are neutralised by 1·343 } of SO^3, HO.
2·9125 of SoO CO^2 " 2·686 }

4·8070 4·029

Now, the mean of the numbers found by experiment, 80·5, corresponds to 4·025 of acid employed, a result which is confounded, so to speak, with that which theory indicates.

The anhydrous, therefore, contains 2 SoO CO^2, PoO CO^2; a simple calcination indicated the quantity of water contained in the hydrated salt, that is to say, 48 per cent., or 18 equivalents.

Whence the formula—

2 SoO CO^2, PoO CO^2, 18HO.

After having made the analysis of this combination, it was natural to investigate the causes which had produced it, and to ascertain whether it was only an accident of manufacture, or whether its formation was

* *Comptes Rendus.*

not regulated by the ordinary rules of chemistry.

The crude salt having been dissolved in order to purify it, the first crystals which were deposited contained, it is true, potassa; but the quantity of water of crystallisation had augmented; the alkalimetry of the dried salt marked a greater number of degrees; analysis, by cold, indicated a less abatement of temperature; in fact, every thing indicated a greater proportion of carbonate of soda.

These same crystals, dissolved and crystallised a second time, gave, by calcination, 62 per cent. of water, and contained only insignificant traces of potassa.

Not being able to admit that the primitive salt, which had given satisfactory results by analysis, was impregnated with so great a quantity of carbonate of soda, I must think that these two carbonates are separated by water; so that the double salt is destroyed, for one part, into carbonate of soda, which crystallised, and into carbonate of potassa, which constitutes the medium of stability for the other part of the combination.

The salt was then dissolved directly in a liquor charged with carbonate of potassa, and it crystallised without having undergone decomposition, for it gave results perfectly concordant with those which I had obtained the first time; and, which is a thing worthy of remark, the combination was always maintained in the proportions of 2 SoO CO^2, PoO CO^2, notwithstanding the presence of a great excess of carbonate of potassa.

It is not, however, indispensable, to avoid the destruction of the salt, to dissolve it in water charged with carbonate of potassa; for in very concentrated and syrupy liquors, it can crystallise without undergoing any very sensible alteration; only, in the first case, we have the certainty that carbonate of soda does not exist in the free state.

It may also be prepared directly by dissolving carbonate of soda in an excess of carbonate of potassa, and the crystals thus obtained represent the double combination, which has a composition identical with that of the primitive salt.

The circumstances in which this double carbonate is produced must be numerous, in certain manufactures, and especially in the potashes of commerce, which are often adulterated with carbonate of soda. For example, the salts of potassa and soda resulting from the incineration of plants must frequently be mixed with the new combination. It follows, then, from the results of which I have been speaking, that the separation of carbonate of soda from carbonate of potassa, does not rest, as hitherto supposed, on their difference of solubility.

This salt is of very great solubility with and without heat; it gives crystals of remarkable beauty, which have the property of retaining a great quantity of water of interposition, which fuse in their water of crystallisation at about 104°F.; they effloresce instantaneously *in vacuò*; but in the open air, there appears to be no efflorescence, and, in all cases, it is extremely slow. In proportion as the respective properties of the two carbonates are found compensated, so to speak, and in a state of equilibrium in the double combination—a very unstable equilibrium, which a slight diminution of pressure rapidly destroys.

Without wishing to consider this fact as the true connection which unites the two carbonates, we may make this approximation:—

$2SoO\ CO^2, PoO\ CO^2, 18HO = 2SoO\ CO^2\ 8\ HO, PoO\ CO^2\ 2H$;

that is to say, that the double combination represented as formed of 2 equivalents of carbonate of soda, with 8 equivalents of water, and 1 equivalent of carbonate of potassa, with 2 equivalents.

It is known that $SoO\ CO^2\ 8HO$ becomes, in dissolving, $SoO\ CO^2\ 10HO$: now, in presence of an excess of carbonate of potassa, the carbonate of soda may undergo a dishydratation, and, not finding its ordinary quantity of water, satisfy its affinity of crystallisation, by combining with the carbonate of potassa.

The decomposition of the salt by water would also be very easily explained; for, in the dilute liquors, and in the absence of free carbonate of potassa, that of soda may resume its ordinary 10HO, crystallised, and abandon in the liquor a certain quantity of carbonate of potassa, which thus maintained the stability of the rest of the combination.

From this view, it must be admitted that in very dilute liquors, the two carbonates are only in atomic mixture, and that it is only under the influence of evaporation that they enter into combination, except the quantity of carbonate of soda which may crystallise with its 10HO, in the state of combination in which the solution is found.

ON ANIMAL HEAT.*

BY M. GRASSI.

A WARM-BLOODED animal always maintains an almost equable degree of heat, notwithstanding the variations of temperature in the

* *Journal de Pharmacie*, Oct., 1845.

circumambient air; that is to say, the losses it experiences by radiation and contact with a medium colder than itself, are compensated by the production of internal heat.

It is to the phenomenon of respiration we must, for the chief part, attribute the greatest portion of the heat thus produced. But is respiration the only source of animal heat? We think not; but before we give our reasons for adopting this opinion, let us sum up, in a few words, our knowledge on this important question.

Messrs. Dulong and Despretz have compared the heat given out by an animal to that which arises from the combustion of the carbon and hydrogen consumed in respiration during the same interval of time.

These two philosophers came to the conclusion that the heat produced by respiration is less than that which an animal can give out during the same time.

We find an opposite result by founding the calculation on the figures given at a later period by Dulong, for the combustion of carbon and hydrogen.

In fact, according to these numbers, an animal, by experiment, gives out, during a stated period, less heat than that produced by the carbon and hydrogen consumed during respiration. This result has been explained in a very ingenious manner; it is said that an animal in a *state of repose* loses less heat than it developes, this excess is latent, to be disengaged during muscular action. Liebig arrives at a similar conclusion; he says, in a note published in the *Journal de Pharmacie*, July, 1845. "The number of degrees of heat an animal gives out to surrounding bodies, agrees exactly with that which the same apparatus would receive, if we converted, within it, by means of combustion, a quantity of oxygen, corresponding with the carbonic acid, found, and the water formed into an equal quantity of carbonic acid and water."

To arrive at this conclusion, M. Liebig repeated the calculations of Messrs. Dulong and Despretz; adapting the number 34792 as the calorific coefficient of hydrogen, and 18558 for that of carbon, which is higher than that given by Dulong.

This number, 8558, is deduced from the calorific coefficient of bicarbonated hydrogen, by deducting, from the latter, the number of units of heat attached to the combustion of the hydrogen contained in the carbonated hydrogen.

To deduce, in this manner, the calorific coefficient of a simple body, from that of a compound body of which it forms a portion, is to suppose that a compound body gives out, during combustion, the same quantity of heat as if its component were burnt separately.

The experiments that have been made, up to the present day, prove the contrary.

Be it how it may, the question of animal heat does not appear to me to be in a sufficiently advanced state to enable us to lay down the propositions I have just related. In fact, a compound body in combustion does not produce the same quantity of heat, as the elements of which it is composed do, when burnt separately; and we ought not to calculate the heat produced by respiration by adding together the quantities of heat produced by the combustion of carbon and hydrogen. For the substances that are consumed in the animal, are not there in a state of liberty; but in connection with combinations containing oxygen and nitrogen. We may, in fact, admit that a portion of the nitrogenised principles of the organism undergoes a perfect combustion, so as to give out carbonic acid, water, and nitrogen. For the experiments of Messrs. Dulong and Despretz, have proved the emission of nitrogen during respiration, and M. Boussingault has confirmed this fact, lately, by his experiments on the respiration of the tortoise. (*Annales de Chimie et de Physique*, vol. xi., page 440). The quantity of nitrogen in expiration, is, according to M. Boussingault, much less than that laid down by Messrs. Dulong and Despretz. The numbers given by these two philosophers are not admissible, as the learned professor of Giessen has clearly proved. It is necessary, therefore, to calculate the heat produced by respiration, to ascertain, in the first instance, the calorific coefficient bearing a relation to the substances consumed in the animal economy during the accomplishment of that function.

In addition to this, respiration is not, I imagine, the only chemical action to be taken into the calculation, when appreciating the causes of animal heat. Many chemical and physical actions, unknown to us, take place during digestion and assimilation; among them some give out heat, while others absorb it. These opposite actions may very possibly not compensate each other, and ought consequently to be placed, in this account, by the side of those that give birth to gaseous products (respiration), and which, alone, we have endeavoured to appreciate, up to the present time.

Thus, without admitting a special calorific power in animals, we think that respiration is not the only source of animal heat. Its effects are far greater than those of other chemical actions, but these last ought not to be looked upon as inoperative.

OBSERVATIONS ON THE AMMONIACAL OXIDE OF MERCURY.*

BY M. E. MILLON.

When we examine the formula by which we express the combinations of binoxide of mercury, and the double mercurial salts with ammonia, it is difficult not to suspect that, notwithstanding the recent researches of able men, chemical analysis has not discovered the nature of this order of compound bodies, with the certainty and precision that can be arrived at in the present day. These combinations, which in most instances furnish medicine with resources it has never misunderstood, present an extraordinary aspect in their chemical constitution, as well as in their characteristic reactions.

Have not those chemists who have busied themselves in the examination of these compound bodies declared that they meet with many of so stubborn a nature, as to resist the action of a concentrated solution of boiling potassa? As to the discordant formulæ which have been assigned to the greater number of the ammonio-mercurial products, they must appear strange, since the experiments of M. E. Reiset on the ammonio-protoxide of platinum has enabled us to represent nearly all the combinations of the metallic oxides with ammonia, as complex bases formed of 1 equivalent of metallic oxide, united with 1, 2, or 3 equivalents of ammonical gas.

I have endeavoured to discover the reason of the apparent anomalies, that effect these combinations, and I hope I have succeeded, in the essential points, in causing them to disappear entirely.

The principal facts of their composition, to which analysis has led me, are explained by the assistance of a single ammonio-metallic group. It is true it is of a novel nature, but its existence, once admitted, the most simple chemical rules are sufficient to explain all formulæ and reactions.

This group, although of a special nature, is fortunately not hypothetical, it is realised during the formation of ammonio-binoxide of mercury. Notwithstanding reiterated attempts we were ignorant of the composition of this curious substance, and the methods employed in its preparation produced it in extremely different states.

The delicate circumstances that present themselves to our notice during this study explain these variations, and the want of correctness.

The ammonio-binoxide of mercury was discovered by Messrs. Fourcroy and Thénard; it is obtained by pouring liquid ammonia on binoxide of mercury. If the mercurial oxide be of the yellow variety, the combination is immediate; if on the contrary you employ the red oxide, the combination is not complete until after three or four days contact. In either case the oxide becomes of a tolerably deep yellow color. It is only white in the proportion in which the caustic ammonia is carbonated; but then the ammonio-mercurial oxide is impure.

As soon as the combination is complete, it may be washed, the substance expressed, and preserved without alteration, provided it has been kept from the contact of the air; but if, instead of pressing it forcibly, it is immediately placed in an atmosphere dried by means of sulphuric acid, the yellow composition quickly deepens in color, and continues, in this manner, to change, until it becomes completely brown; the oxide, as it turns brown, becoming dishydrated; it then remains fixed in its constitution, and unchangeable by contact with the air.

The oxide becomes brown when heated, and loses more water at a temperature of from 212° to 266° without changing, in appearance or color.

The composition of the yellow oxide which has been determined by the proportions of mercury, water, and nitrogen, is expressed with great exactitude, by 4 equivalents of binoxide of mercury, 1 equivalent of ammoniacal gas, and 2 equivalents of water.

$$4Hg\,O + NH^3 + 2HO.$$

two equivalents of water are lost, when placed above the sulphuric acid, at which time the oxide becomes brown; and a heat equal to from 212° to 266° again removes 1 equivalent of water, with a few traces of ammonia but too feeble to be calculated as an equivalent.

So that to represent the constituent parts of this compound substance by an arrangement that lays down, up to a certain point, the modifications it undergoes while losing its water, and also in combining itself, we may arrange the elements as follows:—

Yellow ammonia oxide	$3Hg\,O + Hg\,Az\,H^2 + HO + 2HO$
Brown oxide formed above the sulphuric acid	$3Hg\,O + Hg\,Az\,H^2 + HO$
Brown oxide at 266°	$3Hg\,O + Hg\,Az\,H^2$.

I shall not enter into the many details which the study of this compound body offers to our notice, but at once give the chemical part it performs.

The combination formed by the union of 4 equivalents of oxide of mercury, to 1 equi-

*Comptes Rendus, Oct. 1845.

valent of ammonia, and 2 equivalents of water, is nothing but a base. It answers all the attempts made to prove its basic nature in the least equivocal way. It is an energetic base, that overcomes the strongest affinities; it separates ammonia from its salts, with as much facility as lime or baryta could. It is itself separated from its combinations by caustic alkalis; but it afterwards resists, without losing any of its elements, either the addition of soda or potassa, unless indeed the caustic ley be concentrated and kept boiling for some time.

It is this fixed nature of the ammonio-oxide of mercury, as an intimate combination of ammonia and binoxide of mercury, which has caused it to be believed that white precipitate and ammoniacal turbith did not yield to the decomposing power of caustic potassa. The ammoniacal salt of mercury does not, in reality, resist, but the potassa only separates the acid. The ammonio-mercurial oxide is simply eliminated in the state of a yellow hydrate. It needs a solution of potassa as thick as syrup, to decompose, partially, the hydrated base, and to drive off the ammonia. The brown anhydrous base is still more fixed, and remains unaltered in contact with the most concentrated potassa ley, at a boiling heat. Melted potassa decomposes it into nitrogen, metallic mercury, and binoxide of mercury.

I now come to the determination of the equivalent of this base; and in that, without contradiction, consists the most remarkable part of its history.

It combines directly with oxalic and sulphuric acids, whatever their degree of concentration, and it thus forms a well-defined oxalate and sulphate; it rapidly absorbs carbonate acid, if in the state of a hydrate, and its carbonate, which is very stable, is constant in its composition, and corresponds with that of the oxalate and sulphate; but these acids, whether feeble or energetic, even when employed in great excess, are always fixed in the proportion of 1 equivalent.

This complex oxide, formed of 4 equivalents of oxide of mercury, and 1 equivalent of ammonia, combines only with 1 equivalent of carbonic, oxalic or sulphuric acid. The saline combination takes place instantaneously, and the hydrated base separates carbonic acid from the atmosphere, and from carbonate of ammonia, in the same manner as lime and baryta. It is even almost impossible to obtain the base absolutely free from the carbonate, and this rapid absorption of carbonic acid has caused the ammoniacal oxide of mercury to be described as a whitish substance, a property which only belongs to the carbonate.

The constitution of salts formed directly by the ammonio-mercurial base, soon led me to observe that several compound bodies obtained by the action of ammonia on the mercurial salts, as, the bichloride, the biniodide, the sulphate, the nitrate, and the bromate, attach themselves in the simplest manner to the complex group I have studied.

The ammoniacal turbith differs in nothing from the salt obtained by the direct combination of sulphuric acid with the ammonio-mercurial base. M. Soubeiran has described a nitrate, which is exactly represented by 1 equivalent of nitric acid, and 1 equivalent of the same base. M. Rammelsberg has given an analysis of an ammonio-mercurial bromate, which agrees strictly with the preceding formulæ. Finally, we find among the ammoniacal products derived from bichloride and biniodide of mercury, combinations in which 1 equivalent of the oxygen of the new base, is replaced by 1 equivalent of chlorine and iodine.

By this means we are able to construct the following series, which enables us to judge of the simplicity and the extent of resemblance:—

Hydrated base	$3HgO + HgAzH^2 + HO + 2HO$.
Base dishydrated over sulphuric acid	$3HgO + HgAzH^2 + HO$.
Anhydrous base at 266°	$3HgO + HgAzH^2$.
Carbonate	$CO^2 + 3HgO, HgAzH^2 + HO$.
Carbonate at 275°	$CO^2 + 3HgO, HgAzH^2$.
Oxalate	$C^2O^3 + 3HgO, HgAzH^2$.
Sulphate	$SO^2 + 3HgO, HgAzH^2$.
Nitrate of M. Soubeiran	$AzO^5 + 3HgO, HgAzH^2, HO$.
Bromate obtained by M. Rammelsberg	$BrO^5, 3Hg, HgAzH^2$.
Chloride obtained by washing white precipitate in water	$2HgO, HgCl, HgAzH^2$.
Iodide obtained by M. Rammelsberg, by boiling ammonia several times with biniodide of mercury	$2HgO, HgI, HgAzH^2$.

This arrangement allows us, as we see, to arrange in regular order, the formulæ of the most anomalous of the ammonio-mercurial products. Among those that cannot be re-

presented, simply, by a salt of the new base, are some which may be considered as double salts, formed by an ammonio-mercurial salt, combined with an ammoniacal salt. We find, in fact, by direct experiment, that the binoxide of ammoniacal mercury, and the salts it forms, dissolve easily in many of the ammoniacal salts, and particularly in the sulphate, the nitrate, and the hydrochlorate. M. G. Mitscherlich has analysed the double nitrates thus formed, and their composition agrees very well with that of a double salt, a supposition, agreeing in other respects, and in every point, with the general affinities of ammonio-metallic salts.

Thus one of the double nitrates obtained by M. Mitscherlich has for its formula $NO^5 + 2\,HgO + AzH^3$. Doubling the composition, it is very well represented by—

$$\underbrace{AzO^5, AzH^3, HO}_{\text{Nitrate of ammonia.}} + \underbrace{AzO^5, 3HgO, HgAzH^3}_{\text{Nitrate of the new base.}}.$$

The other nitrate has for its formula—

$$3(AzO^5, AzH^3, HO) + 4HgO.$$

This formula, without any analogy, as yet, with the saline combinations, may very well be arranged thus—

$$\underbrace{2(AzO^5, AzH^3, HO)}_{\text{Nitrate of ammonia.}} + \underbrace{AzO^5, 3HgO, HgAzH^3, 2HO}_{\text{Nitrate of the base.}}.$$

The nitrate of the base in the preceding formula, certainly only contains 1 equivalent of water, the same as the nitrate of M. Soubeiran.

In actions on the sulphate of the binoxide of mercury, by means of caustic ammonia, in great excess, the whole of the salt is dissolved, and by evaporating it over lime in a close atmosphere, we obtain, after a time, large efflorescent prismatic crystals, which in an anhydrous state, are composed of—

$$\underbrace{SO^3, AzH^3, HO}_{\text{Sulphate of ammonia.}} + \underbrace{SO^3, 3\,HgO, HgAzH^3}_{\text{Sulphate of the base.}}.$$

All the ammonio-mercurial salts I acknowledge, cannot be placed in the systematic arrangement I have established; thus ammoniacal gas appears to combine simply either with 1 or 2 equivalents, whether in the bichloride or the biniodide: several other formulæ require a fresh inquiry; but I am in hopes I shall be enabled to deduce, in a natural manner, these formulæ from those we ought to assign both to the combinations of mercury and ammonio-metallic arrangements in general.

The system of combinations I have laid down, already constitutes a well-decided resemblance, it is also characterised, by a molecular arrangement of so curious a nature, that I thought I should be enabled to detach it from the general study I have entered into with respect to the combinations of mercury, and offer it at once to the judgment of the Academy.

AN EXAMINATION OF THE RESIN OF THE OLIVE TREE.*

We meet with in commerce, under the name of gum olive, a resinous substance which exudes from the wild olive tree, and is employed in fumigations, particular in Italy. In fact, this resin, when heated on a metal plate, gives out dense vapours, the odour of which is pleasant enough, and reminds one of that of gum benzoin, and oil of cloves. Pelletier discovered in this resin, a crystallisable substance, to which he gave the name of *olivile*, and which resembles in its properties, strongly oxigenated and non-nitrogenised substances, like salicine and rizine.

M. Sobrero, (in the *Annales der Chem. und Pharm.* liv, 67) has lately submitted it to examination. He extracted it in the following manner: the gum olive, having been reduced to a fine powder, was left to digest in ether for four-and-twenty hours, frequently renewing the liquid; the residuum was then almost completely dissolved in boiling alcohol of 36°, and the solution as it became cold, assumed the form of a crystalline mass, only a certain quantity of gummy matter and woody fragments remaining undissolved. The crystalline mass is first washed, when cold, in alcohol, and then crystallised in the same liquid, at a boiling heat.

The olivile thus obtained is white, without smell, and with a bitter and sweet taste at the same time. It melts with heat into a diaphanous mass, which concretes into

* *Journal de Pharmacie.*

an amorphous and strongly electric substance. A very exalted temperature chars it and drives off acid vapours.

It is soluble in alcohol, spirit of wood, ether, and water. Boiling alcohol appears to dissolve it in any quantity, ether dissolves but a small quantity: in water, it is deposited at a temperature of 158°F. in groups of prisms, in the form of stars, several millimitres in length. These crystals contain water of which they only lose a portion *in vacuó*. It melts in boiling water, and before it dissolves, it is not changed by boiling.

It does not act on vegetable colors, but ammonia and the fixed alkalis easily dissolve it, and by degress the solution changes color in the air, becoming brown. Weak acids produce no sensible variation, sulphuric acid converts it rapidly into oxalic acid.

Its solution rapidly attracts oxygen; it reduces chloride of gold and nitrate of silver. A solution of sulphate of copper becomes of a clear green color when it is boiled in it.

The crystals of olivile melt at 248°F.; when cooled they present an amorphous mass, fusible now at 158°. If this mass be dissolved in alcohol crystals are obtained, which are again fusible only at 248°.

Composition of Olivile.—We obtain anhydrous olivile, by allowing it to crystallise in pure alcohol, or better still by keeping crystallised olivile in fusion, in water, until it no longer diminishes in weight. M. Sobrero found it to contain in five combustions, 63·84—63·16 Hydrogen 6·64—7—09; he expresses these results by the formula C^{28} H^{36} O^{10}, which requires carbon 63·15, hydrogen 6·79. But M. Sobrero had obtained more carbon than this formula requires, and in two analyses also, less hydrogen, so that the preceding formula does not appear to me to be correct.

It is true that the composition of the two hydrates—

Crystallised olivile C^{28} H^{36} O^{10} + Aq.

Ditto dried *in vacuó* C^{28} H^{36} O^{10} + 2 Aq. seems to come to the assistance of M. Sobrero's formula.

The author made several attempts to discover the equivalent of olivile by precipitating it by means of ammonia, a mixture of an aqueous solution of olivile and nitrate, but the results obtained by this process could not be correct.

In the same manner he tried, unsuccessfully, to determine the nature and composition of the resinoïd or spongy substances which the concentrated acids produce with olivile; we need not notice these attempts. He was not more successful in the examination of the products of the oxidation of olivile, by means of the red peroxide of lead, and chromic acid.

The action of heat on olivile.—When olivile is heated on a platinum plate it is decomposed, and gives out white vapours which burn with a white and smoky flame.

If we heat dry and pure olivile in a retort it melts, then swells, and gives out water, and a peculiar oil which M. Sobrero calls *pyrolivilic* acid. The distillation is stopped when the residuum begins to blacken and increase in size.

The oily product is colorless, when pure, but it changes in the air; it possesses the smell and taste of oil of cloves. It dissolves completely in potassa, and the product blackens in the air and becomes opaque.

This oil cannot be dried over chloride of lime, and M. Sobrero proposes for the purpose of purifying it to dissolve it in ether, and submit the solution to distillation in a current of carbonic acid gas; the ether passes over first, and the oil which only boils at a temperature above 392°, passes afterwards. Two analyses of this oil gave carbon 70·16—69·82; hydrogen, 7—31—7·32: but there is nothing to show the correctness of these numbers; M. Sobrero again had recourse to superbasic acetate of lead to precipitate it, but the results offered nothing decisive.

The whole subject requires fresh examination; it is probable that the pyrolivilic acid of M. Sobrero belongs to a class of aldehyds or restricted bodies resembling the hydroguret of salicile or phenol. The numbers found by M. Sobrero do not appear to me to indicate the constituents C^{9} H^{10} O^{2} for a perfectly pure substance.

The author however proposes to return shortly to the subject.

EXPERIMENTS ON VALERIC ACID.*

BY M. G. CHAUCEL.

THE experiments I published on the products of the dry distillation of butyric acid, demonstrated the existence of a new compound substance, butyric aldehyd, under circumstances in which it could not be explained with the ideas we then possessed on the constitution of pyrogenous products. It was while I was endeavouring to ascertain the formation of that substance, that I was led to the discovery of certain relations between the aldehyds and the acetones, and was enabled to promulgate a general theory on the nature of these compounds. Since that time, I wished to satisfy myself whether other acids distilled in the presence of bases

* *Comptes Rendus*, Oct. 1845.

would not also give their aldehyd, or whether the formation of butyral ought to be considered as an exception, and not as a general effect common to the different monobasic acids analogous to acetic acid.

Having had at my disposition a certain quantity of valerate of baryta, I submitted the salt to the process of dry distillation, for the purpose of discovering the presence of valeric aldehyd in the valerone. From the first I could not verify the assertion of M. Lœwig, to whom we are indebted for the first experiments on the dry distillation of valeric acid; I cannot explain to myself how this chemist was able to obtain pure valerone, when, during the whole progress of the distillation considerable quantities of carburetted gas is disengaged, which certainly could not arise from the decomposition of the valerone itself.

The results which are the object of this paper, will show, I hope, the true nature of the products of the dry distillation of valeric acid: we may convince ourselves, that these products are far from being pure valerone, and that they ought to be considered as being composed of valeric aldehyd, scarcely adulterated by a few traces of valerone.

DRY DISTILLATION OF VALERATE OF BARYTA.

The salt on which the following experiments were made was formed from an acid directly extracted from the valerian. It is white, soft to the touch, confused by crystallisation in small shining transparent crystals, it contains 2 equivalents of water of crystallisation, corresponding to 9·5 per cent. Exposed to the contact of air, it effloresces and loses from 2 to 2·5 per cent of its water of crystallisation. This property was previously noticed by M. Chevreul, in the case of the phocenate of baryta. The valerate of baryta is completely soluble in water; the solution is readily effected, and it exhibits a slightly alkaline property, which no doubt is to be attributed to a certain proportion of free baryta contained in the salt; combustion with sulphuric acid, has, in fact, given a quantity of sulphate of baryta, which always yielded a slight excess of the base.

To satisfy myself of the purity of the combined acid that was to be used in my experiments, I converted a certain quantity of valerate of baryta into valerate of silver, this last salt, burned in contact with air, furnished the following results:

0gr. ·200 of valerate of silver gave 0gr. ·103 of pure metallic silver, a result quite conformable to the formula,

$$C^{10}, H^{9} + O^{4},$$

which expressing the composition of valerate of silver, we have

	By experiment.	Calculation.
Silver	51·5	51·7
Acid	48·5	48·3
	100·0	100·0

Almost up to the temperature of 662°F., valerate of baryta does not lose its water of crystallisation; and in other respects it remains unaltered: but above that temperature it begins to be decomposed, furnishing, as I have already had occasion to say, a continued disengagement of an inflammable gas, burning with a very bright flame, and at the same time we obtain a few drops of a slightly colored and highly scented liquid.

But at much more elevated temperature, a dull red heat, is necessary to effect a complete decomposition. I have, in fact, kept valerate of baryta at a heat nearly equal to 752°F. and the quantity of liquid products were almost inappreciable, it is only in an open fire, and at a dull red heat that I have succeeded in obtaining a tolerable quantity of liquid products; the residue contained in the retort, is in that case merely carbonate of baryta with an extremely small deposit of charcoal.

VALERAL.

Under this name I designate the new substance whose history is the principal object of this paper, and which is no other than the aldehyd of valeric acid.

To obtain it in a state of purity, we submit the unpurified liquid product arising from the dry distillation of valerate of baryta to several rectifications; by this means we succeed in separating a sufficiently appreciable quantity of a liquid, that boils at a temperature a little above 212°F. This liquid is the valeral or a perfectly pure valeric aldehyd.

The following are the properties that characterise this compound body:—

It is a limpid and colorless liquid of a very unstable nature; it boils at about 224°F., its density at 71° is 0·820, its taste fiery, its odour sharp and penetrating; it is insoluble in water, soluble in any quantity in alcohol, ether, and essential oils in general.

Valeral is very inflammable, and burns with a bright flame with a narrow blue edge.

Oxidating bodies in general transform valeral into valeric acid, and this oxidation takes place equally in contact with oxygen and with spongy platinum.

Nitric acid in the usual state of concentration, has a most energetic action on this substance, a nitrogenous compound is obtained heavier than water, and resembling in all points butyronitric acid; I shall call it, from analogy, *valeronitric acid*.

The production of this compound is accompanied by a disengagement of golden colored vapours of binoxide of nitrogen, and a colorless gas, readily inflammable and burning with a very bright flame.

I propose as soon as my occupations will allow me, to study perfectly this new class of nitrogenous products, the first example of which I have noticed in my experiments on butyric acid. This study promises to be interesting, for these new compounds appear to be a constant product of the action of nitric acid on the pyrogenous substances of monobasic acids, analogous to acetic acid (homologous series RO^2 of M. Gerhardt). The action of nitric acid on metacetone has furnished me, in fact, with a compound body perfectly analogous, which I shall call *metacetonitric acid*, the salts of which have so great a resemblance to the butyronitrates, that it would be easy to confound them with these last.

Composition of Valeral.—Valeric acid having for its formula

$$C^{10}\,H^{1} + {}^{6}O^{1};$$

that of valeric aldehyd ought to be

$$C^{10}\,H + {}^{10}O^{2}.$$

This, in fact, is the result to which the analysis of the subtance whose properties I have described leads.

I. 0gr.·306 of matter, have given 0gr. ·320 of water, and 0gr.·773 of carbonic acid. In this case the combustion was not completed by means of a current of oxygen.

II. 0gr.·358 of the same sample, gave 0gr.·385 of water, and 0gr.·916 of carbonic acid, (the combustion was completed by a current of oxygen.)

III. 0gr.·321 of valeral (a residuum of the density of vapour) gave 0gr.·347 water, and 0gr.·819 carbonic acid.

IV. 0gr.·284 of another sample, gave 0gr. ·303 water, and 0gr.·726 carbonic acid.

The results reduced into hundredths, produce the following numbers.

	By calculation.	I.	II.	III.	IV.
C^{10}....750	69·8	69·0	69·8	62·5	69·7
H^{10}....125	11·6	11·6	11·8	11·9	11·8
O^{2}....200	18·6	19·4	18·4	18·6	18·5
1075	100·0	100·0	100·0	100·0	100·0

The density of the vapour has confirmed the correctness of the above formula. One case among many others has produced the following results:—

Excess of weight of the balloon full of vapour, over the same balloon full of air	0gr·282;
Temperature of the balance ..	70°;
Barometric pressure	0m.·751;
Temperature of the oil bath....	365°;
Capacity of the balloon	278cc.;
Residue of air	2cc.·5;

We deduce the calculation—

For the weight of a litre of vapour 3gr.·801 density, in comparison with that of air 2·93. This gives—

$$\frac{C^{10}\,H^{10} \times O^{2}}{4} = 2{\cdot}96.$$

To ascertain the degree of purity of the crude valeral, I burnt a portion of the liquid products, simply dried by remaining in contact for four and twenty hours with melted chloride of calcium, but without undergoing any rectification. The following were the results obtained:—

0gr.·355 of the substance. gave 0gr.·373 of water, and 3gr.·921 of carbonic acid.

These numbers reduced into hundredths, give—

Carbon	70·7
Hydrogen..	11·7
Oxygen	17·6
	100·0

A composition differing but little from pure valeric aldehyd; the valerone, on the contrary, requires much more carbon and hydrogen, its formula being

$$\left.\begin{matrix} C^{10}\,H^{10} + O^{2} \\ C^{8}\,H^{8} \end{matrix}\right\} = C^{18}\,H^{18} + O^{2}, \text{ or centrerimally } \begin{cases} C = 76{\cdot}0 \\ H = 12{\cdot}7 \\ O = 11{\cdot}3 \end{cases}$$

If we admit that the crude product consists solely of valeric aldehyd and valerone, it will appear to be a mixture of about 90 per cent. of the first of these substances, and 10 per cent. only, of the second.

Formation of Valeral.—The facts I have

just had the honor of laying before the Academy have justified my theory of the acetones, to which I was led by previous experiments on the butyric series. This theory explains in a most satisfactory manner the formation of valeral. During the dry distillation of valeric acid, or of the valerates, 2 equivalents of valerate take part in the reaction, and we have

$$(C^{10}\,H^{10}+O^4)-O^2 \;\ldots\;\ldots\;\ldots = C^{10}\,H^{10}+O^2, \text{ valeral}$$
$$[(C^{10}\,H^{10}+O^4)+O^2]-C^2\,O^4-H^2\,O^2 = C^8\,H^8 \text{ butyrin.}$$

Again, valerone is represented by the union of the two simple compounds.

$$\underbrace{C^{18}\,H^{18}+O^2}_{\text{Valerone.}} = \begin{cases} C^{10}\,H^{10}+O^2 \\ C^8\,H^8. \end{cases}$$

Can the gas, which is disengaged abundantly during the whole course of the dry distillation of valerate of baryta, be merely butyrin? It has not been possible for me completely to resolve the question, but I intend to return to the subject at some future time.

Acetic acid yields, by dry distillation, so to express oneself, only acetone, butyric acid, and almost equal proportions of butyral and butyrone; while valeric acid gives a product which consists almost entirely of valeral. Here are so many facts beyond our calculation, but of which it is still easy to take advantage. The acetones which, after my theory, are combinations of aldehyds and simple hydrocarburets, are only formed at very high temperatures: if we represent the acids of the homologous series, RO^2 of M. Gerhardt, by the general formula

$$\Delta n + O^4,$$

we see that the equivalent of an acetone whose formula shall be

$$\begin{cases} \Delta n + O^2 \\ \Delta n - 1 \end{cases}$$

is increased considerably by a very feeble augmentation of the coefficient n; and we perceive afterwards, that substances as complex as valerone $= C^{18}\,H^{18}+O^2$ cannot resist the elevated temperature which enforces their formation: thus, when $n=8$ or 10, the acetones have but an ephemeral existence; during their decomposition, they give birth to the aldehyd of the acid, and the complementary molecule $\Delta n - 1$ is set at liberty. I could have wished to have carried my researches to a greater extent, to have studied completely the metamorphoses of valeral under the influence of various agents, and to have compared the reactions of this substance with those of analogous bodies, whose fundamental properties are well established. The high price of valeric acid, and the small quantity I could obtain, have obliged me to postpone the project until a future day.

THE ACTION OF CONCENTRATED SULPHURIC ACID ON FECULA.*

BY M. KALINOWSKY.

We know that fecula combines with concentrated sulphuric acid and forms a mixed acid, which is decomposed by boiling, into glucose and sulphuric acid. M. Blondeau de Carolles had made several experiments on this acid, but his results were very incomplete, and left much to be desired: M. Kalinowsky of Moscow, has again taken up the subject, and procured results which appear to me to be susceptible of a clear interpretation.

He rubbed up a quantity of common starch with twice its weight of concentrated sulphuric acid, and thus obtained a semi-fluid liquid, which was diluted with water, and *immediately* saturated with carbonate of lime. He thus separated sulphate of lime, without the liquid becoming acid; he could not, however, evaporate the liquor to dryness, without decomposing it; consequently he precipitated by means of alcohol, the sulphoamidonate of lime, this last was then placed *in vacuo*. It could not be dried at 212°, but it swelled considerably without being decomposed.

M. Kalinowsky obtained by the analysis of this salt—

Carbon	34·36
Hydrogen	5·65
Sulphuric acid	16·83
Lime	5·70

He deduced no formula from this, but he pointed out the fact that these numbers agreed with those of M. Blondeau, with the exception of the carbon, of which the latter obtained only 29·48 per cent., probably on account of imperfect combustion.

Another analysis of the same product gave similar numbers.

M. Kalinowsky afterwards made a fresh preparation, by leaving the sulphuric acid in contact with the fecula of wheat, for twenty-four hours, before he saturated it. The liquid was evaporated by means of a sand-bath, then treated with alcohol, and the latter, as in the preceding case, precipitated the sulphoamidonate and became loaded with a notable quantity of glucose; the salt obtained

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was rather yellow. The author formed by an analysis the following numbers.

Carbon	34·94
Hydrogen	5·70
Sulphuric acid	16·40
Lime	5·20

Finally, in a third preparation, the fecula was left seven days in contact with the sulphuric acid. The salt produced was even of a deeper color than the preceding ; the analysis gave—

Carbon	33·78
Hydrogen	5·68
Sulphuric acid	13·00
Lime	4·75

It is evident, from this, that the products may vary according to the duration of the reaction, and as the salts are not crystallisable, it is difficult to obtain them uniform in their composition. On this account, M. Kalinowsky drew no conclusion from his analysis in favor of the composition of the sulphoamidonates; it appears to me, however, that, beginning with the composition of glucose, we may construct a formula that will agree well enough with the preceding result, 1 equivalent of glucose being represented in my notation by—

$$C^{12}\,H^{24}\,O^{12},$$

we should have for the salts obtained from the fœcula, and the sulphuric acid—

$$C^{12}\,(H^{23}\,M)\,O^{12},\,SO^{3}.$$

The sulphoamidonate of lime will then contain—

Carbon	33·6
Hydrogen	5·0
Sulphuric acid	17·4
Lime	6·1

We must remember that the physical state of the salt does not allow us to get rid, entirely, of the glucose, and this must necessarily increase the carbon and the hydrogen in the analysis.

REPORT ON M. WURTZ'S MEMOIR, ENTITLED, "INVESTIGATIONS CONCERNING THE ACIDS OF PHOSPHORUS."

BY M. DUMAS.

The Academy having charged MM. Pelouze, Regnault, and myself with the examination of a memoir by M. Wurtz, entitled, "*Investigations concerning the acids of Phosphorus*," we now lay before it our report.

When, under the inspiration of Lavoisier, modern chemistry took the new form under which it has rendered such important services to natural philosophy and to the useful arts, the classification of oxidised bodies became the very peculiar object of attention of the authors of the nomenclature that was adopted by the Academy.

It was ascertained that the same body might be combined in several proportions with oxygen, and that it constituted thus, sometimes several oxides—sometimes several acids.

Hence, the necessity of varying the nomenclature of these different bodies, formed by the same radical; hence, the names of phosphoric, hypophosphoric, phosphorous, hypophosphorous acids, by which the different acids of phosphorus are designated.

These names indicate, or are intended to indicate that these acids, formed of phosphorus and oxygen only, contain increasing quantities of oxygen, beginning with hypophosphorous acid, for the same quantity of phosphorus.

They lead us implicitly to regard as essentially different from each other the molecules of these several acids, since, accepting the present mode of calculating equivalents, they are found to contain :—

Phosphoric acid, 6 molecules, namely, 5 of oxygen and 1 of phosphorus; phosphorous acid, 4 molecules, namely, 3 of oxygen and 1 of phosphorus; hypophosphorous acid, 2 molecules, namely, 1 of oxygen and 1 of phosphorus.

In Lavoisier's theory, the three acids of phosphorus would contain, then, in their own molecule, elementary molecules, in number very different; the first would contain 6, the second 4, and the third only 2. According to this, no one could dream, at first sight, of uniting these different acids under one system, or molecular type.

M. Wurtz proposed, relying on his own experiments, to regard all the acids of phosphorus as containing the same number of molecules, as being all susceptible, consequently, of entering into one same chemical type formed of 6 elementary molecules.

Phosphoric acid would, therefore, always contain 1 molecule of phosphorus, and 5 of oxygen PO^{5}.

Phosphorous acid, would, contain 1 molecule of hydrogen in substitution for 1 molecule of oxygen, PHO^{4}.

Lastly, hypophosphorous acid would have lost 2 molecules of oxygen, and gained 2 molecules of hydrogen in its place, this would make it $PH^{3}\,O^{3}$.

Thus, instead of three types for the acids of phosphorus, we should only have the general type PO^{5}, modified by substitutions that would enable us to foresee the existence, not only of bodies already known, PHO^{4} and $PH^{3}\,O^{3}$, but also that of two bodies

which complete the series PH^3O^2 and PH^4O.

Chemists will easily perceive, by looking at these formulæ, that their separation takes place by the intervention of water.

Up to the present time, it has been imagined that the acids of phosphorus, being able to exist without water, were yet capable of combining with it.

M. Wurtz, on the contrary, declares that two of these acids only, exist in combination with the elements of water. He even places the water in their molecule, arranges it by its side, and by this means arrives at the remarkable analogies we have just established.

Thus, the question which M. Wurtz proposed to resolve, consists in ascertaining whether phosphorous and hypophosphorous acids are oxides of phosphorus capable of combining with water, the molecules of the oxide and water remaining distinct, or whether these molecules, confounding their elements, give birth to a peculiar, but more complex molecular system, a grave and delicate question which arises in every shape in the researches of modern chemistry.

Let us examine the proofs on which M. Wurtz relies as the foundation of his particular opinion, and set forward the facts which he considers decisive, after having pointed out the object he had in view.

If the hypophosphorous acid merely contains PO, as we have hitherto believed, it would be easy, says M. Wurtz, to isolate this body, or at least to discover it in pure and simple combination with a base.

Thus, up to the present time, dry hypophosphorous acid PO, is a purely hypothetical body; and it has never been isolated. Agreeing perfectly as to the fact, this argument has no great importance in our eyes. The body PO might exist in a free state, without authorising us to conclude that it is found in the same state as in hypophosphorous acid. But the most decisive fact is, that the analysis of fifteen different hypophosphites, by means of their base, have constantly exhibited to M. Wurtz a composition of such a nature, that, independently of the molecule PO, and that of the base, the salt always contained H^2, O^2.

Thus, in conformity with usage, so far as concerns the constitution and nature of the hypophosphites, it ought to be said that these salts always contain 3 compound molecules, namely, the molecule PO, representing the acid RO, the base, and 2 molecules of water, H^2, O^2.

M. Wurtz prefers considering them as being formed of one base, RO, and one acid, PH^2, O^1, or PH^3, O^4.

In other respects, the hypophosphites are well defined salts, they readily crystallise, they can be preserved without alteration, and, in one word, present every guarantee chemists are in the habit of wishing for, in analytical researches.

The following table contains a summary of all the analyses M. Wurtz made on the hypophosphites :—

Hypophosphite of potassa	PH^2O^2, KO.
„ ammonia	PH^3O^2, H^4AzO.
„ strontia	PH^2O^2, SrO.
„ barata crystallised in needles	PH^2O^2, BaO, + HO.
„ crystallised in tables	PH^2O^2, BaO.
„ lime	PH^2O^2, CaO.
„ magnesia (crystallised)	PH^2O^2, MgO + HO + 5Aq.
„ (dried at 212°)	PH^2O^2, MgO + HO.
„ (dried at 302°)	PH^2O^2, MgO.
„ manganese	PH^2O^2, Mn + HO.
„ manganese (dried at 302°)	PH^2O^2, MnO.
„ zinc (rhomboidral)	PH^2O^2, ZnO + HO.
„ „ (octahedral)	PH^2O^2, ZnO.
„ iron	PH^2O^2, FeO + 6HO.
„ cobalt	PH^2O^2, CoO + 6HO.
„ nickel	PH^2O^2, NiO + 6HO.
„ chrome	$2PH^2O^2$, Cr^2O^3 + 4HO.
„ „ (dried at 632°)	$2PH^2O^2$, Cr^2O^3.
„ copper	PH^2O^2, CuO.
„ lead	PH^2O^2, PbO.

Thus, all the hypophosphites, even those formed by isomorphous bases, by means of alumine, like the oxide of chrome, contain the elements of 2 equivalents of water, which we have added in the preceding table to the elements of the hypophosphorous acid, and which it is impossible to confound with the water of crystallisation. In fact, the

latter is easily removed at a tolerably high temperature, while it is impossible to separate the oxygen and hydrogen in question, in the form of water.

But if this acid contains hydrogen as one of its elements, we ought to be able to separate this gas under certain circumstances. This, in fact, is what experiment points out to us. If we allow alkalies to act upon hyposulphites, or even bodies endowed with much less energetic basic properties, such as subacetate of lead, recently precipitated, a disengagement of hydrogen gas is apparent even at ordinary temperatures, and it forms a phosphite. This shows, according to M. Wurtz, that the hypophosphorous acid possesses itself of a molecule of oxygen, borrowed from the water, and that it loses a molecule of its own hydrogen, PH^2O^3, becoming PHO^4.

In representing hypophosphorous acid by the formula, PH^3O^4, the illustration of this fact becomes more simple, for the bases are confined to the disengagement of H^2, the separation of which would be no doubt assisted by the stability of the phosphite, which would have a tendency to be formed. The hydrogen is also separated, under the influence of oxidising bodies; but in that case it may happen to be separated as a gas, as in the preceding instance, or it may combine with an excess of oxygen, to form water. If, for instance, we heat hypophosphorous acid with sulphate of copper, we may obtain, according to the circumstances under which we operate, hydroguret of copper, a disengagement of hydrogen, or, when an excess of oxide of copper is present, water, and a deposit of metallic copper, as noticed for the first time by M. Wurtz; this separation of hydrogen in a gaseous state, under the influence of an oxidising body, is, certainly, a fact worthy of notice, and it forms one of the principal arguments of which M. Wurtz can avail himself in support of his opinion.

It is, in fact, easily perceived, that the oxygen of the oxide of copper having added to the hypophosphorous acid PH^2O^2, the fourth atom of oxygen, which should make it PHO^4, the separation of an atom of hydrogen takes place, which cannot remain in combination with the new acid.

It is less easily understood, on the other hand, why the hypophosphorous acid, looked upon as a hydrate, PO, H^2O^2, merely because its superoxidation was begun by the oxide of copper, effects the decomposition of the water, and sets the hydrogen at liberty.

In the last place, if we represent the formula of hypophosphorous acid by PH^3O^4, the tendency of the oxide of copper to separate the hydrogen from it, for the purpose of forming water, is explained without difficulty, but it is not the same in the case of the formation of hydroguret of copper, and the separation of hydrogen.

There are, therefore, some details to study in these two facts, some important peculiarities to explain; they deserve all the attention of the author, who ought to subject them to serious and deep consideration.

The great analogy that exists between the hypophosphites induced M. Wurtz to extend his researches on phosphorous acid and its combinations. This acid has a double base, and contains, in the free state, 3 molecules of water, 2 of which belong to the bases, and the third must, according to his opinion, be looked upon as a constituent water, that is as forming a portion of the elements of the acid itself. In fact M. Wurtz has discovered this molecule of water in all the phosphites. These salts are not met with in an anhydrous state.

He analysed crystallised phosphorous acid, as well as that of a large number of the phosphites. Among these he particularly endeavoured to establish correctly the composition of acid phosphites, which, in general, are well-defined salts. The following is a summary of the results M. Wurtz obtained as to the composition of these salts:—

Phosphorous acid (crystallised) $PHO^4\ 2HO$.
Dry neutral phosphite of potash $PHO^4, 2K$.
Acid phosphite of potash .... $2PHO^4 \left\{ \begin{matrix} KO \\ HO \end{matrix} \right\} + PHO, 2HO$.
Phosphite of soda (crystallised) $PHO^4 2NaO + 10HO$.
Phosphite of soda (dry) .... $PHO^4, 2NaO$.
Acid phosphite of soda ..... $2PHO^4 \left\{ \begin{matrix} NaO \\ HO \end{matrix} \right\} + PHO^4, 2HO - Aq$.
Phosphite of ammonia ..... $PHO^4, 2H^4AzO, 2HO$.
Neutral phosphite of baryta .. $PHO^4, 2BaO + HO$.
Acid phosphite of baryta .... $PHO^4 \left\{ \begin{matrix} CaO \\ HO \end{matrix} \right\} + HO$.
Acid phosphate of lime .... $PHO^4 \left\{ \begin{matrix} BaO \\ HO \end{matrix} \right\} + HO$.
Phosphite of copper ..... $PHO^4, 2CuO + 5HO$.
Phosphite of lead ..... $PHO^4, 2PbO$
Basic phosphite of lead .... $PHO^4, 2PbO + PbO$.

We see that independently of the water of the base, and the water of crystallisation, which is disengaged at a more or less elevated temperature, the phosphites always contain 1 equivalent of water, of which it is impossible to deprive them. M. Wurtz, taking, at present, the same view of phosphorous acid as he published long since, in regard to hypophosphorous acid, asserts that this water forms part of the phosphorous acid itself, whose formula would then become, according to the theory of Lavoisier, PHO^4.

To establish this hypothesis on a firm basis, M. Wurtz has not contented himself with the arguments the analysis of the thirteen different phosphites might furnish him; he has endeavoured to form new and perhaps more decisive combinations of this acid. In causing chloride of phosphorus to act upon common alcohol, and on amylic alcohol, he obtained two acids, ethero-phosphorous acid, and amylo-phosphorous acid, corresponding to sulphovinic acid, and a neutral ether, amylo-phosphorous ether.

Without wishing to describe, in this place, the properties of all the bodies M. Wurtz has so carefully studied, we shall only observe that the numerous analyses he has made, established, in a decided manner, the fact that phosphorous acid preserves, in its etherial combinations, the molecule of water that is essential to its constitution.

Thus the formula and the capability of saturation are, of—

Ethero-phosphorous acid PHO^4, C^4H^5O, HO

Amylo-phosphorous acid $PHO^4, C^{10}H^{10}O, HO$

Amylo-phosphorous ether $PHO^4, 2C^{16}H^{11}O$,

plainly confirming the view taken of the subject by M. Wurtz.

It must be well understood there is no reason against uniting the bases in the formula of the acid; and thus, making the body PHO^3O^6, the acid phosphites become PH^2O^6; R, and the neutral phosphites PHO^6; R^2, in which case the phosphites no longer belong to the same type of the hypophosphites.

All these opinions are so independent of the existence or non-existence of oxide of phosphorus in a free state, that we may remove it from these formula, and nothing prevents their being admitted in this particular case, the rather because we are acquainted with the body, PO^3, in an isolated state, in the same manner as we know its corresponding chloride, PCl^3.

In the meantime, all the facts observed by M. Wurtz have a tendency to confirm him in the idea that arose in his mind from the commencement of his inquiries, and of which he has sought for a confirmation, or disproval, by means of a long and patient series of analyses. The possibility of passing from one acid of phosphorus to another by the addition of hydrogen to oxygen, or reciprocally, becomes, we must say along with him, extremely probable.

The same facts that he has observed in the acids of phosphorus, he also found in the chlorides of that substance. In fact, the bichloride of phosphorus corresponds, as we know, with anhydrous phosphoric acid. Thus it may give rise to two special compound bodies by the operation of water or sulphuretted hydrogen. In both these cases it loses chlorine, and takes in its place either sulphur or oxygen.

We have, therefore, in this manner, $P\,Ch^5$ for bichloride of phosphorus; $PS^2\,Ch^3$ for chloro-sulphuret of phosphorus; and lastly, $PO^2\,Ch^3$ for the oxychloride of phosphorus; peculiar substances which M. Wurtz obtained by adding water to bichloride of phosphorus. It is the compound liquid that has hitherto been looked upon as a hydrate of bichloride of phosphorus.

M. Wurtz has thus discovered a series of bodies already very complete, containing the same number of molecules, which they can derive from each other, and of such a nature as to be arranged, perhaps, under the same type; for instance:—

Perchloride of phosphorus ..	$Ch^5\,P.$
Oxychloride of phosphorus ..	$\left\{\begin{matrix} Ch^3\,P \\ O^2\,P. \end{matrix}\right.$
Sulpho-chloride of phosphorus	$\left\{\begin{matrix} Ch^3 \\ S^2\,P. \end{matrix}\right.$
Phosphoric acid.....	$O^5\,P.$
Phosphorous acid	$\left\{\begin{matrix} O^4\,P \\ H. \end{matrix}\right.$
Hypophosphorous acid.....	$\left\{\begin{matrix} O^2\,P \\ H. \end{matrix}\right.$

M. Wurtz observed, that if such simple analogies are altered in a slight degree by the necessary consequences attached to Lavoisier's theory, when properly interpreted—when we consider the acids of phosphorus according to the theory of Davy, it discloses other not less remarkable connexions between these acids, under this new form; in fact, if we represent by

$PH\,O^6$, phosphoric acid with one base,

$PH^2\,O^7$, phosphoric acid with two bases,

$PH^3\,O^8$, phosphoric acid with three bases,

it is very extraordinary that $PH^3\,O^4$, hypophosphorous acid, should possess one base like the first, and like it, contain 7 molecules, united to 1 molecule of phosphorus; and that $PH^3\,O^6$, phosphorous acid, should have two bases like the second, and, like it, contain 9 molecules, combined with 1 molecule of phosphorus.

We have thus two series converging towards

the body $P H^3 O^8$, as we may see by the following table:—

$PH^3 O^8$, an acid with three bases.
$PH^3 O^7$, $PH^3 O^6$, acids with two bases.
$PH O^6$, $PH^3 O^4$, acids with one base.

By subtracting 1 or 2 molecules of hydrogen, or 1 or 2 molecules of oxygen from the molecule $PH^3 O^8$, it will become bibasic or monobasic, which may be readily understood. But that which is most worthy of attention is, that the same molecule, by losing 2 to 4 molecules of oxygen, could undergo the same modification.

It is almost impossible not to see the equality that exists between the terms.

$$PO^7 + H^2 = \underset{H}{PO^6} + H^2,$$

$$PO^6 + H = \underset{H^2}{PO^4} + H;$$

in this we clearly see that the phosphorous and hypophosphorous acids contain hydrogen replacing the oxygen; and hydrogen capable of being replaced by a metal, that is to say, hydrogen in two distinct states.

These connexions with each other could be still further multiplied, but we shall leave to the author the task of disclosing them, and supporting them by the experiments he so successfully performs, in a road he has opened for himself, which promises him still more discoveries, and in which we cannot too strongly recommend him to persevere. The vote of the Academy will make it his duty to do so.

The facts observed by M. Wurtz have appeared to your Commission extremely worthy of the attention of chemists. Some of these facts are new; all are well observed and discussed: the opinion laid down by the author is very interesting. In consequence, it has the honor of proposing the insertion of his memoir in the *Recueil des Savans étrangers*.

The report was adopted.

EXAMINATION OF OIL OF AMBER.*

BY M. DŒPPING.

When we subject amber to dry distillation, it produces empyreumatic oils and resins, which offer no well determined chemical characters. If we submit these products again to rectification, we obtain a yellow oil, which has been already examined by Elsner, as well as by Messrs. Pelletier and Walter; their labours on this subject have led to nothing decisive. M. Dœpping has not been more fortunate. He confined himself to the task of showing that the rectified oil is plainly of the same composition as essence of turpentine, but that its boiling point is not constant, so that we cannot determine the density of its vapour. Treated with sulphuric acid the oil yields an oily carbonated hydrogen, which also shows a similar composition.

* *Journal de Pharmacie*, Oct. 1845.

ON THE DISCOLORATION OF FŒCES.

To the Editors of The Chemist.

Gentlemen—

A few months since you favored me by inserting a letter on the coloration of fœces during the internal use of trisintrate of bismuth by the action of sulphuretted hydrogen, or its compounds contained in the intestinal canal. I now beg to lay before your readers the result of some experiments, which will prove that sulphuretted hydrogen and ammonia are simultaneously disengaged during the decomposition of fœces in the intestines; and that calomel is liable to undergo considerable change by its presence, when given internally, which may account for its uncertain action in many diseases and constitutions. I obtained some fresh dark-colored unhealthy fœces from a person while laboring under severe colic pains in the bowels, which attacked him at an early hour every morning; during the day he complained of lethargy, great prostration of strength, accompanied with head-ache and constipation of the bowels. I put the fœces into a glass vessel, in the centre of which I placed a bottle having a large wide mouth, filled with a solution of acetate of lead, and luted a coon over the whole, and placed the vessel to the temperature of blood-heat; in a few hours the surface of the liquid was covered with a black metallic pellicle: I allowed it to remain in this temperature for two days, and then withdrew the inner bottle, and collected the sulphuret of lead thus obtained, which being well washed, and converted into sulphate of lead by nitric acid, weighed 2·2 grains. The excess of lead held in solution was precipitated with hydrosulphuric acid. The filtered liquid slightly acidulated with hydrochloric acid, was evaporated nearly to dryness, and introduced into a small retort with caustic potash, when ammonia distilled over resembling in smell common hartshorn. I treated another portion of fœces from the same person in the manner described, only substituting calomel, moistened with a few drops of water, instead of the solution of lead. On examination, after twenty-four hours, I found that considerable action had taken place, and *that* portion of the calomel in

contact with the water being entirely decomposed, and converted into the black protosulphuret of mercury, while the other part of the calomel which remained dry, was changed to a sulphur-yellow color. This phenomenon takes place, when sulphuretted hydrogen gas, or the dry fumes of hydrosulphate of ammonia are made to pass over calomel—the sulphur is deposited, but on adding water *immediately*, protosulphuret of mercury and hydrochloric acid are instantly formed. This change must therefore take place in the bowels whenever calomel is given internally; but in a greater or less degree, according to the quantity of sulphuretted-hydrogen present, and the ammonia which is set free, united with the hydrochloric acid, forming sal-ammoniac.

In hot climates, putrefaction of the fœces may propably take place more rapidly, and a larger quantity of sulphuretted-hydrogen and ammonia being generated, renders the practice of giving large dozes of calomel absolutely necessary; not only to act as a purgative, but for the purpose of decomposing in the hydrosulphate of ammonia, which I consider malignant fevers, and probably cholera may have their origin.

According to Liebig, gases are capable of permeating through an intestine, and all animal tissues, either living or dead. *Organic Chem.* p. 114. It is therefore probable that checked perspiration and constipation may cause absorption of the gas to take place, and thereby effecting a decomposition of the blood in a greater or less degree, precisely the same as when it is inhaled by the lungs.

Sulphuretted-hydrogen produces immediate death when injected into the veins, in the cavity of the chest, or even when it is kept for some time in contact with the sound skin. Nine quarts injected into the anus of a horse, killed it in one minute. *Christison*, new edit. The same author gives the following account of the effects of this gas in combination with ammonia arising from the pits of the Parisian necessaries.

"The symptoms in cases when the vapors are breathed in a state of concentration, are sudden weakness, and all the signs of ordinary asphyxia. The individual becomes suddenly weak and insensible; falls down, and either expires immediately, or if he is fortunate enough to be quickly extricated, he may revive in no long time, the belly remaining tense, and full for an hour or upwards, and recovery being preceded by vomiting and hawking of bloody froth. When the noxious emanations are less concentrated, several affections have been noticed, which may be reduced to two varieties, the one consisting of pure coma, the other of coma and tetanic convulsions. In the comatose form, the workman seems to fall gently asleep while at work, is roused with difficulty, and has no recollection afterwards of what passed before the accident. The convulsive form is sometimes preceded by noisy and restless delirium, sometimes by sudden faintness, heaving, or pains in the stomach, and pains in the arms, and almost always by difficult breathing, from weakness in the muscles of the chest. When the exposure has been too slight to cause serious mischief, the individual is affected with sickness, colic, imperfectly defined pains in the chest, and lethargy. When individuals have been incautiously exposed to the inhalations of the vapors proceeding from the bodies of persons who have been asphyxiated from the vapors of the fosses, are attacked with sickness, colic, and pains in the chest, and sometimes violent convulsive affections have originated." The latter affection appears to give some idea respecting the nature of contagion.

From the above account of the poisonous properties of sulphuretted hydrogen with ammonia, and the effect which it produced on myself when exposed for some time in an atmosphere slightly impregnated with the fumes of hydrosulphate of ammonia, I am led to believe that many diseases in this country proceed from the presence of this compound, arising from the decomposition of vegetable and animal matters; but owing, more generally, to the hydrosulphate of ammonia being generated in a condensed state in our intestines.

The symptoms which appear to me to indicate the gradual absorption of the gas from the fœces, is, lethargy (sulphuretted hydrogen being a narcotic) great prostration of strength, and sometimes nausea, head-ache, with pains of the bowels, stomach, and extremities. These symptoms are every-day occurrences in this country, and which frequently precede attacks of fevers, &c.

The most important remedy for removing hydrosulphate of ammonia from the bowels is calomel, owing to its chemical and mechanical action; blue pill possesses a similar chemical action, but not so energetic. Julap, scammony, and gamboge, are also efficacious remedies for evacuating the lower intestines, but they exercise no power in decomposing the poison.

In cases of typhus, small and repeated doses of calomel is employed with great success; the action may be owing to the calomel becoming absorbed, which prevents the absorption of the gas by mercurialising the system.

In cholera and dysentery, the fœces are in a high state of decomposition, and sulphuretted hydrogen and ammonia, must be disengaged in abundance; I therefore would suggest that trisnitrate of bismuth might be given with advantage, and used also as an injection, which would instantly absorb the sulphuretted hydrogen, and probably arrest the further progress of putrefaction.

The result of my experiments on the fœces was witnessed by Dr. Oke, one of the physicians of this town; and if you deem the foregoing worthy so large a space in your valuable journal, you will oblige me by its insertion.

I remain,
Gentlemen,
Your obedient servant,
H. Osborn.

Southampton, Nov. 3, 1845.

II. CHEMICAL MANUFACTURES AND AGRICULTURAL CHEMISTRY.

ON THE EMPLOYMENT OF SILVER INSTEAD OF THE AMALGAM OF TIN IN THE MANUFACTURE OF COMMON LOOKING-GLASSES.*

BY DOCTOR MEURER.

Professor Boettcher, of Eradefort, communicated to the meeting of naturalists, at Bremen, a process for substituting silver for the amalgam of tin in the manufacture of looking-glasses; and he presented to the meeting several mirrors prepared in that manner.

The process is extremely simple, and M. Meurer, after several failures, became at length constantly successful. He dissolved about 25 centigrammes of nitrate of silver in a solution of hydro chlorate of ammonia, in water, and he then added to the liquid another solution, composed of one drop of essential oil of cinnamon, two drops of oil of cloves, and from four to six grammes of highly rectified alcohol. This mixture soon becomes clouded; after which a precipitate is formed, which is separated either by decantation or filtration. The clear liquor is then poured over the glass, and in the space of a few hours, only, the surface is found to be covered with a firmly attached coating of silver.

At present, M. Meurer has only used watch glasses, because a bordering of wax, plaster of Paris, &c., was not sufficient to retain the liquid on plane glasses. Would not frames of hard wood or glass, cemented by means of mastic or caoutchouc answer the required purpose?

This process is simple, and when employed with all the necessary precautions, the cost is certainly less than when the amalgam of tin is used, and its influence on the health of the workmen is never injurious.

The coating of metal that covers the glass is much whiter than that arising from the amalgam of tin, the reflected image appears yellower, but more natural than in common looking-glasses, for the latter often give a bluish tinge to the reflected images of objects, to which it is true we have become accustomed, and consequently pay no attention.

By means of the above-named mixture, we can, according to our author, cover a surface of sixteen square inches, and still find a portion of the silver in the precipitate.

* *Archives der Pharmacie*, Jan., 1845.

THE EXAMINATION OF POTASH BY THE NATROMETER.*

BY M. PESIER.

The mode I propose to employ for the examination of potash, is based on the increase of density occasioned by sulphate of soda in a saturated solution of pure sulphate of potash, and the mode of appreciating it rests on the employment of a particular description of areometer, to which I have given the name of natrometer.

This principle is deduced from the following facts, which have been proved by experiment:—

1. That a saturated solution of neutral sulphate of potash is always of the same density, when it is formed at the same temperature.

2. That sulphate of soda increases the density of the solution, progressively, according to its quantity. This increase is the more

* *Journal de Pharmacie*, Oct., 1845.

sensible from the solubility of the sulphate of potash being much increased when the two salts are present.

It is to be understood that the results are the same, if, instead of taking them ready formed, they are ascertained by the decomposition of carbonates and chlorides by means of sulphuric acid.

I imagined, at first, that to obtain tolerable correctness, it was indispensable to combine the whole of the oxide of potash with one and the same acid, and that induced me to give the preference to sulphuric acid, whose power is known.

Further experiments taught me that the chloride of potash, when dissolved in a saturated solution of sulphate of potash, separated a part of that salt, and yet did not sensibly change the density of the liquid. As much as 50 per cent. must be introduced to produce an increase equal to that effected by 3 per cent. of soda!*

I then endeavoured to ascertain what took place, when soda was added to sulphate of potash, and I observed it rendered its density less than it would have been without its influence. Synthetical experiments have enabled me to explain this apparent anamoly: chlorine, in the presence of the sulphuric acid, of the potash and soda, acts upon the latter base; it is the chloride of sodium that modifies the solubility of sulphate of potash, and it increases it rather less than it does that of sulphate of soda; the instrument only indicates 0·25 of oxide of sodium, when as much as from 0·14 to 0·20 has been added in the state of chloride of potassium, with sulphate of potash in excess. If, under particular circumstances, there be but an error of one and a half, the small quantity of chloride the potash of commerce contains, authorises us, in most cases, to dispense with its elimination.

These considerations have induced me to describe the two processes, one of which I look upon, in the hands of chemists, as a method of analysing, quite as correct and more rapid, than those they practise. The other, which is but a simplification of the first, produces results more than sufficiently correct for the manufacturer.

THE PROCESS OF EXACT ANALYSIS.

The experiment consists of three distinct operations. 1. The transformation of all the salts into sulphates. 2. Neutralisation and saturation. 3. The calculation of the quantity of soda by means of the instrument. I shall now describe the details necessary for practice.

ON THE TRANSFORMATION OF THE SALTS INTO A SULPHATE.

I think it useless to enlarge on the mode of proceeding in selecting the sample to be experimented on. We know that in order that it should possess the average composition of the potash to be examined, it must be composed of a collection of small portions taken from different parts of the heap.

Fifty grammes of potash are weighed, then dissolved in as little water as possible, and then filtered; the vessel is washed several times with fresh water, which is thrown upon the filter to clear it of all trace of alkali. Most frequently you may dispense with the filtration when the potash is of the first quality, when it leaves but little residuum, and when extreme correctness is not required. It is only recommended for the purpose of separating the alumine and the calcareous salts, which might by their presence increase in a slight degree the density of the liquid.

The potash being dissolved, it is decomposed by sulphuric acid, and as it is necessary to add it in sufficient excess to remove the chlorohydric acid, all necessity for making trial of the necessary quantity is avoided by employing a small china measure, capable of containing enough for every case.

There is no need to fear that such a dose of acid would be at any time too powerful, for the chlorides are only the more readily decomposed, and the fusion thus becomes more easy. A great excess of sulphuric acid has no other inconvenience than that of requiring more carbonate of potash to neutralise it.

The liquid is afterwards placed in a porcelain capsule to be evaporated; when its consistence becomes a little thickened, it is necessary to stir it with a small glass rod to prevent any portion being thrown out, and the stirring is continued until, by increasing the fire violently for a few minutes, it quickly fuses. The capsule is then left to cool, taking care that it does not break.

Another method presented itself to my mind to produce the transformation of chloride into sulphate, it is to employ sulphate of silver. We obtain the same result, by its assistance, but with more difficulty and at a greater cost, and I thought it best to adhere to the method I have already described.

ON THE NEUTRALISATION AND SATURATION.

After the capsule has become cold, you

* This singular property in opposition to all recognised opinion, finds its application now in the laws laid down latterly by M. Balard.

dissolve the mass several times in hot water; that you may employ a small quantity of liquid (less than 300 cubic centimetres) all the liquids are placed together in bottles capable of containing 600 grammes, and the excess of the acid is perfectly neutralised by a concentrated solution of pure carbonate of potash.

This solution is easily prepared, and with great certainty, by burning cream of tartar (bitartrate of potash) in a cast-iron saucepan. When the carbonisation is complete, the product is made into a ley, and the filtered liquid should be employed immediately, it being more convenient than concentrating it by evaporation.

As we may expect, a considerably quantity of sulphate of potash is precipitated by the operation, and the process is stopped when a drop taken up on the end of a stick does not change the color of red and blue test-paper. If too much carbonate of potash is used, the error is of no further inconvenience, excepting that it obliges you to add more acid. It is as well in all cases, as soon as the point of neutralisation is nearly reached, to make use of weak solutions of the potash and acid.

The neutralisation being perfect, it is necessary to obtain a saturated solution of sulphate of potash, at the temperature of the atmosphere. This is soonest accomplished by plunging the vessel in pump-water, and frequently stirring it. A thermometer placed in the solution will point out when the solution is nearly the same as that of the air, and it is better to let it become rather lower (it may be raised a few degrees by the heat of the hand), and then the vessel is to be withdrawn from the water. In order that the saturation should be accomplished at the surrounding temperature, everything is left to itself for a few seconds, merely taking care to stir it from time to time, and to see that the thermometer always remains at the same degree.

This being accomplished, it is filtered in a standing test-glass, on which a circular mark indicates a capacity of 300 cubic centimetres. If the previous instructions have been attended to, it will be seen that there is not sufficient liquid to make up that volume. The quantity is made up by washing the deposit of sulphate of potash several times with a saturated solution of the same salt, which it is useful to have in reserve, and at hand, that it may be filtered immediately, without waiting while the saturation is being accomplished. Every one must see the necessity of making up a stated, although arbitary, volume of liquid; the same quantity of sulphate of soda could not equally increase the density of two different measures of the saline solution.

The appreciation of the Soda by the natrometer.—After having mixed the different portions of the filtered liquid, by agitating them together, nothing more is necessary than to plunge the areometer into it. That which I have constructed has two scales close to each other, one coloured red, which has on it the figures that mark the temperature it indicates for every degree of the centigrade thermometer, the dipping point in a solution of *pure* sulphate of potash, the other scale represents the quantity of soda (oxide of sodium) in hundredths.

The zeros of the two scales coincide. If the experiment is made 0°, the quantity of soda will be directly ascertained, but if we operate at + 25 centigrade, we know that at the point at which the instrument plunges into the solution of pure sulphate of potash, saturated at that temperature, it corresponds with 8 hundredths of soda; it is, however, necessary to place the zero of the scale of the soda at that point: it is easily reduced to this point by subtraction.

As the experiment has shown us that the degrees of the soda cannot be equal, that they are less as they indicate more of the alkali, the observation ought to be made on the red scale, considered as a measure of equal divisions. From the number found you deduct the degree of temperature at which the saturation was made, and the remainder is exactly the number of hundredths of soda. For instance, in an observation made at + 25°, if the level of the instrument is at 59, deducting 20 from 59, we see that with respect to 39, the potash contains 13 per cent. of oxide of sodium.

As the deposit of sulphate of potash may contain interposed sulphate of soda, it is proper, to avoid all error, to wash it with the saturated solution, and to make up a second time, the volume of 300 hundredths. You plunge the areometer into it, and add the minimum quantity of oxide it indicates to that found in the first instance.

To obtain a greater degreee of correctness in the analysis, you must take into account the amount of the fractions of a degree of the instrument, but they may be neglected without any great error, because three degrees of temperature only occupy a space about equal to one hundredth of soda.

Process for Commerce.—We have seen above the influence of the chloride on the results of the analysis of a sample of potash by the natrometer; but it is of no consequenee, in most cases, in an industrial point of view. Unless, therefore, there is proof that the proportion of soda is very great,

traders have no necessity to endeavour to separate it. The same may be done in reference to insoluble substances, for what they yield to diluted sulphuric acid are sufficiently insignificant.

The following is the mode of proceeding I recommend :—

Take the potash from different parts of the barrel, either in fragments or powder, mix the quantities so as to have an average sample of the product. Weigh 50 grammes of this, throw it into a strait necked flask, holding about 600 grammes, cover it with about 200 grammes of water, and shake it to assist the solution. Then add sulphuric acid; it will produce an effervescence, and a deposit of sulphate of potash. It is easily seen that if we employ pure soda, or very much adulterated potash, no sulphate of potash will be formed, as it is quite necessary to find an excess under the form of an easily pulverised deposit; if it is not produced, you must add it in a fine powder; that which has arisen from natrometrical operations is excellent for the purpose. When the gas is no longer disengaged, be cautious, and only add the acid, diluted in water, drop by drop, to arrive at the perfect neutralisation of the alkali. You will know this is accomplished when the liquid will no longer change the color of red or blue test-paper, if by chance you add too much acid, and the red paper becomes blue, you can rectify the mistake by adding a little of a weak solution of potash, until it is perfectly neutral.

As the reaction has raised the temperature, and it is necessary to reduce it to that of the atmosphere, to operate more rapidly, you plunge the vessel in spring-water, and shake it from time to time. You remove it when a thermometer placed in the solution indicates about the same degree as that of the air; if the temperature is lower, the heat of the hand will be sufficient to bring it to the required degree. After agitating it for a few minutes at the surrounding temperature, decant it, pour the liquid by degrees on a small filter (I use a quarter of a sheet of paper), placed over the test-glass that accompanies the instrument. To find the required level, and at the same time to separate the sulphate of soda, still mixed with the deposit, wash the latter several times with a saturated solution of sulphate of potash, which it is always necessary to have at hand.

As soon as the volume of 300 hundredths is complete, the stopper is removed, the orifice of the test-glass closed with the hand, and the whole shaken well to mix the different portions of liquid. This being done, you carefully take hold of the natrometer by its upper part, and gently plunge it in the solution. If the potash experimented on is pure, you will read on the red scale, below the level produced by capillary attraction on the circumference of the tube, the degree of temperature at which the experiment was made. If, on the contrary, it contains soda, you will find a few degrees above whose number, when compared with soda scale, which is contiguous, is changed on the opposite side into hundredths of soda. Example: A trial made at + 12° centigrade, gives a solution which marks 25° on the natrometer; that is 13° due to the foreign alkali; comparing these 13° on the soda scale, I learn, that the potash contained 4 per cent. of soda, or 4 kilogrammes of oxide of sodium in 100.

It has appeared more convenient to me to adopt this new mode of graduation, rather than form a new scale, and construct tables to which reference must be made for the correction of temperature; I even thought I perceived an advantage in the system I adopted: it is that every one can verify the correctness of the natrometers by the levels indicated for a saturated solution of pure sulphate of potash.

To discover the quantity of Soda in leys. —We frequently in the arts want to discover the quantity of alkali contained in a liquid; this is the case with the soap makers. When the caustic ley is made it is called a pipette of ley (50 cubic centimetres), deducting from the degree found the weight of the potash by hectolitre.

The employment of the natrometer is here, also, easily applicable to the discovery of the quantity of soda; the analysis occupies no more than fifteen minutes. To avoid the trouble of neutralising it you may make use of the liquid arising from the alkalometric proof. It is sufficient if it is supersaturated, add to it a few drops of potash, then an excess of sulphate of potash in fine powder; you stir it a few moments, and immediately filter it, because, in this instance, the temperature cannot vary much from that of the air; after having made the volume equal to 300 hundredths the natrometer is plunged into it, it correctly indicates in kilogrammes the oxide of sodium of the solution in hectolitres. It is easy to discover by tables how many alkalometric degrees correspond to the quantity discovered, and in consequence the distinguishing dose of potash. We shall learn, in addition, without calculation, by consulting a table which I have drawn up from the alkalometric notice of M. Gay-Lussac, the quantity of carbonate of the chloride and sulphate, corresponding to the proportion of oxide of sodium obtained.

After having described the process it is fit to explain what it promises. I selected from among my experiments a type of each kind, and the following is a table of the figures obtained from the analysis made at different temperatures, when decomposing the chlorides and when not decomposing them. The first column contains, in comparison with each result, the quantity of oxide of sodium it contains, whether it has been added, or determined by the perchlorate of baryta, by following the mode of operation I have already described. In these attempts we have kept an account of the fractions of a degree in the scale, and we always formed double the volume of 300 hundredths by washing the deposit and the filter with a saturated solution of sulphate of potash.

	Quantity of soda contained.	Soda found in 400 parts.	
		By decomposing the chloride. 1st experiment	Without eliminating the chloride. 2d experiment
Tuscany potash, obtained at Paris	*1·57	1·55	1·25
The same, analysed a second time	idem	1·55	1·50
Tuscany potash, obtained at Valenciennes	1·86	2·25	undetermined
,, pearlash	undetermined	0·5	0·5
American potash	*4·6	4·0	4·1
Potash from the Vorges	2·69	3·0	undetermined
Brine from burnt molasses	*14·16	14·75	..
Beetroot potash, from Cambray	undetermined	14·85	12·70†
The same, by a second analysis	..	14·7	12·75
Beetroot potash, purified by neutral salts and soda, manufactured at Valenciennes	..	5·25	5·25
The same, analysed in 35 minutes	..	..	5·25
By adding 50 per cent of dry carbonate of soda	29·28	a failure	28·8
The same experiment repeated	..	..	29·3
Ditto	..	..	28·5
By adding 52 per cent. of carbonate of soda, and 10 of chloride of sodium	35·76	34·8	undetermined
100 sulphate of soda, in two trials	43·8	a failure	43·8 and 44
100 chloride of potassium	0·0	0·0	

The figures speak for themselves; it is clear that the plan may be adopted, with equal success, for the determination of the total base of the soda of commerce.

Perhaps it may be objected that the soda discovered by the natrometer does not always come from the carbonate, that it has possibly been introduced into the potash in the state of a sulphate or a chloride. Instead of considering this defect as a defect (at any time easily corrected), I look upon it as a happy circumstance, because the process is capable of discovering all the frauds possible by means of the salts of soda. That by means of the carbonate, also, must be the most frequently employed by clever speculators. The neutral salts diminish the expense of potash when they are purchased by the quality, but there would, certainly, be no compensation by increase of weight. In addition to this, there is a consideration that must not be lost sight of, which once admitted, overturns this objection: it is, that be the form what it may, under which the soda is introduced, either as chloride or sulphate, in virtue of double decomposition it always appears as a titular alkali, as soon as the potash is dissolved, and transforms a proportional quantity of carbonate of potash into a salt of less value.

I shall not conclude without directing attention to the fact, that before we attack the honesty of a dealer, whose products have been discovered adulterated with soda, we ought to remember that every variety of potash contains it, and that certain indigenous kinds possess as much as 14 per cent.

* Determined by the bichloride.

† We know that the crude potash of beetroot contains from 20 to 22 per cent. of chloride, and this difference was taken into calculation.

III. PHARMACY, MATERIA MEDICA, THERAPEUTICS, &c.

To the Editors of THE CHEMIST.

GENTLEMEN—

"WITH regard to Mr. Squire's proposition, so to modify the formula as to equalise the doses of medicines of a similar class, he thought this would be impossible, because there were, in fact, no fixed doses of medicines, the quantities to be administered depending on the circumstance under which they are given.

"Dr. A. T. THOMSON."

In the *Pharmaceutical Journal (?)* several funny papers have appeared compiled by a Mr. Squire. In the November number of that journal, another has appeared, by the same author, remarkably funny. The object of such papers I know not; probably they are published to show the immense amount of leisure afforded by the business of a retail shop, and to announce to the world that he has "contrived an infusion pot."

I shall not notice the "much discrepancy in the formulæ of the several colleges" so familiar to every apprentice, and which present no difficulty to the legitimate chemist, notwithstanding "the increased, and daily increasing facilities for locomotion," but briefly offer a few remarks upon the *parvum in multo* of Mr. Squire.

Acidum sulphuricum dilutum, P.L. Sulphuric acid, a *fluid* ounce-and-a-half; distilled water, fourteen fluid ounces and-a-half; not as Mr. Squire has given it.

"A fluid *drachm* (dram ?) of the mixture of three measures of sulphuric acid, and thirty of water," does not "exactly satrate" (neutralise ?) "twenty-eight grains of crystallised carbonate of soda."

"Among the ethereal preparations," Mr. Squire objects to "the formula for the preparation oleum æthereum," that it "*is said* by some practical men to be impracticable." Dii vostram fidem! There are those who say the moon is made of green cheese.

"Spiritus ætheris nitrici." Mr. Squire complains that he has "never met with a specimen made according to the London process, that contained half as much *ether* as that made by the Edinburgh process. The process of the London college yields a product which varies considerably in strength, *the proportion of ether* present depending upon the *quantity of ingredients* operated upon, and the *rapidity* with which the distillation is conducted;" that is to say, precisely as with the "No. 4 cathartic or physic balls of J. Bell and Co." "Aquæ distillatæ." Mr. Squire refers to Mr. Phillips, and says, "Many years ago (Mr. R. Phillips), in his notes on the *Pharmacopœia*, pointed out the injurious effects of the spirit in causing the waters to become acid, the spirit being converted into ascetic acid."

In the *Pharmacopœia*, 1836, spirit is directed to be added; and, Mr. R. Phillips, in the advertisement to his translation (so called), states that "the Royal College of Physicians, having appointed a committee to revise their *Pharmacopœia*, it may, perhaps, be allowed me to state, *that I have*, at their request, either conducted or *inspected* the preparation of almost every medicine which has been introduced, and in many cases repeated the processes which the *Pharmacopœia* already contained."

Mr. Squire informs us that, "on referring to Mr. Phillips's list of doses, in his translation of the *Pharmacopœia*, I find that there are

12 tinctures, whose maximum dose is ʒi,
13 ditto ,, ,, ,, ʒii,
11 ditto ,, ,, ,, ʒiii."

Mr. Squire is most unhappy in his authority for the doses of medicine. Mr. R. Phillips, speaking concerning himself upon this head, in the advertisement to his translation (so called), says, "but not being a medical practitioner."

In a word, Mr. Squire, having referred to the "Notes on the *Pharmacopœia*, by Mr. R. Phillips," I beg most respectfully to conclude with a quotation from the "Remarks on the *editio altera* of the *Pharmacopœia Londinensis*, &c. by R. Phillips," which seems to have escaped Mr. Squire, "but for one who has no longer the fear of the birch before his eyes, it would have been better to have left them altogether to schoolboys."

I remain, most respectfully,
Gentlemen,
Your most obedient servant,
GEORGE WHIPPLE.

London, Nichol-square, Hackney-road,
Nov. 10, 1845.

ON THE BENEFIT ARISING FROM THE USE OF MERCURIAL OINTMENT FOR THE PREVENTION OF CICATRICES IN CASES OF SMALL POX.*

BY DR. GOBLIN.

Many plans, more or less ingenious, have been imagined, within these few years, to prevent the pustules in cases of small pox, leaving indelible marks behind them, on the skin of the face and hands. M. Serris was among the first to point out a method which he called *ectrotic*, which consists in opening the pustules before they arrive at maturity, and cauterizing them with nitrate of silver; but this plan is tedious and painful, and not quite efficacious. For this method, simple compression, by means of strappings of diachylon plaster was resorted to in the first instance, and afterwards Vigo plasters, *cum mercurio*. But these methods are troublesome, and applied with difficulty to the nose, the eyelids, or ears. At the *hôpital des enfans*, they have given a preference, for several years, to the employment of an ointment of the following composition.†

Mercurial ointment,	24	grammes.	
Yellow-wax, ..	10	,,	
Black pitch, ..	6	,,	

This ointment succeeds excellently well, as I have myself witnessed in as many as twenty instances. Mr. Goblin, a medical practitioner at Staines, who, probably, was not aware of the formula adopted at the *hôpital des enfans*, has just completed a task, in which he has endeavoured to demonstrate, that the mercurial ointment applied to the skin of those afflicted with the small-pox, before the pustules are developed, prevents their coming to a head, and entirely removes the danger of unsightly cicatrices.

The mercurial ointment may be very efficacious in the cases in question; but it is inconvenient in two ways, in the first place, it may cause salivation; the second, it melts too readily, and spreads itself over the garments of the patients to whom it is applied. The ointment of which I have just given the receipt, has the advantage of containing much less mercury, and consequently, of being less likely to cause salivation, and, being of a firmer consistency, it less easily melts, yet at the same time it is completely efficacious.

It will be as well to add, that it has no bad effect on the progress of the disorder, nor does it interfere with the favorable issue of the disease, in one word, there is no need to fear that the pock will be driven back upon the internal organs, by using it on this account. I have no fear in saying, that a medical man, who, at the first breaking out of the small-pox, neglects to resort to one of the above plans, will have committed a great fault, if his patient should be afterwards disfigured.

* *Journal de Pharmacie.*

†Formula of the *hôpital des enfans*, preferable to the mercurial ointment.

A METHOD OF MAKING PROTO-IODIDE OF IRON.*

BY M. KOP.

It is known that the protoiodide of iron, prepared in the usual way, cannot be obtained solid in a state of purity. The limpid and colorless solution first formed, acquires a portion of oxygen from the air, and a part of the iron is thus oxidised, while, at the same time, a portion of biniodide of iron is formed, so that the product consists of a mixture of iodide and biniodide of iron, with peroxide of iron. If this mixture be dissolved in water, the solution will be of a yellowish red color, more or less deep, according to the quanity of biniodide, or even of free iodine present; the solution will also be turbid on account of the peroxide of iron held in suspension.

The author recommends the following method of preparing pure iodide of iron:—Triturate four parts of iodine with two parts of water in a large dish; then add, at once, one part of iron filings in a state of fine division, and continue the triturition. In a few moments there will be manifested a considerable elevation of temperature, together with the disengagemnt of the vapor of iodine. Sometimes, especially if the temperature of the atmosphere be low, the heat developed from the mixture is insufficient to cause the disengagement of iodine vapor; but, in this case, it is only necessary slightly to heat the mixture, immediately after the addition of the iron filings. The mixture is at first liquid, but it soon becomes solid.

There exists, in the protoiodide of iron thus prepared, a small quantity of iron, which may be easily separated by a filter, when the salt is used in solution, and the filtered liquor will then be colorless, and free from any mixture of binodide, or of free iodine.

This preparation may be easily administered in the form of pills made with any proper excipient.

‡ *Schmidt's Jahrbuch*, and *Journal de Chimie et Médicale.*

THE EFFECT OF THE DETERIORATION OF THE NURSE'S MILK ON THE INFANT.*

BY M. GIRARD.

The following are two very remarkable instances that demonstrate how the microscope may be of service in practical medicine.

A young woman, possessing a good constitution, fifteen days after her delivery, chose a nurse for her infant. This woman's milk was fifteen days older than her nurse-child, but it was white, tolerably thick, and in fact had the appearance of possessing every requisite physical property. She herself was young, a brunette, and in excellent health, and the infant had every appearance of a good constitution.

After the lapse of a few days the child had no sleep, it suffered from a continual diarrhœa, and became visibly thinner; shortly afterwards repeated vomitings came on, and a confluent eruption of the skin (*muguet*) broke out. An active and continued treatment having had no effect on these disorders, Dr. Dufosse examined the nurse's milk, and the following were the results:—

The milk was neutral, of the usual consistance; and it became slightly viscous on the application of ammonia.

The globules were large, but instead of appearing brilliant, like pearls, they were dull, and of an opaline white.

The field of the microscope was covered with rounded particles which might be compared, in form, to a flattened strawberry, and offering all the characters assigned by microscopic observers to the corpuscles of colostrum.

After having learned these particulars, they obtained, the same evening, a fresh nurse, chosen from among several, after having ascertained that her milk had a satisfactory appearance under the microscope.

The infant had scarcely partaken of this new aliment for the space of forty hours, when the diarrhœa disappeared, the eruption became less extensive, and a few days afterwards, the re-establishment of the health of the child was so great, that it could hardly be recognised.

In a second case, the subject was an infant, two months old, who became afflicted with almost constant vomiting, was wasting day by day, and in addition to this, was subject to attacks of coma, that returned at uncertain intervals. The examination of the milk showed that it contained a considerable quantity of mucus. The mother being unwilling to trust her child to another nurse, they waited awhile. The vomitings having diminished, a new examination of the milk proved a decrease of the quantity of mucus; but soon afterwards they were redoubled: the mucus had reappeared in great abundance. There could be no further hesitation, the milk musk must be changed. Eight nurses were examined; the milk of seven contained mucus, granulous bodies, or exhibited some other diseased appearance. The eighth having perfectly pure milk was chosen, and gave suck. Two days afterwards, the vomitings and the other bad symptoms disappeared, and did not again return.

These facts, if the microscopic observations did not lead astray, not only show the danger of nursing upon bad milk, but they show how deceitful are the outward appearances of the nurse's milk, since that which looks the richest, and is the best colored, may possess qualities injurious to the child, and exhibit these appearances; in fact, so frequently, that out of eight nurses one only was found to possess it in a pure state. The practical consequence of all this is, that too much attention cannot be paid to the milk of the nurse, and that to act with safety it is prudent not to confine yourself to the look and taste alone.

* *Journal de Pharmacie.*

DECOCTION OF THE ROOTS OF THE BLACK CURRANT IN CHRONIC HÆMORRHAGE FROM THE UTERUS.*

BY M. COLOMBAT.

Dr. Meigs recommends the trial, in some perverse cases of metorrhagia, of a remedy that has often proved useful in his hands; and where the use of astringents is indicated, he would make use of it with no little confidence. The remedy in question is a decoction of the roots of the common black currant, ripe, and the roots of the dewberry. A handful of each of these articles is to be boiled in two quarts of water, and, after straining the liquor, the patient should take half a cupful occasionally for a dose. The draught is by no means disgusting, and Dr. M. ventures to say that the effect of the administration has often been a very sensible diminution of the discharge. He would deem such a formula very useful in country places, where every facility cannot be enjoyed in varying the scientific prescriptions repeatedly upon the failure of such as may have been tried without success.

* *Medical Gazette.*

ON THE ACTION OF OIL OF OLIVES ON ZINC, WITHOUT HEAT.

Symptons of poisoning exhibited themselves a short time back, at Beziers, in several individuals belonging to the same family; Dr. Lary successfully treated the vomiting and colics that manifested themselves. This physician attributed the accident to oil of olives, which had been used in the preparation of various dishes, and had been kept in a bottle made of zinc.

We know, according to the experiments of Vauquelin and Deyeux, that zinc is easily attacked by water-vinegar, the juices of lemon and sorel, hydrochlorate of ammonia, common salt, and butter.

M. Audouard made several experiments to ascertain the action of cold oil of olives upon zinc, and the result of these trials was, that zinc is really attacked by this substance. It is, therefore, imprudent, and even dangerous, to keep olive oil in zinc vessels. This oil, no doubt, like every other combustible oil when in contact with zinc, takes up, even when cold, a considerable quantity of the metal forming at the same time, oleates and margarates of zinc. These, it is true, are insoluble; but they may be easily decomposed, and converted into soluble salts, by the acids that are contained either in the stomach or in the food, in the preparation of which the oil is used.

ABORTION PRODUCED BY SULPHATE OF QUININE.

M. Petit Jean, a medical practitioner at Seurre Department of the Côte-d'Or, has frequently observed the influence possessed by sulphate of quinine in procuring abortion, in his part of the country, where intermittent fevers are frequent. He, has well as others, observed that the salts of quinine, in the dose of 1 gramme a day, in many instances caused abortion. On this account, this honorable practitioner was in the habit, when called upon to attend a pregnant woman, suffering with intermittent fever, to leave it to nature to cure the disease. He has found by following this practice, that the feverish action ceased of itself after a moderate time, and in these cases the pregnancy always ended favorably. It is difficult, in the present state of science, to explain, in a satisfactory manner, the action of sulphate of quinine on pregnant women. But the fact appears no less certain, and as such it ought to fix our attention. It is singular that in this case the remedy produces a serious evil, which the disease, when left to itself, does not bring about.

ANALYSIS OF FUSIBLE CALCULI.

Fusible calculi are formed of the compound phosphate of ammonia and magnesia, mixed with phosphate of lime. The analysis of the phosphates, when separate, is very simple; but the process becomes more complex when they are mixed. It has been commonly recommended to separate them, by digesting the fusible (or mixed) calculus finely powdered, in acetic acid, which dissolves the phosphate of ammonia and magnesia (triple calculus), and leaves the phosphate of lime (bone-earth). The fusible are very liable to be mistaken for the triple calculi.

In a late number of the *Lancet*, Liebig remarks that both species are similar in appearance; their surface is generally rough, and they have internally an earthy or chalky character, very rarely exhibiting a crystalline structure. When boiled with potash, ammonia is evolved from both, and both are easily dissolved by diluted nitric or muriatic acid.

Liebig recommends the following process for distinguishing this calculi:—Dissolve in a diluted mineral acid, render this feebly alkaline by ammonia, then add acetic acid to re-dissolve any precipitated phosphate. If chloride of iron be now added, and the liquid boiled, the whole of the phosphoric acid will be precipitated under the form of phosphate of iron. The lime and magnesia may be discovered in the filtered liquid in the usual way. If oxalate of ammonia should give a precipitate, this would indicate the presence of phosphate of lime in the calculus, and show that it belonged to the mixed or fusible variety.

Royal College of Chemistry.—This useful institution appears to be going on prosperously, and promises to be productive of considerable benefit to the application of chemistry in the arts and agriculture—in physiology, pathology, &c. We have only time for a brief notice this month, which prevents our enlarging on its merits as we could wish.

TO CORRESPONDENTS.

Vol. VI. will be ready in a few days.

Notice.—*All Communications and Books for Review must be addressed* "To the Publisher of The Chemist, 310, Strand, London." *Communications must be prepaid, and sent before the 15th of each month; Books for Review before the 10th.*

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